THE PERMANENCE OF CONTINENTS.

By J. STARKIE GARDNER, F.G.S.

'IT is not too much to say that every spot which is now dry land has been sea at some former period, and every part of the space now covered by the deepest ocean has been land.' This sentence occurs in the latest edition of Lyell's *Principles of Geology*, still perhaps the most authoritative text-book on the subject, and the view it expresses has been generally received as an article of faith by geologists until within a few years, or even months ago.

Lately a change of view has taken place, and now many distinguished men hold the completely opposite opinion that oceans have been permanent from the remotest times, and that continents are, and have ever been, fixed lands, subjected to ceaseless modifications of form. Among the most conspicuous partisans of the new theory are Sir Wyville Thomson, Prof. Geikie, and Mr. Wallace; and the latter especially seems to have collected together and presented in his fascinating book, *Island Life*, every kind of evidence that tends to support it. Nothing appears to have escaped him, yet the whole when summed up must seem to every geologist to fall far short of proof. Still, although the evidence upon which the theory is based is as yet wholly insufficient, it by no means follows that the theory itself is improbable.

The chief evidence upon which the *Permanence of Continents* at present rests, is purely geological. It is argued that the whole of the sedimentary rocks are littoral deposits, or those of inland seas; and if this can be maintained, the theory would, almost as a matter of course, be accepted. Mr. Wallace, therefore, endeavours by every means to prove it.

Chief among deposits hitherto supposed to be oceanic, is the Chalk; and to the discussion of this formation, accordingly, almost a whole chapter is devoted. Mr. Wallace expresses the belief that, far from the Chalk sea representing a wide ocean with a few scattered islands comparable to some parts of the Pacific, 'it formed as truly a portion of the great northern continent as it does now.'

The evidence, which he has to set aside, in favour of the Chalk being a truly oceanic deposit, is extremely weighty however, and by no means easily disposed of. Its vast extent—stretching from Sweden to Bordeaux, and from Ireland to China—and its freedom everywhere from impurities derived from the degradation of land, are greatly in favour of its oceanic origin. The areas that are known to have been denuded, and the enormous deposits of flint-shingles which characterize the Eocenes from their base upward to the most recent gravels, show how colossal this denudation has been.

The Chalk that has escaped seems but the fragment of a mass which once passed under the Atlantic, for even the Scilly Isles are strewn with flint, and the last remains of it in Devonshire and the north of Ireland are as pure as elsewhere, and show no signs whatever in the Chalk itself, towards its western boundaries, of the proximity of shores. This vast deposit abounds with *Globigerina*, of species identical with those of the modern Atlantic mud, and with coccoliths and discoliths. Representative siliceous Sponges are abundant in both, and the recent chalk-mud has yielded a large number of the group *Porifera vitrea*, which find their nearest representatives among the *Ventriculites* of the White Chalk. The Echinoderms of the deeper parts of the Atlantic basin are very characteristic, and yield an assemblage of forms which represent in a remarkable degree the corresponding group in the White Chalk. Species of the genus *Cidaris* are numerous; some remarkable flexible forms of the Diademidæ seem to approach the *Echinothuria*;* *Rhizocrinus* is closely allied to the chalk *Bourgueticrinus*, while even among fish the genus *Beryx*, so abundant in the Chalk, has been found by Dr. Carpenter, and the fresh light that the publication of the deep-sea fish of the *Challenger* Expedition is likely to throw on the subject will be looked forward to with much interest.

Prof. Duncan,† when investigating Corals, became impressed with the remarkable persistence of character and absence of variability in those of the deep-sea fauna. 'The dredging in 1095 fathoms off the coast of Portugal, which yielded *Pentacrinus Wyville-Thomsoni*, Jeffreys, produced many corals; and the series presented an eminently Cretaceous facies. The genus *Bathycyathus*, whose species, *Sowerbyi*, is so well known in the Upper Greensand, was represented there by numerous specimens of a species closely allied to that form.'

* Sir Wyville Thomson, *Nature*, vol. iii. p. 297.
† *Quart. Jour. Geol. Soc.* xxvii. p. 437.

A new species of *Caryophyllia*, allied by its structural peculiarities to *C. Bowerbanki* of the Gault, and a species identical with the well-known *Caryophyllia cylindracea*, Reuss, sp., were discovered at the same time. The homotaxis of part of the Coral fauna of the Atlantic and that of the Cretaceous ocean, Prof. Duncan considers to be very remarkable.

Against this well-nigh irresistible evidence in favour of the oceanic origin of Chalk, Mr. Wallace states that no specimen of Globigerina ooze yet examined agrees, even approximately, with Chalk in chemical composition. The differences between the few analyses that have been published, are chiefly in the relative quantities of carbonate of lime, silica, alumina, and oxide of iron. It is by no means apparent that Sir W. Thomson's sample is the nearest analogous deposit to Chalk that could be found in the beds of the Atlantic or Pacific; but supposing it to be so, the great changes in chemical composition to which Chalk has been subjected since its consolidation, are entirely overlooked in comparing the analyses.* Chalk is, and probably always has been since its upheaval, constantly saturated with percolating rain-water, which enters as soft water charged with carbonic acid, and comes out in springs of hard water charged with carbonate of lime; and this alone in the course of ages would carry away the more soluble constituents such as iron, alumina, and magnesia. An even more important change is due to the removal by segregation of its silica into the form of flint. This, doubtless, took place when the silica was in a colloid state, and seems to have been arrested, whilst the Chalk was consolidating, wherever harder and softer layers alternate. Its once viscid, almost fluid, state is shown by the manner in which it has penetrated the minutest pores of Echinoderms before destroying the shell; and it seems probable from the way in which it has replaced carbonate of lime,† that it had not parted with its

* The analyses relied upon in support of this are by Sir W. Thomson, of Globigerina ooze, viz.:—

Carbonate of Lime	43·93	to	79·17	per cent.
Carbonate of Magnesia	1·40	to	2·58	"
Alumina and Oxide of Iron	6·00?	to	32·98	"
Silica	4·60	to	11·33	"
Supposed Volcanic Dust	4·60	to	8·33	"

And of Chalk, by David Forbes:—

	Grey Chalk, Folkestone.	White Chalk, Shoreham.
Carbonate of Lime	94·09	98·40
Carbonate of Magnesia	0·31	0·08
Alumina and Phosphoric Acid	a trace	0·42
Chloride of Sodium	1·29	—
Insoluble débris	3·61	1·10

† All the carbonate-of-lime shells are replaced in the Blackdown deposits by silica.

organic acids. That it did not assume the solid state until at least after the partial consolidation of the Chalk is obvious, through the filling in of fissures at right angles to the bedding, which could not have existed when it formed the surface sediment of the ocean bottom. In comparing the White Chalk analysis with that of the ooze, therefore, we must bear in mind that, as already pointed out by Mr. Sorby, Mr. Sollas, and Dr. Wallich, a portion of flint must be added equal to that which has been separated away. In a similar manner, iron has been removed and segregated together, to be crystallized principally into globular balls with a radiating structure.*

The shells composed of carbonate of lime, such as those of Gastropods,† Cephalopods, and Dimyaria, seem also to have been dissolved away, perhaps by the rain-water which falls upon the Chalk, saturates it, and passes through it by capillary action unceasingly. Another evidence of change is shown in the crystalline condition of shells composed of phosphate of lime, such as the Aviculidæ, the Branchiopoda, the Echinodermata, &c.

It is surprising to find that no allusion whatever is made to this range of facts by Mr. Wallace; and those of his readers who are unacquainted with them, are left unaware that Chalk has undergone such great changes in its composition since it was the bed of the sea, as to deprive the unqualified statement that the analyses of Chalk and Globigerina ooze 'do not even approximately agree,' of any scientific value.

These facts further tend to show, as indeed is obvious from a comparison of the faunas, that the similarity in the analysis of the Oahu chalk and the White Chalk, upon which so much stress is laid, is purely superficial.‡ In spite of the fact

* It assumes very beautiful forms in the Grey Chalk, and has occasionally completely replaced Sponges. The iron is frequently ochreous in the White Chalk.

† Gastropods are found as casts in the Grey Chalk, slightly coated with iron, and occasionally traces are met in the Lower White Chalk in the same condition. Higher than this even the most indistinct outlines of the larger forms, such as *Pleurotomaria*, are rare. I have seen but one trace of shell on any spiral Gastropod, and this on a fragment of *Funis* from the White Chalk near Norwich. Small thin fragments adhered to the cast, and the circumstance is remarkable as *Funis* almost alone of the Gastropods preserves its shell in the Cambridge Greensand. The shells of Cephalopods seem to possess a slightly greater resisting power, and their casts are, as a rule, more distinct.

‡ Analysis of Oahu chalk:—

Carbonate of Lime	92·800
Carbonate of Magnesia	2·385
Alumina	0·250
Oxide of Iron	0·543
Silica	0·750
Phosphoric Acid and Fluorine	2·113
Water and loss	1·148

Geology of the U. S. Exploring Expedition, p. 150.

that 'this chalk consists simply of comminuted corals and shells of the reef,' and is, when examined microscopically, 'found to be destitute of the minute organisms abounding in the chalk of England,' Mr. Wallace states that in several growing reefs a similar formation of modern chalk, undistinguishable from the ancient, has been observed.

Mr. Wallace thus assumes that the Chalk is derived from excessively fine mud produced by the decomposition and denudation of coral reefs; but this view appears to me to be untenable. Mr. Murray expressly states that no *Globigerinæ* were found in any of the enclosed seas of the Pacific which possess this chalky bottom; and to account for *Globigerina* in the Chalk it has to be supposed that the Chalk sea was open to the Gulf Stream, *i.e.* the Atlantic. Further, to provide the necessary conditions we are obliged to suppose this vast sea to have been bordered with islands and coral reefs, and that no large rivers flowed into it, and yet absolutely no traces of these coral reefs remain, while an inland sea could hardly have existed in proximity to a great permanent continent without some rivers draining into it. A curious piece of reasoning is that in the Maestricht and Faxoe chalks 'we have a clear indication of the source whence the white calcareous mud was derived which forms the basis of the chalk.' If these local and far newer deposits are seen to be highly coralline and the Chalk is not seen to be so, we have rather a clear indication that they were not deposited under the same conditions. The presence of *Mosasaurus* in the Maestricht beds, and the far newer aspect of its fauna, show that it must have belonged to an altogether different period, probably the one represented in America by a great so-called Cretaceous series containing a mixture of Cretaceous and Tertiary mollusca, dicotyledonous plants, and *Mosasaurus*. From every point of view, in fact, the inference that the vast Cretaceous deposits are analogous to small local deposits of coral mud in the Pacific does not appear to be the true one.

With regard to the probable depth of the ocean which deposited the Chalk, the evidence brought together by Mr. Wallace is less unsatisfactory. Mr. J. Murray, for instance, sees the greatest resemblance to it in mud from depths of less than 1000 fathoms; and Dr. Gwyn Jeffreys finds that all the Mollusca of the Chalk are comparatively shallow-water forms. We must bear in mind, however, that the characteristically deep-sea families and genera, such as *Bulla* and the Solenoconchia, *Leda*, *Neæra*, and *Verticordia*, would have long since been dissolved away if present; while great and highly characteristic cretaceous genera, such as *Inoceramus* and *Hippurites*, are wholly extinct, and nothing therefore can be safely predicated concerning their habits.

In the Grey Chalk near Folkestone dark impressions of nearly all the deep-sea Mollusca enumerated above are abundant; and the Gault and a part of the Lower Greensand are full of their shells in perfect preservation. Their absence in England at least, from the Chalk, seems very clearly due rather to subsequent destruction than to their never having been present. Of the Chalk genera that are preserved, *Pecten, Amussium, Lima, Spondylus, Anomia,* and the Brachiopoda are represented by Dr. Gwyn Jeffreys as having been dredged at from 1450 to 1750 fathoms and upwards. As for the abundance of Ammonites showing, as Dr. S. P. Woodward once supposed, the water to have been as shallow as thirty fathoms, Mr. Wallace himself would be the first to repudiate such mere supposition, were it urged against the theory he seeks to establish. Were *Nautilus* and *Spirula* shallow-water forms they would long since have been captured abundantly. The still existing shells of the Chalk itself are so few that little weight can be attached to them as an indication of depth, but in the lower Cretaceous deposits Mollusca abound, as already stated, and in perfect preservation; and their facies, taken with the complete absence of shallow-water forms, implies, Dr. Gwyn Jeffreys believes, a depth of sea in the Gault period of somewhere about 1000 fathoms. Mr. Sorby, from quite other considerations, believed the Gault to be an altered red clay, similar in all essential respects to the red clay now forming at the ocean-bottom. There seems thus to be abundant evidence, endorsed by our greatest authorities, that at least some of the Cretaceous deposits were deep-sea, while there is a total absence in them of anything necessarily indicating the proximity of land.* With regard to the Chalk itself, however, the facts are still somewhat contradictory, for it far overlaps the Gault and Grey Chalk in Devonshire, and rests upon greensand; yet although it thins out to the west it remains a perfectly pure rock, without any apparent evidence of the upper part of the formation having gradually shallowed as the sea-bed became upheaved.

The immensity of the gap, seldom adequately realized, between the true Cretaceous and the next overlying beds, implies an interval sufficient to have permitted the grandest changes in the distribution of land and water, and the gulf of the Atlantic, which stretched over the greater part of Europe,

* No American or European so-called Cretaceous land-flora can be proved to be as old as our White Chalk. The few vegetable remains found in marine Cretaceous rocks are not incompatible with the deposits having taken place at a distance from shore.

to become elevated; and, after enormous denudation, to be converted into land.

But even altogether apart from what is to be learned from the Cretaceous rocks, it is not apparent that continents have been uninterruptedly permanent. Australia and Asia, Africa and Madagascar, New Zealand and Australia, Europe and America, are all supposed to have been united at some more or less remote period; and to explain the present distribution of organisms, seas of a thousand fathoms depth are bridged over as often as it happens to be deemed requisite. But it is still questionable whether these former land connexions, which are admitted by Mr. Wallace, will be found sufficient to explain all the past as well as present peculiarities of distribution. For instance, a much more southerly land connexion between England and America seems required to explain the presence of tropical American plants, such as palms, in our Eocene, because their absence in beds of corresponding age in the United States and Greenland implies that they did not pass along the northern route traced out for them. If sea-beds have been elevated to the extent of a thousand fathoms, and if there are forces capable of elevating the highest mountains in the world from below the sea level within a comparatively recent period, why are 'hypothetical continents bridging over the deep oceans' 'so utterly gratuitous and entirely opposed to all the evidences at our command,' as Mr. Wallace wishes us to believe? There appears to be no valid reason why Europe should not have been connected with South America, by the so-called Atlantic ridge, or even Australia with South America by way of Easter, Gambier, and the Fiji Isles; for if these great banks, with islands occasionally rising to the surface, do not mean changes of level in the sea bottom, whether of elevation or depression, what do they mean?

To take other instances, in which Mr. Wallace's explanations do not seem to be the best solution of the facts. Sir Joseph Hooker, in his singularly interesting introductory essay to the New Zealand flora, stated that seventy-seven plants are common to New Zealand, Tasmania, and South America, comparatively few of which are universally distributed species. Further, there are upwards of 100 genera or well-marked groups of plants almost confined to lands of the south temperate zone, effecting 'a botanical relationship or affinity between them all, which every botanist appreciates.' For reasons which appear to be unanswerable, he has rejected the theory that these plants were transported across the seas which now separate these lands, and considers that the plants of the Southern Ocean are 'the remains of a flora that had once spread over a larger and more continuous tract of land than now exists in that ocean,' and that this land had been broken up by climatic and geological causes. Mr. Wallace supposes an emigration to have taken place from Chili by way of the South Shetland Isles, 500 miles south of Cape Horn, thence by way of an antarctic continent or group of isles, which probably extend around the South Polar area to Victoria Land, again on to the outlying Young Island, across 750 miles of sea to Macquarie Island, and, finally, across another similar distance to the 1000 fathom line, which, he considers, 'probably marks the former southern extension of Tasmania.' This appears a route beset with obstacles both climatal and geographical, and broken up by extents of sea, which Sir Joseph Hooker has expressly stated many of the plants common to these remote lands to be specially unfitted to traverse.* The bed of the ocean is as undulating as the surface of the land; and this is hardly the condition it would have assumed had its state been that of eternal rest. The objection that oceanic islands, with the exception of New Zealand and the Seychelles, hardly ever afford traces of Palæozoic or Secondary formations, and cannot therefore be remains of continents, is far from insuperable. The smaller oceanic islands, to which the statement alone seems to apply, would, if belonging to continental areas, be only the summits of mountains that are either rising or sinking; and as they are mostly of comparatively recent volcanic origin, it is hardly likely that we should meet with Palæozoic or Mesozoic stratified rocks exposed on them. It is even more curious, if they have been uplifted from the great depths which surround them, that no traces of the bottom sediment, which must have been accumulating continuously from the Palæozoic period, should have been brought up with them. Speculation is, however, useless, for the only geological fact regarding them about which we can be certain is that whatever secrets they have to disclose lie buried deeply under volcanic outbursts. It is certainly strange that Mr. Wallace makes no difficulties whatever in admitting changes of level in the sea bottom to the extent of 1000 fathoms, but will not entertain the possibility of any greater upheaval. Yet some oceanic islands must have been upheaved from vastly greater depths, and mountain chains have been raised to three times that extent in comparatively recent times.

It is well known that these forces are unceasingly acting, yet no reason is put forward to show why an elevating force once set in action in the centre of an ocean, should not continue gradually to act until even a continent is formed. In the

* The elevation of from 400 to 1300 feet which Chili and Patagonia have undergone for several hundred miles since the existence of the living species of Mollusca must imply at least correspondingly great subsidence elsewhere.

words of Prof. Huxley, 'Surely there is evidence enough and to spare that the Cretaceous sea, inhabited by various forms, some of whose descendants Sir W. Thomson, as I believe justly, recognizes in the present deep-sea fauna, once extended from Britain over the greater part of central and southern Europe, North Africa and Western Asia to the Himalayas. In what possible sense can the change of level which has made dry land of, and sometimes mountain masses of, nine-tenths of this vast area, be said, to be "in direct relation to the present existing coast-lines." That the abyssal plains were ever all elevated at once is certainly so improbable that it may justly be termed inconceivable; but there is nothing, so far as I am aware, in the biological or geological evidence at present accessible to render untenable the hypothesis that an area of the mid-Atlantic, or of the Pacific sea-bed, as big as Europe, should have been upheaved as high as Mont Blanc, and have subsided again any time since the Palæozoic epoch, if there were any grounds for entertaining it.'*

It is so obvious that the causes which lead to elevation and subsidence must react one upon the other, that I am tempted to speculate upon them and their effects on deep-sea basins. I have long been struck with the almost universal tendency to depression exhibited in areas occupied by deltas and estuaries. The thought has occurred to many, and has perhaps been most clearly expressed by Dr. Charles Ricketts, that this subsidence is produced by the accumulation of sediment.† The cause appears insignificant, yet something must determine the movement of the Earth's crust, and even an accumulation of a few feet of clay over several square miles may create disturbance, and eventually lead to a downward tendency. Supposing a sediment, 50 feet in depth and entirely submerged, to have displaced an equivalent of sea-water, we should have an increased pressure per square yard, taking the mean density of the materials composing a delta at 120 lbs. per cubic foot, of rather more than 25,000 lbs., or about 34,848,000 tons per square mile. As soon as the whole of the sea-water on an area is displaced and movement has set in, every cubic yard of sediment deposited adds a weight of about 3240 lbs.; and when we see that deltas have accumulated to depths of perhaps even beyond 1000 feet, and extend, as in the Mississippi, to 19,450‡ square miles, we can realize how vast a force is present.

* Review of the first volume of the publications of the *Challenger*. *Nature*, vol. xxiii. p. 1.

† *Geol. Mag.* 1872, vol. ix. p. 119.

‡ *Report on Mississippi U. S. War Department*, 1864, p. 434.

Records of borings in deltas are, the Po, 500 feet, Ganges, 481, Mis-

The inference as to the origin of depression, which can be drawn from delta and estuary areas, may equally be applied to coral-reefs and islands, and even to great accumulations of ice, as in Greenland; for in almost all such situations there appears to be a nearly continuous downward tendency. There are even grounds for supposing that the depression generally observable round sea-coasts may be due to a similar cause. The sediments from the wasting of the shore* are known to be thrown down almost wholly upon a belt thirty miles wide. The moving power of waves is not felt to a greater depth than forty feet; and it is therefore difficult to explain, except upon the the theory of subsidence, why in the absence of currents, the sea in proximity to shore should ever be more than forty feet deep. All ancient lands should be surrounded by extensive shoals of uniform depth, for tides appear to have no permanent action in removing sediment, and shore-currents of the requisite power are local. The prevailing action, indeed, on our own coasts appears to be silting, if we may judge from the way wrecks become imbedded; and the evidences of subsidence are innumerable. The records of submerged land vegetation are frequent, and though, on the other hand, there are in many places raised beaches, it should be remembered that while these are always conspicuous, depressed beaches cannot easily attract notice.

If it were once conceded that sedimentation directly caused subsidence, we should discover a reason for the permanence of ocean basins, for deposition must have been unceasing since Palæozoic times, and would to a large extent have filled in the depths of the ocean were this action not compensated by constant and gradual depression, exceeding perhaps the rate of sedimentation. The mean of four experiments made on the *Challenger* expedition, determined the quantity of carbonate of lime in the form of living organisms in the surface waters to be 2·545 grammes, so that if these animals were equally abundant in all depths down to 100 fathoms, it would give 16 tons of carbonate of lime to each square mile of 100 fathoms depth.†

The weight of sediment must exercise enormous pressure, tending to make the *greatest depths* of the sea permanent, and to continually elevate lines of least resistance into ridges or banks, leading where the state of tension is extreme, to

sissippi, 630, in which the lowest beds reached were turf and vegetable matter. The total thickness of many deltas, such as that of the Ganges, may be inferred from the depth of the sea in which they are accumulating.

* The denudation has been estimated to equal nineteen feet in 1000 years.

† In great depths shells are reduced to bicarbonate, and this may imply loss of material. The supply of lime does not seem, however, to be obtained to any great extent from dead organisms, but is probably kept up by rivers.

isolated volcanic outbursts. The lines of absolute least resistance would probably, however, generally coincide with sea-margins, and upon coasts, therefore, while we might find a tendency to local depression, owing to the littoral sedimentation at a few miles from land, there would be inland a far more important and preponderating tendency to elevation.

Thus there would ever be a direct action deepening ocean basins where they are deepest, and raising up the shallower parts to higher levels, thereby slowly lessening the superficial area occupied by seas. On the other hand, the dry land would extend in a corresponding degree, and its surface become more diversified, for new mountain chains would in succeeding ages have a tendency to greater and greater elevation.

I think all we are able to gather from the records of Palæozoic rocks points to a comparative uniformity in the condition of the earth's surface in remote times, there being neither evidence of great depths in the sea, nor of mountainous elevations in the land. These conditions, to judge from palæontological evidence, were increasingly modified during the Secondary period, and on to the present day; so that the theory that increasing weight causes increased depth, derives support from the Geological Record.