ORIGIN AND RELATIONS OF CONTINENTS AND OCEAN-BASINS.

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(Introduction.)

The reasoning of Thomson, Hopkins, and Darwin, and the very important consideration that pressure — except with a few bodies which, like water, expand on freezing — must raise the fusing point and thus favor solidification, leave but little room to doubt that the earth as a whole is a very rigid body. So convincing are the arguments, that this notion of an essentially solid earth has already supplanted, in the minds of the great majority of geologists, the view but recently almost universally held that below a comparatively thin solid crust the globe is yet wholly liquid.

While the reconstruction of dynamical geology on the basis of a solid earth undoubtedly marks a great advance in the science, yet it appears to me that some eminent writers have carried the new views too far, and made certain portions of the earth too solid. Thomson asks for an average rigidity equal to that of steel, and Darwin says that no considerable portion of the interior of the earth can even distantly approach the fluid state; but this is not necessarily inconsistent, as many appear to think, with the existence of a shallow, discontinuous, *plastic* zone between the rigid crust and a nucleus *more rigid than steel.*

One of the most important results reached by Thomson is that up to this time the refrigeration of the earth has been limited to the superficial portions, that below a depth not exceeding 300 miles the temperature ceases to increase downward at a sensible rate, and that the maximum terrestrial temperature is probably not above 7,000° to 10,000° Fah. In other words, the great interior portion of the earth, embracing five-sixths of its volume and probably seven-eighths of its mass, has a sensibly uniform temperature, which is not incomparably higher than temperatures with which we are familiar on the earth's surface. But the pressure to which materials in the interior of the earth are subjected increases steadily all the way to the centre. Therefore, since heat acts against, while pressure promotes, rigidity, if the earth-matter, as is generally believed, is solid at a depth of 300 miles in obedience to pressure (about 2,000,000 pounds per square inch) and in spite of its high temperature, it is to my mind quite conceivable that at greater depths, where the temperature is not sensibly, or at most not greatly, higher, a pressure ten times greater (or 20,000,000 pounds per square inch) may induce a degree of rigidity exceeding that of steel or any substance with which we are acquainted.

The extreme probability, if not the absolute certainty, of the existence of this high degree of rigidity in the central portions of the earth becomes apparent when we consider that, as Professor

Dana has correctly stated,[1] the rigidity of slowly solidified rock, at the earth's surface, is beyond that of glass or steel.

If we may assume that rigidity varies with varying temperature and pressure according to some simple and similar, though contrary, laws, then by the side of Thomson's temperature curve we may construct a curve for pressures, based on any probable law of the downward increase of specific gravity; and readily observe where the relations of temperature and pressure are most favorable for a plastic zone; *i. e.*, where the pressure is least relatively to the temperature. This region must be comparatively superficial, since the temperature increases downward rapidly at first and very slowly at greater depths; and I have found in this way that the ratio of temperature to pressure probably reaches its maximum between 40 and 80 miles below the earth's surface.

But the experiments of Daubrée have shown, contrary to the old idea, that the capillary absorption of water by the earth's crust, within certain limits at least, is facilitated, rather than hindered, by the high subterranean temperatures; and hence we are warranted in believing that the crust is hydrated to a very great depth. Now the principle is generally accepted that water reduces in a marked degree the fusing points of rocks, liquefaction resulting from aqueo-igneous fusion at temperatures far below those of purely igneous softening. Therefore, since the minimum ratio of pressure to temperature occurs so near the surface, water becomes an important element in locating the most probable position of a plastic zone. The maximum plasticity, other things being equal, will be determined, approximately at least, by the minimum ratio of the pressure to the product of moisture and temperature; and this probably exists nearer 40 miles than 80 miles below the surface. Several well established and entirely independent facts — such as the high specific gravity of the earth as a whole, the excess of basic elements in certain eruptive rocks, and the metallic nature of many meteorites — point unequivocally to the conclusion that the earth's interior consists chiefly or entirely of metals, among which iron must occupy a prominent position. Now it may very well be that, as Judd has suggested in his recent work on volcanoes, the access of water to the outer portions of this highly heated but only partially oxidized earth-

[1] Amer. Jour. Sci. (3), VI, p. 11.

matter is attended by farther oxidation and the evolution of considerable heat. So that it is conceivable that a higher temperature may reign in the lower portion of the hydrated crust than in the regions immediately below the limit reached by the water; and it will not escape observation that this higher temperature is developed at the point where the other conditions are most favorable to the formation of a plastic stratum. If we hold that in the anhydrous globe the pressure is everywhere more than a match for the temperature, it is yet difficult to believe that it can be so in the lower levels of the hydrated crust, where moisture coöperates with a temperature of from 2,000° to possibly 5,000° Fah. In other words, from these generally accepted principles the inference flows almost irresistibly that the earth possesses a plastic zone, and that this is near the surface; and, as already pointed out, this is in harmony with the conclusions of Thomson and Darwin, if we make the not unreasonable assumption that the central portions of the earth possess a degree of rigidity greater than the mean which they require.

One fact to which considerable weight has been attached by Le Conte [1] as an argument against a plastic zone is rapidly disappearing. I refer to the elliptical form of the earth's equatorial section. It is certainly true that the existence of such a zone would be extremely improbable, if the inequality of the equatorial radii was very marked. Now the values of these radii usually accepted are those determined by Col. A. R. Clark, of the British Ordnance Survey, according to whom the major exceeds the minor radius by 6,378 feet, the major axis intersecting the meridian of 15° 34′ E. But Colonel Clark has published a revision of his results, founded on new data; and he now finds the difference between the equatorial radii to be only 1524 feet; whilst the meridian of the major axis is 8° 15′ W.; and the polar radius is lengthened 1000 feet. Colonel Clark, himself, evidently regards the ellipsoidal form of the equator as doubtful. Thus, there is, at all events, no *proved* result of geodesy opposed to the hypothesis of a plastic zone in the earth.

On the other hand, there are many important and generally admitted geological facts which can be satisfactorily explained only on the supposition of a yielding stratum at a moderate dis-

[1] Amer. Jour. Sci. (3), IV, p. 351.

tance below the earth's surface. This stratum, however, in order to answer perfectly all the purposes of the geologist, need not be very extensive in the vertical direction. If we assign it a volume equal to one-hundredth that of the earth, I think there are few geologists who will not find it sufficient. Nor need the stratum be absolutely continuous, though it must be nearly so. Nor, again, do we require anything more than a very low degree of plasticity.

Babbage's theory that elevation and subsidence of the earth's crust are due to the expansion and contraction of rocks by heating and cooling is an adequate explanation of some local and limited movements, in volcanic districts. But it is no explanation at all of extensive movements affecting large areas. Let us observe the increase in temperature required in one hundred miles in thickness of the earth's crust to lift the great plateau of central Asia from the level of the sea to its present altitude of 16,000 feet — approximately, an expansion of three miles in one hundred. Taking Colonel Totten's highest coëfficient of expansion — .000,009,532, or say .000,01 — then to produce an expansion of .03 we must have an average increase of 3,000° Fah. The increase, evidently, cannot be so large near the surface, and consequently must be larger at considerable depths. But at a depth of one hundred miles, according to Thomson, the crust has nearly its original temperature, and cannot lose or gain heat in any such fashion. Therefore, a smaller thickness must be heated still higher. But at all depths not very near the surface we suppose the rocks to have temperatures near their fusing points; and an increase of from 3,000° to 5,000°, without increase of pressure, would surely fuse them. And, further, no explanation is offered as to where this heat comes from or goes to. A clear statement of this theory, as applied to really important instances of elevation and subsidence, is sufficient to refute it. It breaks down with its own weight. It also fails to explain local movements which are paroxysmal. Babbage appreciated some of these difficulties, and suggested that the elevation of extensive areas of the sea-floor, where sediments are accumulating, could be explained by expansion due to rising of the isogeotherms. But this would not account for subsidence any where, nor for the elevation of any old land-surface. Besides, it is a well established fact that areas where thick deposits of sediments are formed usually are, and, as a rule, *must* be, areas of *subsidence.*

Herschel started with this datum of the science, and added the following: the rising of the isogeotherms softens and weakens the hydrated crust; and the weight of increasing sediments bears it down. But it is now generally conceded by geologists that subsiding sea-floors are the *cause* and not the consequence of thick deposits of sediments.

Although the true theory of elevation and subsidence must be one recognizing a variety of causes; yet the mere thickening and thinning of the crust by expansion and contraction, and the softening of its lower portions, do not explain the grand facts of the rising and falling of continents and ocean-floors. In short, the magnitude of the phenomena compel us to suppose that, in many cases, elevation and subsidence are due to the bending of the crust through its entire thickness. But this supposition involves the farther supposition of a plastic zone beneath the crust. And, as a matter of fact, the great majority of geologists, in spite of the reaction from the hypothesis of a liquid globe, do admit the existence of a relatively soft and plastic layer as the only satisfactory explanation of the extensive vertical movements of the earth's surface. This point is even conceded by those who, like Professor Dana, stand most strongly, in their theories of continents, for a globe continuously solid from centre to circumference.

In the American Journal of Science (3), vol. 6, p. 7, Professor Dana says: "The Appalachian subsidence in the Alleghany region of 35,000 to 40,000 feet, going on through all the Paleozoic era, was due, as has been shown, to an actual sinking of the earth's crust through lateral pressure, and not to local contraction in the strata themselves or the terranes underneath. But such a subsidence is not possible, unless seven miles — that is, seven miles in maximum depth and over a hundred in total breadth — of something were removed, in its progress, from the region beneath." In other words, "there existed, underneath a crust of unascertained thickness, a sea or lake of mobile (viscous or plastic) rock, as large as the sinking region." And it is also stated that "this under-Appalachian fire-sea probably dated back to the era of the general fluidity of the earth."

have extended east beyond the existing border of the continent. And he further states that "the under-crust fire-sea on the Pacific border must have had great length from northwest to southeast; and, also, great breadth, for the border region is at least 1,000 miles wide; and great breadth and great length seem plainly its characteristics even till Tertiary times." Again, "It is further to be noted that, in the course of past time, the whole continent has had its surface, from one side to the other, criss-crossed with oscillations and lines of disturbance, from the lateral pressure acting against its opposite sides, whence it is clear that the continental subterranean areas were *once* continuous. The facts from the ocean seem to demand a vastly greater range for the under-crust mobile layer." And, finally, in conclusion, Professor Dana says there is a flexible crust and mobile rock beneath it.

Thus we find Professor Dana fully committed to the view that the earth contains a nearly continuous plastic zone; although his theory of continents, as will appear farther on, appears to require that the continents should be for the most part at least, rigidly connected with the earth's solid nucleus. We hardly seem warranted, however, in *assuming* — for it certainly has not been proved — that a nearly if not quite universal mobile layer, which we are compelled by the phenomena of elevation and subsidence to believe was in existence from the period when the entire globe was an incandescent liquid down to the later Tertiary times, suddenly became solid under the continents with the close of that epoch.

Besides, we have abundant evidence, not only that nearly all parts of the globe experienced extensive vertical movements during the Tertiary period, but also that the grand continental oscillations do not belong wholly to the past; for great changes in the elevation of the continental masses, and in the relative distribution of land and sea, are now in progress. Geologists are familiar with this evidence, and I will notice only a single example. Perhaps the most striking instance of change of continental level now in progress is that afforded by the western half of South America. It was long ago demonstrated, by the observations of Darwin and others, that this part of the earth's crust is *now* rising slowly, yet at a rate which is measurable from century to century, or even from decade to decade. And the evidence is very clear that this movement has been going on for a long time over the whole southern half of the continent, but especially on the west side. From Cape Horn northward, 1,200 miles on the east coast and nearly 2,500 miles on the west coast, old sea-beaches and terraces full of species of shells now living in the adjacent seas, are observed at heights ranging from 100 to 1,300 feet above the sea. These are the facts furnished by Darwin, but more recently Alexander Agassiz has traced the ancient shore line in Peru at an elevation of 3,000 feet by means of coral still sticking to the rocks. There is also evidence that points distant from the coast rise faster than the coast itself.

Here are indisputable facts which demand a plastic zone under a large section of South America at the present time. And, by presenting the similar facts observed in other continents, it would be easy to show that a very large part of the land of the globe is now moving up or down, and hence must be now underlaid by a plastic stratum. While, on the other hand, there is not a vestige of trustworthy, positive evidence to show that this plastic stratum is not as wide-spread and continuous to-day as it ever was; and the probabilities certainly are that it is nearly so. That is, contrary to the ideas inculcated by some leading geologists, I venture to assert that we have no evidence that the continents are more stable now than they were during the Tertiary or Secondary periods.

Among recent geological writers of note, there are none who are more pronounced believers in a thoroughly solid earth than Archdeacon Pratt and Prof. Joseph Le Conte. They both appear to deny the existence of a plastic zone, considering the earth as entirely solid, with the exception of a few limited and isolated lakes of liquid rock situated in the crust and forming the sources of volcanic materials. Rejecting the idea of a plastic zone, Le Conte is, as he confesses (Amer. Jour. Sci. (3), vol. IV, p. 472), unable to explain the phenomena of elevation and subsidence. Le Conte is an able and prominent advocate, and in part the author, of the theory of the formation of mountains which has been developed during the last score of years — chiefly by Hall, Hunt, Dana, and Le Conte — and which is at present very generally accepted by geologists. The most prominent and essential feature of this theory is that mountains are due to a *horizontal* mashing up of the earth's crust along lines of weakness, the crushing force being simply the tangential thrust induced in the crust by the continued cooling and contraction of the earth's interior, after the crust has attained a constant temperature and volume. Now, according to Le Conte, this horizontal crushing of the crust, as evidenced by folds and slaty cleavage, is so great that on the average the breadth of the crushed area — the mountain-zone — is diminished in the ratio of 5 to 2; 10,000 feet becoming 4,000 feet, and 10 miles becoming 4 miles. In other words, the formation of a mountain-range is equivalent, in one sense, to closing a fissure several miles in width; and this requires a decided horizontal movement in a very large part of the earth's crust, the latitude and longitude of places being permanently changed.

This slipping of the crust over the nucleus could not take place without a plastic zone, which is thus seen to be essential to the generally accepted theory of the origin of mountains.

Therefore, to sum up, whether we consider the relative distribution of temperature, pressure and moisture in the earth's crust, the phenomena of elevation and subsidence, the formation of mountains, or the origin of volcanic products, to which I have scarcely alluded, we equally reach the conclusion that the earth must contain a nearly universal plastic stratum. Consequently, any theory of the origin and relations of continents and ocean-basins incompatible with the existence of this mobile layer must, in the present condition of the science, be considered as in so far inadequate. A yielding layer or bed, we are compelled to believe, underlies the continents and seas and must be regarded as fundamental in any true theory of these grandest of the earth's surface features.[1]

1 Within a few months several prominent geologists have revived the old view that below a thin, solid shell the earth is wholly liquid. So far as the relations of continents and ocean-basins are concerned, the condition of the central portions of the earth is of little consequence to one who has accepted the view here insisted upon, that the crust rests upon a nearly continuous mobile layer; for the theory of a liquid globe merely extends the mobile layer to the centre. It does not appear to the writer, however, that any arguments yet advanced forbid us to believe in the essential solidity of the earth's great central nucleus. Hon. J. W. Powell, Director of the United States Geological Survey, holds (Science, III, 480) that the phenomena of faulting, plication and vulcanism are incompatible with a solid earth. But certainly a mobile layer ten to twenty miles thick would satisfy the dynamical geologist as well as one 4,000 miles

THEORIES OF THE ORIGIN OF CONTINENTS AND OCEAN-BASINS.

Taking a general view, the theories or explanations of the origin of continents and ocean-basins may be regarded as two in number. A brief statement of each of these will be first in order; and after that we will consider more in detail the arguments that oppose or support them.

1. We have, first, what may be called the old theory, the one taught by Lyell and all the older geologists and still very generally accepted. This is the theory held by the present writer; and hence the following statement of it on account of the personal coloring may not, as a whole, meet the views of those who would still indorse its main features: The mobile stratum between the solid nucleus and the solid exterior lies at the foundation of this theory; for the continents and ocean-basins are here regarded as broad upward and downward *bendings* of the crust. These great crust-flexures are produced and sustained by the tangential thrust arising from the contraction of the earth's interior. The final result of the increasing horizontal strain in the crust is that the crust is mashed up and a mountain-range formed. No crushing can take place, however, until the strain reaches a certain high maximum; for otherwise it would go on all the time, and we would recognize no distinct mountain-building epochs in the earth's history. But I hold that, until the crust can obtain final and permanent relief by crushing, it will accommodate itself to the shrinking nucleus by undergoing a grand distortion or warping, which will be slight in the vertical direction when the strain is small, but as the strain accumulates during the lapse of ages the deviation from the normal spheroidal form will *tend* to become greater — the continents higher and the oceans deeper —, though this may not actually occur in every case because denudation is constantly degrading the land and filling up the seas. It

thick; and on this basis we can reconcile the conclusions of the geologists and physicists. It is doubtful, however, if physicists will accept Major Powell's novel argument from the "flow of solids;" namely, "that pressure itself would reduce the interior of the earth to a fluid condition." Dr. M. E. Wadsworth (American Naturalist, XVIII, 587) points out the self evident fact that Thomson and Darwin necessarily assumed as the basis of their reasoning conditions different from those of the actual earth. But he has not shown that the difference is of such nature as to necessarily or even probably invalidate their conclusions.

is a principle generally accepted by geologists that the accumulation of sediments on the subsiding sea-floors along the margins of continents is attended by an elevation of the isogeotherms and a consequent softening of the crust. Thus are developed the weak zones which yield and are mashed up to mountain-ranges when the crust can no longer resist the growing tangential strain. And when this occurs the distortion or warping of the crust is lessened which means a diminution of the mean height of the continents; though their margins are elevated by the mountain-making. These periods of catastrophe and mountain-making are regarded as favorable to more or less extensive interchanges of land and sea, of areas of elevation and depression; for this theory does not require that the continents should be fixed.

2. The second theory is that developed during the last third of a century, chiefly by Professor Dana, and commonly known as Professor Dana's theory. The main points in this theory, as gathered from the latest expression of Professor Dana's views, are the following:[1] — The earth, superficially at least, is, and was originally, before it had a solid crust, of unlike composition on different sides. This heterogeneity caused a corresponding difference in heat conductivity. The more rapidly conducting areas cooled fastest and were the first to become covered with a solid crust. Solidification is attended by contraction; and therefore the newly formed crust must have been heavier than the liquid immediately beneath it. As a consequence it broke up and sank until it reached a liquid stratum of the same specific gravity as itself; and afterwards the process of crusting and sinking went on until a solid crust was built up from this point to the surface. Through the continued escape of heat this primitive crust is thickened and is still thickening by additions to its lower surface. These first formed portions of the crust became, and will always continue to be, the continents. The remainder of the earth's surface was still liquid, after the solidification of the continental areas was well advanced; and, of course, as long as it continued liquid its surface was level with that of the crust areas. Finally it became the theatre of a similar process of crusting and sinking, and at last permanently froze over. Now the main point is that the contraction of this inter-continental crust during its formation

[1] Amer. Jour. Sci. (3), vols. V and VI.

caused its surface to sink below that of the continents; and the depressions thus developed became the future ocean-basins, which, like the continents, are, according to this theory necessarily of a permanent character. Indeed, it is a plain deduction from Professor Dana's hypothesis that the existing continents and oceans are as old as the earth's crust. Although subject to extreme changes of level and frequently submerged, yet no portion of the continent has ever become the site of the deep central ocean; nor has any portion of the floor of the abyssal sea ever been elevated to form continental land. In the beginning, the continents were narrow and the oceans shallow; and during the course of geologic time the continents have become constantly wider and the oceans deeper.

Closely related to Professor Dana's theory is that held by Archdeacon Pratt and Professor Le Conte. The following statement of this theory is given in Le Conte's own words:[1]—"Continental surfaces and ocean-bottoms are due to *unequal* radial contraction of the earth in its secular cooling. It is evident that in such secular cooling and contraction, unless the earth were perfectly homogeneous, some parts being more conductive would cool and contract more rapidly in a radial direction than others. Thus some radii would become shorter than others. The more conductive, rapidly contracting portions, with the shorter radii would become sea-bottoms; and the less conductive, less rapidly contracting portions, with the longer radii, land-surfaces. In other words, the solid earth becomes slightly deformed and the water collects in the depressions." Le Conte and Pratt further hold that the quantity of matter along each of the terrestrial radii was not only originally, but is yet, essentially equal; the matter being denser along the shorter oceanic than along the longer continental radii."

Certain passages in Le Conte's writings lead one to infer that he regards his theory as essentially similar to Professor Dana's. Nevertheless, the language quoted above shows that the difference is fundemental. They agree in requiring a heterogeneous earth as a basis for unequal radial contraction and fixed continents; but beyond that they are diametrically opposed. For Professor Dana says that the more rapidly conducting and cooling

[1] Amer. Jour. Sci. (3), vol. IV, p 352.

areas form the continents; but Professor Le Conte says they form the ocean-bottoms. Professors Dana and Le Conte agree, however, upon the main point against which the principal arguments of this paper are advanced; namely, that the continents and ocean-basins are permanent, their present positions being those which they occupied at the beginning of geological time.

We pass now to a more detailed examination of the two main theories, with the view to determining which one, in the light of our present knowledge has the best claim to the title of the true theory of continents and ocean-basins.

1. **Continents and ocean-basins are upward and downward bendings of the earth's crust.**

This theory harmonizes perfectly with the known facts of elevation and subsidence; and it is the only theory that does offer an adequate explanation of this important class of phenomena. It permits the interchange of continents and seas; a relation which is supported by a vast mass of geological data, and against which no sound argument has yet been urged. The principal, and, so far as I am aware, the only serious objection to this theory is that brought forward or at least specially insisted upon by Le Conte.[1] He says that the great crust arches could not sustain themselves for a moment, even if the crust were several hundred miles thick; but the continental arches would break *down*, and the oceanic arches would break *up* and restore a level surface. But he goes still further and remarks: — "So great is this force tending to the general form of equilibrium that, even if the earth as a whole were rigid as a solid globe of glass (that is, had no plastic stratum), it could not resist it. Hence he is driven to the conclusion, already expressed in his theory of continents, that there is the same amount of matter along each of the terrestrial radii; since, if there were not, the continents, although they are parts of a globe continuously solid and as rigid as glass, would, according to his view, settle down after the manner of the inequalities of a mass of pitch. If, then, we can show that the masses of the terrestrial radii are not equal Professor Le Conte's continents are left absolutely without support. Now nothing could be easier than to demonstrate this. For, although we may readily grant that, leaving out of view the equatorial protuberance due

[1] Amer. Jour. Sci. (3), vol. IV, p. 346.

to the earth's rotation, this was the normal constitution of the liquid globe; yet, after solidification, this equal radial distribution of matter would very soon be spoiled by denudation; and now, after the continents have been repeatedly swept away and the débris piled upon the sea-floor, we may fairly claim that it has entirely ceased to exist. The horizontal movement of the crust during mountain-making also operates to the same end; for there is certainly, as Professor Le Conte himself insists, more matter along the radii terminating in the Sierra Nevada and other mountain ranges now than before the mountains were formed.

The notion, then, that the terristrial radii are now of equal weight is wholly untenable; but the continents, with their vast plateaus and mountain ranges are actual existences; proving that Le Conte has greatly over-estimated the levelling effect of gravitation upon the inequalities of the earth's surface.

Having shown that the well established facts of denudation and mountain-making are a sufficient refutation of the second part of Le Conte's objection to the theory here advocated, I turn now to the consideration of the first part where he states, in effect, that continental and oceanic arches resting on a plastic stratum could not sustain themselves even though the crust were several hundred miles thick. But, first, I will point out an apparent oversight in the only alternative which he allows those who believe in a plastic stratum. He says, "If there be, indeed, a solid crust on a liquid interior (or plastic zone), in order to sustain itself the inequalities of the upper surface in contact with the air *must be repeated on the lower surface;*" and his figures in illustration show inequalities on the under surface of the crust equal in form and size to those on the upper surface. This equality between the upward and downward protuberances of a floating crust could exist only in the case where the density of the crust is just half that of the supporting liquid; but we are obliged to suppose that the difference in specific gravity between the crust and liquid in the case of the earth would be very small. If we assume that the liquid is one-tenth heavier than the crust, then a protuberance of two miles on the upper surface of the crust would require a protuberance of twenty miles on the lower surface to sustain it. Hence Le Conte is quite right in saying that this theory "breaks down with its own weight;" and it becomes doubly important for those believing in a plastic stratum to show that an arched crust is a possibility.

In the first place I desire to direct attention to the *very slight* amount of distortion required in the crust to produce the existing continents and oceanic hollows. Le Conte's diagram gives a wrong impression, because the depressions are never concave. Taking the mean height of the land at 1,000 feet and the mean depth of the sea at 15,000 feet, we have a mean relief of 16,000 feet. So far as this argument is concerned, however, the oceans are not so deep; for the water helps to hold the depressed surfaces down and thus to maintain the distortion, being equivalent in this respect to a layer of rock equal in thickness to two-fifths of the depth of the ocean — 6,000 feet. This reduces the total effective relief to 10,000 feet, or say two miles, which is one-two thousandth of the earth's radius. In other words, the oceanic depressions, taking the average depth, are not deeper relatively than the varnish one-fiftieth of an inch thick on a globe eighty inches in diameter. On such a model of the earth the continents and ocean-basins would be produced by warping the film of varnish to the extent of its own thickness.

Le Conte denies the possibility of crust-arches, even if the crust is several hundred miles thick. But he admits, as all geologists must, that the tangential strain due to the cooling and shrinking of the earth's interior accumulates through long geological ages before the crust finally finds relief by crushing. Of course no one supposes now that this horizontal pressure ever even distantly approaches its highest conceivable maximum, which would result when an actual separation occurred between the earth's nucleus and crust. But the attainable maximum pressure is, nevertheless, very great; being sufficient to crush a more or less rigid crust at least twenty, and possibly fifty, miles in thickness. Now, the important question seems to be, has this enormous lateral thrust any visible effect upon the crust before the time when the crushing of the latter begins? Those who answer in the negative must be prepared to show that the crust is far more uniform in thickness, weight, and rigidity than most geologists would probably be willing to admit that it can be. The case is like this: by the shrinking of the nucleus, the crust is left unsupported to a certain extent; but it will continue to maintain its original form of fluid equilibrium, and the strain will be without visible effect until it becomes great enough to crush the crust; provided that the tendency to follow the contracting interior is equal in all parts of the crust; *i. e.*, provided that the thickness and rigidity of the crust are sensibly the same at all points. The accepted theory of mountain-formation, however, not to mention other considerations, forbids us to believe that the crust is thus homogeneous. But if it is not, then it must, in the case supposed, be in a state of unstable equilibrium; the thicker and heavier parts are pulled downwards most strongly; and the result is a grand distortion or warping of the crust whereby its interior capacity is diminished so that it can accommodate itself to the shrunken nucleus. The depressed areas become sea-floors and the elevated areas continents. It is not clear how those who admit the existence of a plastic stratum in the earth can consistently doubt that a condition of unstable equilibrium in, and consequent warping of, the crust would inevitably follow the shrinking of the nucleus.[1] Of course, if the earth is continuously rigid from centre to circumference, as Le Conte claims, there is an end at once of all extensive elevation and subsidence, and of all mountain-making too. For, although Le Conte has not said it, yet it seems to be a logical consequence of his theory that the crushing of the crust would not be limited to narrow zones since that implies a slipping of the crust over the nucleus which would be impossible if the two were everywhere rigidly connected; but the crushing would be, like the wrinkling of the skin of a withered apple, more or less uniformly distributed, producing many minor corrugations but no lofty, well-defined and dominant ranges.

As already stated the warping of the crust, unlike the crushing, does not require that the tangential pressure shall first accumulate through long periods of geological time; but we may fairly suppose that it begins soon after the development of the pressure

[1] This point is easily illustrated by experiment. If from the interior of a thin spherical shell of some flexible substance we remove a portion of the air so that the atmospheric pressure, which may be taken to represent gravity in the case of the earth, will be greatest on the external surface and thus develop a tangential pressure in the shell, it will be readily seen that it would be almost impossible to construct a model of such perfect uniformity and symmetry that a general distortion or flattening of its form would not be the immediate result.

and grows as that grows. Of course this distortion does not relieve the crust from the lateral thrust; but, after warping, the thrust is due, not to the *partial* action of gravity upon the whole crust, but to the *entire* weight of the continental masses, or those comparatively small portions of the crust lifted above the mean level of the ocean-floor.

The argument, then, shapes itself in this wise; granting that the earth contains a mobile layer, and that the crust is not equally thick, rigid, and convex in all parts, it follows that as the contraction of the nucleus goes on the crust experiences either a general distortion or continuous crushing; but we are obliged to reject the last supposition, because geologists recognize in the earth's history distinct mountain-forming epochs. A certain amount of distortion, therefore, is inevitable; and we have simply to consider whether the existing relief of the continents could probably be developed in this way before the strain would become great enough to crush the crust. According to Mallet, if the crust were completely separated from the nucleus, its sustaining power would not exceed one four-hundredth of its own weight. Now the continents, if we may estimate the thickness of the crust at fifty miles and the continental relief at two miles and make proper allowance for the higher specific gravity of the lower portions of the crust, embrace about one one-hundred and fiftieth of the mass of the earth's crust; or a load two to three times greater than the crust could sustain, if it were entirely unsupported. But it is proper to assume that mountain ranges, being due to the horizontal mashing of the crust, are in part sustained by corresponding inequalities of the under surface of the crust. And besides there is a vast difference between an arch supported only by its abutments and one resting at all points upon matter which is only imperfectly plastic, which is plastic perhaps only in comparison with the enormous pressure to which it is subjected, and which under some parts of the continents may be as rigid as the crust itself. In view of these considerations it seems probable that the continents approach, though they do not necessarily exceed, the maximum load which the crust can sustain; and it may be that in the limited sustaining power of the crust we have a correct explanation of the comparatively uniform and small depth of the oceanic hollows. This view with regard to the behavior of the earth's crust under the influence of the tangential thrust is indorsed by Professor Dana, althought it appears to be inconsistent with his theory of the origin of continents. As already shown, he admits the existence of a nearly universal mobile layer; but he also goes farther and insists that *all important movements of elevation and subsidence, except where mountains are formed, are due to the bending of the crust in consequence of the lateral pressure arising from the cooling and shrinking of the interior.* In other words, Professor Dana says that *the crust experiences a grand distortion before the strain becomes great enough to crush it.*

The argument for this theory may be summed up as follows: The earth either has or has not a plastic stratum. If it has not, it is impossible to explain the phenomena of elevation and subsidence and the formation of mountain systems; but if it has, the continents and ocean-basins can only exist as upward and downward bendings of the crust, and against this view of their constitution no insuperable objections have been urged.

2. **The continents and ocean-basins are due to the greater conductivity and more rapid cooling of the continental portions of the earth's crust; hence they are permanent, and the continents are wider and the seas deeper now than at any former period.**

This theory rests at the very outset upon an assumption which is not supported by a vestige of evidence; namely, that the earth was originally, and is now, of unlike composition along different radii or on different sides, the continental portions of the crust being composed of denser materials than the oceanic. If the liquid globe had possessed this constitution the ellipsoidal form of the equatorial section which, under the most refined measurements of geodesy almost disappears, would, in obedience to the laws of hydrostatic equilibrium, be strongly marked.

It seems strange that an assumption so vital to the theory should have been made without any attempt to demonstrate its validity. What are the facts that support it? Where are the analyses showing an essential unlikeness in composition between the different portions of the earth's crust? — showing, in other words, that the rocks exhibited on the continents are denser, *i. e.*, more basic on the average, than those forming the ocean floor?

We may safely say that the known facts and the probabilities are all against the supposition that such a difference exists. But without this unproved difference in composition there could be no difference in conductivity and radial contraction and the theory entirely fails. However, granting, for the sake of the argument, the possibility of this diversity of composition; it still fails as a foundation for the theory, since it could only have been a very transient characteristic of the crust. For, as Professor Dana admits, the elevation or subsidence of large areas of the crust must involve a horizontal displacement of liquid material beneath. Material under the Pacific, for example, being squeezed under the bordering continents. But this process mixes up the matter which by cooling forms continents and ocean-basins through unequal contraction; and the areas of high and low conductivity are no longer kept distinct.

But we may grant farther the possibility of a permanent difference in composition and still doubt the necessity of Professor Dana's inferences. As a rule dense bodies are not only good conductors of heat, but they also have low fusing points. This is eminently true of the main constituents of the earth's crust. The most favorable supposition that could be made for Professor Dana's theory would be that the continents have, or had originally, the composition of basalt and the sea-floors the composition of granite; and in any case the difference in composition must be regarded as similar to, but less rather than greater, than that between basalt and granite.

But if areas of liquid basalt and granite have the same initial temperature and cool under identical conditions, it does not necessarily follow that the basalt areas will solidify first. The basalt, on account of its greater conductivity, loses heat more rapidly than the granite; and yet, in view of the higher fusing point of the granite, the probabilities are that it would first assume the solid state, the two rocks being wider apart in their fusing points than in their power of conducting heat. This conclusion is abundantly sustained by the observations made on the relative liquidity of basic and acidic lavas. Rhyolite and trachyte solidify at a high temperature and so rapidly, when exposed to the air, that they are often left in the amorphous or glassy condition of obsidian; while basalt, congealing at a much lower temperature, usually remains fluent long enough to permit more or less crystallization and rarely retains the vitreous texture. Hence vitreous basalt or tachylite is a rare rock, although vitreous rhyolite or obsidian is abundant.

In short, this theory requires an unproved and improbable constitution of the earth's crust; but, if the existence of this indispensable condition were demonstrated, the known facts with regard to the solidification of basic and siliceous rocks would apparently lead us to a conclusion diametrically opposed to that reached by Professor Dana; namely, that the lighter and more slowly conducting areas would become solid first, forming the *continents*, while the denser and more rapidly conducting areas would be the last to solidify and would form the *ocean-floor*. In connection with his theory, Professor Dana offers the following explanation of the fact that the land is mainly in the northern hemisphere; the southern hemisphere is composed of heavier material than the northern, and consequently the ocean is attracted in the former direction. But it will not escape observation that this admission that the densest matter is under the sea sustains the point made in the last paragraph and is a direct contradiction of the most essential part of Professor Dana's theory of the origin of continents.

Professor Dana says that his theory accounts for the abrupt slopes of the continental borders, the ocean deepening rapidly and not gradually, after we cross the true edge of the continent. But it seems to the present writer that this is just what Professor Dana's theory does not account for. For, if we were to admit that the earth is of unlike composition on different sides, it would certainly be contrary to all analogy to suppose that the areas of unlike composition are sharply marked off from each other; and yet the steep slopes of the oceanic depressions, according to this theory, require an abrupt change in radial contraction and consequently in conductivity and composition.

The existence of a plastic zone beneath the crust is inconsistent not only with the supposition that there is a marked absence of uniformity in the composition of the crust, as has been pointed out; but it is also believed to be inconsistent with another essential feature of Professor Dana's theory; namely, that the existing relief of the crust has originated in the unequal radial contraction of the areas of dissimilar composition. To appreciate this point we have only to conceive separate blocks of the crust floating on the mobile layer, and to reflect that they must be of nearly the same density as the layer in which they are immersed. If we suppose the liquid to be one-tenth heavier than the blocks, it is clear that blocks forty miles thick would rise four miles, or one-tenth their thickness, out of the liquid; while blocks ten miles thick would project only one mile; a difference of thirty miles in the thickness of the blocks corresponding to a difference of three miles in their altitudes above the surface of the liquid. But, if the blocks are connected to form a continuous crust, the principle of flotation will hold just as truly, the upper surfaces of the thick and thin portions being much more nearly in one plane than the under surfaces. Therefore the development of the existing relief of the earth's surface in accordance with Professor Dana's theory would require an excess of contraction in the oceanic as comppared with the continental areas of more nearly thirty miles than three miles as Professor Dana has estimated, and this enormous contraction would correspond to a continental crust nearly four hundred miles thick. In fact, this theory is entirely inconsistent with a floating crust; but it demands, on the contrary, that the earth should be solid from centre to circumference, at least under the continents; and yet Professor Dana admits that the Rocky Mountains have been elevated 8,000 to 10,000 feet since Cretaceous time, and that the eastern part of the continent subsided 40,000 feet during the Paleozoic era, and so on.

But, ignoring for the present the principle of flotation, let us assume that the theory in question is sound up to this point, and inquire whether unequal contraction of the continental and oceanic areas could produce the depressions of the earth's surface. We will suppose, with Professor Dana, that average rocks contract eight per cent between the liquid and solid states — much less than Bischof's experiments show, but more in harmony with the most recent determinations; and make the extremely favorable though improbable supposition that the oceanic areas remained liquid until the continents became entirely solid. Now Professor Dana says the average depth of the depressions is three miles, equal to the contraction resulting from the cooling and solidification of about thirty-eight miles of rock. There is one point of vital importance, however, which has been entirely overlooked; namely, the transferrence of material, as the result of denudation, from the continents to the sea-floors. If, as this theory requires, the continents and oceans are fixed, this action must have been always in the same direction. Now few geologists estimate the thickness of the stratified rocks at less than ten miles; but it will be claimed by those believing in the theory that the sediments must be much thinner over the floor of the central ocean; therefore we will assume five miles as the average thickness for the globe, and allow that they are three-fourths on the continents now. But to restore to the continents what they have lost according to this view would increase the height of the land and the general inequality of the surface at least five miles, which, added to the existing relief of three miles, gives eight miles as the excess of contraction of oceanic over continental areas, corresponding to a crust 100 miles thick. Remembering, however, that according to Professor Dana, the most of this detritus was derived from much smaller continents, say one-half as large, and it is seen that the excess of contraction of the oceanic areas corresponds to a crust over 200 miles thick. It seems no exaggeration, therefore, to say that a clear statement of this part of the theory is sufficient to refute it.

As already pointed out, Professor Dana's theory affords no adequate explanation of the known facts of elevation and subsidence. His writings often appear ambiguous and contradictory upon this point since, though generally admitting the existence of a plastic zone, he often argues as if the continents at least were solid to the earth's centre.

No proposition in geology is more firmly established than this: during the whole of geological time the earth's crust has been subject to extensive and wide-spread oscillations; and, as already pointed out, we know beyond a doubt that these movements are still in progress. Geologists do not now generally believe that the profound subsidences permitting the deposition of thick sedimentary formations are produced by these same sediments; but they rather agree with Professor Dana that the oscillations are due to lateral pressure and go on independently of the sedimentary process. But this view certainly does not harmonize well with the notion that these great vertical movements of the crust are merely local phenomena. On the contrary, all will concede that it is more reasonable to suppose that the area affected is, on the average, roughly proportional to the change of level. Now the subsidence of 40,000 feet in the Alleghany region during Paleozoic time did not make a deep ocean *there*, because deposition kept pace with the downward movement. But where is the evidence that the subsidence was limited to the eastern border of the great Paleozoic sea? Are we not, in accordance with the foregoing, at liberty to conclude that it affected, perhaps in equal measure, the central portions of the sea, where the deposition was only one-tenth as rapid as in the east? To answer in the affirmative is to admit that a large part of the present continent became the site of the abyssal ocean. If, as we believe, the great oscillations of the crust go on independently of deposition, it is certainly strange that they should be limited to the neighborhood of coast lines. Professor Dana admits that the important upward movements of the crust affect extensive areas, as witness the elevation of North America, Europe, and Asia in the Tertiary period, and the elevation of South America at the present time; and no good reason is apparent for denying that the same holds true with important downward movements. The evidence of elevation is, from the nature of the case, abundant and positive; while the evidence of subsidence is meagre and negative; except along the coast-lines where deposition measures the movement. But speculation is unnecessary here, because the coral-islands of the Pacific are monuments of a subsidence which is at once profound and wide-spread.

Professor Dana agrees with most American geologists that during Paleozoic time a considerable body of land lay to the eastward of the present Atlantic coast-line of the United States. But, if it extended very far in that direction — more than one hundred miles — it must be now in part under one to two miles of water. According to Professor Dana and the great majority of geologists, the important movements are necessarily reciprocal, one part of the crust rising as another part sinks; and Professor Dana says farther that the oceanic crust is more flexible and rests on more mobile material than the continental. Why, then, should he, with the certain knowledge of a Paleozoic subsidence of 40,000 feet in the Alleghany region, a Mesozoic subsidence of

50,000 feet in central Europe, and a subsidence in the Rocky Mountain region, according to King, of 60,000 feet, etc., hold that it is extremely improbable that any part of the floor of the deep sea ever has been or will be elevated to form dry land?

Again, what is the basis for the view that all extensive upward movements are confined to the land areas? It certainly is a strange doctrine that, while the stable continental crust is subject to repeated up and down movements of from *one to ten miles*, the (according to Professor Dana) comparatively flexible oceanic crust is only susceptible of slight oscillations, in addition to a slowly progressing subsidence covering the whole of geological time. As Professor Dana has shown, however, the coral islands testify that a large part of the floor of the Pacific has subsided from 3,000 to at least 10,000 feet in quite recent geological times. He also insists that this subsidence is a true downward *bending* of the crust, being due mainly to pressure and not to contraction, that the ocean-floor moves as a unit, and that the entire crust of the of the earth is involved in the movement. But 10,000 feet subtracted from the depth of the Pacific would make it a very shallow ocean; and its islands would be vastly more numerous and larger than they are now.

In fact, the Central Pacific, before the subsidence began, was probably as continental as the major portions of Europe and Asia during the early Tertiary epochs. Now, since this coral island-subsidence is not the result of contraction, what large element of improbability is there in the supposition that it may some day be reversed to the extent of 10,000 or even 20,000 feet? Nearly all the land bordering on the Pacific is rising, and rising probably (as Dana has suggested) as a joint and complementary effect of the same great cause that produces the oceanic subsidence. It is safe to assume, however, that these continental movements will, sooner or later, be reversed; and when that happens will not the Pacific subsidence be almost necessarily reversed too?

The formation of extensive deposits of sediments requires a continent as well as an ocean. So far as our present purpose is concerned we may say that the continents are composed entirely of stratified rocks, there being no igneous rocks except such as have come up through the stratified series. In other words, no part of the primitive or unstratified crust is any where exposed. Furthermore, the old crystalline or Eozoic formations which, according to Professor Dana, formed the first land and the nuclei about which the present continents have been developed, are of enormous thickness. The Eozoic rocks of the Rocky Mountains, Canada, New England, and the southern Appalachians have an average thickness of probably not less than 50,000 feet, and many geologists would say 100,000 feet. Where was the land whose waste afforded the material for building these tens of thousands of feet of strata? It must have existed somewhere. It was probably outside the borders of the modern continents; and it was certainly land whose site was subsequently occupied by the sea.

Thus it is clear that extensive bodies of land, in other words *continents*, were in existence before any part of the land of to-day had appeared above the sea.

But, without pressing farther the question as to how, if the theory in question is correct, the modern continents ever came to have a beginning, let us advance a step and look for the source of the materials composing the subsequent additions to the continents, including the Paleozoic and all later formations. According to Professor Dana, they were derived entirely from the comparatively small Eozoic areas. This, however, means not less than ten, and possibly twenty, miles of erosion, and necessarily implies either that this primitive land had originally an incredible height, or that during the course of geological time it has been constantly renewed by elevation as fast as worn away. But what are we to think of the original volume of formations which could suffer this enormous waste and still have a thickness measured by miles? We could not emphasize more strongly the absolute necessity of extensive pre-Eozoic continents to serve as a source of Eozoic sediments.

If the continents, since their first appearance, have been elevated ten to twenty miles and are still rising, they must rest on a plastic stratum. Thus Professor Dana's theory, when rigorously followed out, leads to the conclusion that the continents are essentially great upward bendings of the crust. A large part of this stupendous elevation must have occurred, if at all, since the early Paleozoic beds were deposited; and consequently they must *also* have suffered miles of erosion, and have been completely removed from large areas which they once covered; so that the Eozoic lands, according to this view, are much wider now than at the beginning of the Paleozoic era. But this only augments the difficulty of finding an adequate source for the post-Eozoic sediments; — a problem which would appear to be especially difficult for Professor Dana, since there are few geologists who now restrict the Eozoic rocks within such narrow limits as he does, extensive formations commonly regarded as Eozoic being referred by him to the Paleozoic.

It is said that all the stratified rocks exposed on the continents are shallow water deposits, and consequently that the floor of the deep sea has never been elevated to form land. This proposition is more easily stated than demonstrated. Among the crystalline sediments, especially, there are many kinds which, for aught that we can now determine, may very well have had a deep sea origin; but the subsequent development of crystalline characters has, in most cases, made it impracticable to trace their histories. There is nothing in our great formations of white crystalline limestone, such as that stretching along the western base of the Green Mountains, to indicate that they are shallow water deposits; and it is simply begging the question to set them down as such. Their purity and uniformity are favorable to the view that they have not been formed near the land.

Over considerable areas of the ocean-floor glauconite or greensand is now accumulating; and, so far as I am aware, the essential identity of this deposit with the great beds of greensand in the Cretaceous and other formations has not been questioned. It frequently happens that the siliceous organisms always present in the deep sea oozes predominate to such an extent as to give character to the deposit, which then becomes a Diatom or Radiolarian ooze; and it is difficult to understand why such a deposit is not fairly represented by the well known diatomaceous, or so-called infusorial, earths of Tertiary age, or even by the hornstones and cherts of the older formations. The Radiolarian ooze has been found in the deepest parts of the Pacific, and nowhere at a less depth than 2250 fathoms.

Mr. Wallace, in his "Island Life," contests the generally accepted view that the chalk is a deep sea deposit and that it is the ancient representative of the modern calcareous or Globigerina ooze found in all the great oceans at depths varying from 250 to nearly 3,000 fathoms. The chief objection which he raises is that the chalk and ooze differ widely in composition; and analyses are quoted to show that the ooze is, on the average, poorer in calcium carbonate, and richer in silica, alumina, and iron than the chalk. In these chalk analyses, however, no account has been taken of the flints, which may be fairly regarded as representing the silica in the ooze, the flints being due to the segregation of finely divided silica which was originally uniformly diffused through the chalk. The silica, therefore, may be at once stricken from the list of differences between the chalk and ooze.

The Cretaceous age closed several millions of years ago, a time long enough to permit considerable changes in the character of the deep sea oozes. The alumina and iron in the Globigerina ooze are chiefly the insoluble residue of the volcanic dust spread every where over the ocean-floor; they form a part of all marine formations, and the fact that they are conspicuous constituents of the calcareous ooze simply implies that the Foraminifera shells accumulate with extreme slowness at the present time. To make the ooze chemically identical with the chalk, we have only to increase the rate of the organic deposition. But Mr. Wallace has already done this for the Cretaceous period; for he shows, first, that the abundance of the pelagic Foraminifera, of the calcareous tests of which both the chalk and the ooze are mainly composed, is, other things being equal, proportional to the temperature of the water; and, secondly, that the Cretaceous seas of Europe were very warm. He conceives that a land barrier stretched from Scandinavia to Greenland, concentrating the Gulf stream and directing it across the site of modern Europe.

Mr. Wallace's explanation of the chalk of Europe embraces propositions that are not easily reconciled. For he insists, and rightly, upon the great purity of the chalk, and yet holds that it was deposited in a shallow and narrow sea, and consequently near large bodies of land. He derives the chalk in large part from the comminution of coral-rock, and yet names only two points in Europe (Maestricht in Belgium and Faxoe in Denmark) where coral-reefs of Cretaceous age may be observed, and refers to no modern coral-reefs where chalk is now forming in this way.

The Oahu deposit belongs to the past and is very small, twenty to thirty feet across and eight to ten feet thick and entirely destitute of Foraminifera. The chalk contains no corals, nor fragments of corals, nor does it shade off at the borders into coarser calcareous rocks composed of broken coral; and in no modern ocean are the coral-reefs entirely converted, as fast as formed, to an impalpable slime or ooze. If all this occurred, as Mr. Wallace imagines, in the Cretaceous Mediterranean of Europe, the force of the waves and currents must have been sufficient to transport the finer débris of the land long distances from shore, but this supposition is negatived by the great purity of the chalk.

Sir Wyville Thomson, from whose reports on the voyage of the "Challenger" Mr. Wallace quotes, and to whom the calcareous ooze analyses were known, evidently failed to discover the great disparity between the ooze and the chalk, for he says:[1] "I imagine, however, that the limestone which would be the result of the elevation and slight metamorphosis of a mass of Globigerina ooze would resemble very closely a bed of gray chalk;" perhaps as closely as it is reasonable to expect, considering the enormous lapse of time between the two deposits.

The truly abyssal deposit of the modern ocean is the red clay which is found at nearly all depths below 2,500 fathoms and, according to Sir Wyville Thomson, covers not less than ten million square miles of the ocean-floor. In the "Challenger" report Sir Wyville expresses the opinion that a deposit of this red clay might come to be very like one of the Paleozoic schists. But, apparently, he did not hold this view long; for at the meeting of the British Association for the Advancement of Science, in August, 1878, he concluded an account of the deep sea clay as follows:

"So far as we can judge, after a most careful comparative examination, the deposit which is at present being formed at extreme depths in the ocean does not correspond either in structure or in chemical composition, with any known geological formation; and, moreover, we are inclined to believe, from a consideration of their structure and of their imbedded organic remains, that none of the older formations were laid down at nearly so great depths — that, in fact, none of these have anything of an abyssal character. These late researches tend to show that during past geological changes abysssal beds have never been exposed, and it seems highly probable that

[1] Voyage of the "Challenger," vol. II, p. 256.

until comparatively recent geological periods such beds have not been formed."

It must have occurred to many of the readers of the paper just quoted that Sir Wyville has failed to exhibit any sufficient reason for abandoning his earlier view that the deep sea clay and ooze are not essentially unlike some of the argillaceous and calcareous rocks exposed on the continents. But it is not my purpose to further contest this point; for, although believing that the deep sea deposits are matched lithologically among the formations on the land, I do not claim that the chalk, for example, is, in consequence, *necessarily* a consolidated abyssal ooze. Although this conclusion is much strengthened by the fact that the chalk does not resemble any shallow water deposit of the present day half so much as it does the Globigerina ooze. Still, nothing is truer in geology than that very similar effects may flow from very dissimilar causes.

But the point that I wish to raise now is embodied in the following question: Are there any deep sea deposits? If, as I believe, this question may be fairly answered in the negative, at least as regards the truly abyssal portions of the sea, then the argument that these sediments are not represented on the continents ceases to have any weight, in fact, it no longer exists. Now nothing has been more clearly demonstrated by the deep sea explorations carried on during the last fifteen years than that the abyssal sediments, and especially the red clay, are accumulating with extreme slowness. Over the red clay areas the dredge brings up large numbers of nodules of very irregular forms varying in size from minute grains to masses weighing several pounds and consisting chiefly of the iron and manganese per-oxides arranged in concentric layers in the matrix of clay, around a nucleus formed by a shark's tooth, or a piece of bone, or an otolith, or a piece of siliceous sponge, or more frequently a fragment of pumice. Sir Wyville Thomson has shown that we have in these nodules, and in some of their nuclei, "ample evidence that this abyssal deposit is taking place with extreme slowness; for the nodules are evidently formed in the clay, and the formation of the larger ones and the segregation of the material must have required a very long time; while many of the shark's teeth forming the nuclei of the nodules and which are frequently brought up uncoated with foreign matter, belong to species which, we have every reason to believe, have been extinct since early Tertiary times. Some teeth of a species of Carcharodon are of enormous size, four inches across the base, and are indistinguishable from the huge teeth found in the Eocene beds." On this point Mr. John Murray, also of the "Challenger" scientific staff, says: "when there has been no reason to suppose that the trawl has sunk more than one or two inches in the clay, we have had in the bag over a hundred shark's teeth and between thirty and forty ear-bones of cetaceans; and we may conclude with great certainty that the clays of these oceanic basins have accumulated with great slowness." He also says: "It is indeed almost beyond question that the red clay regions of the Central Pacific contain accumulations belonging to geological ages different from our own." Again, "the shark's teeth, ear-bones, manganese-nodules, altered volcanic fragments, zeolites, and cosmic dust are met with in greatest abundance in the red clays of the Central Pacific, at that point on the earth's surface farthest removed from continental land. They are less abundant in the Radiolarian ooze, are rare in the Globigerina, Diatom, and Pteropod oozes, and they have been dredged in only a few instances in the terrigenous deposits close to the shore. These substances are present in all the deposits, but owing to the abundance of other matters in the more rapidly forming deposits their presence is masked, and the chance of dredging them is reduced. We may then regard the greater or less abundance of these materials, which are so characteristic of a true red clay, as being a measure of the relative rate of accumulation of the marine sediments in which they lie. The terrigenous deposits accumulate most rapidly, then follow in order Pteropod ooze, Globigerina ooze, Diatom ooze, Radiolarian ooze, and, slowest of all, red clay."

The time since the Eocene, when the large Carcharodons lived, is estimated by geologists at more than a million years, and yet enough clay has not been deposited during this immense period to bury the teeth of this giant shark beyond the reach of the dredge! the rate of increase of the sediment being probably less than one foot, and possibly not more than two or three inches, in a million years.

Now suppose that after a submergence of ten million years the floor of the deep ocean is slowly raised to form dry land. Is it surprising that a bed five or possibly ten feet thick of ferruginous clay containing organic remains similar to those found in shore deposits is not recognized as of abyssal origin, but is completely lost among the miles of marginal sediments composing the new continent? In the ordinary sense there are no abyssal sediments, but we find over these oceanic wastes merely the impalpable dust that slowly settles during the lapse of countless ages from the limpid water of the central sea. The land is the great theatre of erosion and the sea of deposition; but just as there are extensive rainless tracts on the continents where there is practically no erosion so there exist still larger areas of the ocean-floor over which the complementary process, or deposition, approaches the vanishing point. On both land and sea the main field of geological operations is marginal, following the shore line; but nowhere does the earth's crust experience such perfect rest as under the deep sea.

A large proportion of the volcanoes of the globe are in the central portions of the ocean, nearly all the oceanic islands being either volcanic, or consisting of coral-rock resting upon old submerged volcanoes; while of the submarine volcanoes which have never reached the surface we of course know nothing, but it is probable that such exist and possible that they out-number those whose craters are dry land. Now on the land we observe no important exception to the rule that volcanoes are situated upon, or in the immediate neighborhood of, thick deposits of recent sediments — Tertiary or Secondary. And we also observe that in the earlier periods of the earth's history the same law held good.

Are the oceanic volcanoes to be regarded as exceptions to this general law? If so, upon what ground? If not, then the inference is at least probable that the great volcanic archipelago of the Pacific, as well as the numerous volcanic islands in the Atlantic and Indian Oceans, rests upon extensive stratified formations of no great geological age. But the deep sea sediments, as we have seen, are of very trifling thickness, with the exception of the coral-limestone; and this rests upon, and is newer than, the volcanoes. Hence the inference is plain that the floor of the central ocean has been at one time a marginal sea-bottom and the theatre of active and extensive deposition. This means that a large part of the deep sea-bottom has formed not only the shoulders but the dry land of the continents.

If, as those believing in stable continents and oceans virtually claim, the oceanic portions of the earth's crust have been covered since the beginning of geological time with a sheet of cold water, the frigid zone extending, along the ocean-floor, through all latitudes to the equator; and if, during the whole of geological history, deposition has been almost entirely suspended over these vast areas, the sediments of probably not less than a million years being insufficient to cover the teeth of the Eocene shark; then, since the strength and thickness of the earth's crust are, in the main, due to, and are a measure of, the refrigeration which it has experienced, it must be admitted that the oceanic crust is probably very thick and very stiff. That sediments are in general a source of weakness rather than of strength in the crust is the testimony of the ablest students of structural geology; and this proposition forms the basis of the generally accepted explanation of the origin of mountains.

Now, if volcanoes are evidence of anything, they are evidence of weakness in the earth's crust. They prove the presence of fissures reaching down to the plastic zone beneath the crust; and, as we have already noticed, they are, on the land, intimately connected with thick deposits of sediments — with what are generally recognized as weak places in the crust. But it is a logical deduction from the hypothesis here combatted that the numerous oceanic volcanoes do not stand on thick accumulations of sediments — for no deposits of sensible thickness are formed in the deep sea, and that they occur on the strongest, rather than the weakest, portions of the earth's crust — for nowhere are the conditions more favorable for deep and permanent refrigeration of the crust than under the oceanic abysses and, according to Mr. Wallace and Professor Dana, the site of the deep sea has remained unchanged during all the changes of which geology furnishes a record.

Professor Dana says the oscillations of the sea-floor are slight compared with those of the land, the principal movement being a gradual subsidence running through the ages which may be reversed to the extent of a few thousand feet, but never sufficiently to convert the sea-bottom into dry land. Yet this cold, thick, stable oceanic crust, which has never been weakened by thick sedimentary deposits, is an area of wide spread and intense volcanic activity; while the continental interiors which, according to the theory in question, have experienced far greater oscillations of level and are covered by unknown but great thicknesses of stratified rocks, are almost entirely free from active volcanoes.

Volcanoes have burned, and poured out their floods of rock, over nearly all parts of the continents. But all volcanoes are, in a geological sense, short lived; and, ere the sediments through which they reach the surface have become old, their energy is exhausted and the wound in the earth's crust is permanently healed. And there can be little doubt that active, terrestrial volcanoes follow the sea-shore simply because it is there, chiefly, that thick deposits of recent sediments are found. It is a natural inference from these considerations that the volcanoes of Polynesia, for example, are piled upon thick sedimentary formations deposited, perhaps, during the slow subsidence of a great Pacific continent. But, according to Professor Dana, they are quite unlike terrestrial volcanoes, having no necessary connection with sediments and being as old as the earth's crust.

The submarine mountain-ranges are, equally with the oceanic volcanoes, an argument against the immutability of oceanic conditions. Few geological theories are now more generally accepted than the theory that mountains are formed by the horizontal mashing up of thick deposits of sediments. These stratified formations of immense thickness — five to ten miles for most important mountain-systems — can only be formed on a marginal sea-bottom. Hence it is impossible to avoid the conclusion that mountains are of sea-shore origin. But an application of this theory to the submarine mountain-ranges is fatal to the notion that the oceanic abysses are permanent. Yet what warrant have we for supposing that these grand corrugations of the ocean-floor are different in structure and origin from the continental mountain-systems? The supposition that they are composed entirely of volcanic ejectamenta is contrary to all analogy and extremely improbable. No well defined and important mountain-ranges on the land have this composition. While the idea that submarine mountains are original corrugations of the earth's crust, formed, perhaps, before the advent of oceans and stratified rocks, is equally gratuitous and baseless.

As an argument in favor of the permanence of continents and oceans, Mr. Wallace attaches great importance to the supposed fact, first mentioned by Darwin, that, with the exception of New Zealand, and the Seychelles Islands in the Indian Ocean, none of the truly oceanic islands contain either Paleozoic or Mesozoic rocks; the inference being that during the Paleozoic and Mesozoic eras neither continents nor continental islands existed where our oceans now extend, for had they existed Paleozoic and Mesozoic formations would in all probability have been accumulated from sediment derived from their wear and tear.

This argument is not so formidable as it at first appears. Mr. Wallace thinks it is doubtful if New Zealand can be properly called a true oceanic island. But it is difficult to see how it can be differently classified, since the ocean between it and Australia is one thousand miles broad and three miles deep. But there are other exceptions to the law which he formulates. New Caledonia is an oceanic island, over 700 miles of deep water separating it from Australia, while the sea in its near neighborhood has a depth of 15,000 to over 17,000 feet; and yet it is composed of stratified crystalline, Paleozoic and Mesozoic rocks. The Salomon Islands, 500 miles from New Guinea and nearly twice that distance from Australia, are, according to Garnier, composed of rocks similar to those found in New Caledonia. Kerguelen Island, in the southern part of the Indian Ocean, and 2,000 miles from the nearest continent, is certainly a true oceanic island; and yet it is composed, in a large part, of stratified rocks, both fossiliferous and crystalline. The Philippine Islands contain Secondary, if not Paleozoic, strata; and, although only 300 to 500 miles from Borneo and the continent of Asia, they are surrounded on all sides by water from two to three miles deep. Naturalists are generally agreed that the true borders of the continents are not the actural shore-lines, but the lines, sometimes 100 to 200 miles from shore, where the water commences to deepen rapidly and the abysses of the ocean begin. All land beyond this true continental edge is oceanic. Now, judged by this criterion, the Philippines are, apparently, oceanic islands. It is certainly unreasonable to say that all oceanic islands must be remote from the continents. As well might it be claimed that all the higher parts of the continents, or mountains, must be remote from the sea. I have been informed by Prof. Jules Marcou that the Marquesas Islands, lying on the eastern border of Polynesia and near the centre of the Pacific, contain representatives of the older stratified formations. And now we learn through Dr. C. Doetter of Graz that the Cape Verde Islands do not consist exclusively of volcanic rocks, but contain also gneiss, mica and clay slates, and limestones.[1] Paleozoic and Mesozoic rocks are well developed in Spitzbergen, which, it would seem, may be fairly classed as an oceanic island. Again, many oceanic islands have not been examined geologically with sufficient care to justify Mr. Wallace's sweeping and positive statement that, with two exceptions, none of them contain any traces of the older stratified formations.[2]

With the exceptions noted, the oceanic islands are nearly all small, and are composed of eruptive rocks or of coral reefs resting, presumably, upon a volcanic foundation. The oceanic islands are, of course, merely the tops of submerged mountains; and it is only with the highest points of the continents that they can be properly compared. Now, supposing the existing continents were submerged to an average depth of 15,000 feet, what would be the geological character of the land remaining above the sea? Paleozoic and Mesozoic rocks would probably be about as scarce in it as in modern oceanic islands. As a rule the loftiest mountains of the globe are composed of eruptive rocks, and in many cases they are distinct, or even active, volcanoes; although the main mass of every mountain system is formed of stratified, and often of fossiliferous, formations. The volcanic materials usually form but a small part of the whole; but they are the cap-sheaf.

1 Petermann's Mittheilungen, 1883, 72.

2 The report of the German Meteorological Expedition to South Georgia Islands contains the interesting information that the only rock observed on this ice-capped Antarctic land is clay-slate. Even the blocks brought down by the glaciers from the lofty mountains of the interior were all slate. (Nature, 29, 509.) And Mr. T. Mellard Reade (Geol. Mag. I, 225), in commenting on this report, directs attention to the fact that this large remnant of an undoubtedly ancient and extensive sedimentary formation is now a true oceanic island, standing in deep water remote from the continents.

Granting, however, for the sake of the argument, that the older fossiliferous formations would be left above the water in some cases ; if the islands of this class were large, they are fairly represented in the modern ocean by New Zealand, New Caledonia, and Spitzbergen, and, if small, by the Seychelles, Salomon, Marquesas and Cape Verde Islands, and Kerguelen Island. The smaller stratified islands, however, would usually be short-lived, being destroyed by erosion. Volcanic and coral islands, on the contrary, are constantly growing and making good the loss by erosion. Submarine volcanoes suffer no erosion until their summits reach the surface of the water ; and their growth is mainly vertical, since the water must ordinarily prevent the lava from flowing far from the outlet or crater. Consequently, if a continent, the stratified summits of which are high and the volcanoes low, is submerged, the former will be soon swept away by erosion, and the lavas ejected by the latter will be piled up, monument-like, until they reach the surface, when, although erosion checks the upward growth, its ravages are constantly made good by fresh outflows of lava.

In the opinion of the writer, these considerations materially diminish the surprise which one feels on first observing that the oceanic islands are mainly volcanoes or coral reefs. For in no other class of islands do we find those elements of growth which enable them to keep pace with the increasing subsidence and to make good the encroachments of the sea. An active volcano cannot be permanently submerged, and the same is true of a coral island, provided the subsidence goes on slowly enough. In short, nearly all the larger oceanic islands do embrace considerable masses of the older stratified formations; and the fact that the smaller ones do not as a rule is satisfactorily explained by a comparison with the highest points of existing continents, and a due consideration of the facts that small stratified islands would necessarily be short-lived, and if submerged only 100 feet might remain forever unknown, and that the volcanic and coral islands cannot usually be either submerged or worn away, possessing a power of growth which makes them eternal.

If the oceans are permanent, the vast archipelago of Polynesia is, beyond a doubt, one of the most inexplicable features of the earth's surface. As Professor Dana has shown, we know by the coral islands that the depth of the Polynesian sea has increased at least 10,000 feet, or two miles, in comparatively recent geological times. The present average depth of this part of the Pacific is probably not more than three miles. Consequently, when the great coral island subsidence began the average depth was about one mile. Now no one can doubt that the elevation of Polynesia by two miles would bring into existence hundreds, perhaps thousands, of new islands and greatly extend the area of those now existing, probably uniting whole groups into one or two large islands, and thus giving Polynesia the aspect of a larger Malaysia. Mr. Wallace has shown very clearly that the Malay Archipelago, like the West Indian Archipelago, is a vast area of half-submerged continental land. But probably its appearance is no more continental now than was that of Polynesia before the coral island subsidence began.

The western islands of Polynesia are the largest, and parallel with this we have the fact that the subsidence, as indicated by the coral monuments and the heights of the volcanic land, increases eastward; so that it seems entirely an arbitrary matter as to where we draw the line between the two great archipelagoes of Malaysia and Polynesia. Professor Dana has directed attention particularly to the fact that the trend of the whole of Polynesia, and of each of the Polynesian groups, is exactly parallel with the Malaysian trend, which is continued through Australia to New Zealand.

The largest and most western islands of Polynesia, like the still larger islands of the Malay Archipelago, include both the older and newer stratified formations, as well as volcanic rocks. But as we pass toward the east and north the islands not only become smaller, indicating a more profound subsidence in that direction; but the older stratified rocks disappear and the islands are composed entirely of volcanic materials and coral formations.

That Sumatra, Java, Borneo, etc., have been a part of the Asiatic continent in very recent geological times few will question. That Australia and New Guinea have, at an earlier period, been connected with this great Asiatic peninsula is almost equally certain. And that New Zealand, New Caledonia, the Salomon group and New Ireland were, in like manner, once joined to Australia hardly admits of doubt, in view of the fact that they are, in geological structure, essentially identical with the great island continent. Although comprising both the older and newer stratified formations, these islands are largely volcanic, and are bordered by extensive fringing coral reefs. But they are properly a part of the great Polynesian Archipelago, and differ from the equally high, volcanic and coral-girt New Hebrides and Feejee groups mainly in being larger and containing some Paleozoic and Secondary rocks. And beyond the last named groups the islands are still smaller and lower, and barrier reefs predominate, until the volcanic heights entirely disappear and their sites are marked only by atolls and coral islands. Still farther east and north the subsidence becomes too great for even these, and the blank sea alone remains. Thus the continental shade off insensibly into oceanic conditions; and the same argument which makes the larger islands of the Pacific portions of a submerged continent applies with nearly equal force to the multitude of smaller islands.

In this connection it is interesting to consider what would be the aspect of the continent of Asia after a subsidence of between two and three miles. The great plateau of Thibet would still have a considerable elevation above the sea, and, with its bordering mountain ranges, would compare in size with Australia. While stretching away from it in all directions, especially to the east and north, would appear long meandering lines of islands, becoming smaller and lower with the distance. If the sea were warm and the subsidence sufficiently gradual, the sites of the lower mountains and ridges would be marked by monuments of coral formation, and active volcanoes would also keep pace with the subsidence. In short, modern Australasia and Polynesia would be substantially reproduced. Hence it cannot be said that there is anything in the topography of the Pacific islands militating against the idea that they are remants of a submerged continent.

It is generally admitted that the West India islands are submerged portions of the American continent; in fact, the relations of the fauna and flora to those of South and Central America compel us to accept this conclusion. Yet these islands stand, for the most part, in very deep water; depths of from 10,000 to over 18,000 feet being common; and the deepest holes being actually within sight of the shore line. Is not this, then, an instance where continental land becomes the bottom of the deep sea?

Many writers on this subject seem to consider that the dawn of the Paleozoic era was near the beginning of geologic time, and that, if they can demonstrate that continents and oceans have not changed places since that period, the whole question is settled. But this is undoubtedly an erroneous view; and I think we may safely concede that the continents stand now where they did in Cambrian times without admitting their absolute permanence; and of course the latter is the real point in dispute. If we compare the thickness of the fossiliferous with that of the Eozoic formations, or the degree of metamorphism of the Cambrian with that of the Laurentian beds, or the amounts of organic evolution before and since the deposition of the earliest Cambrian sediments, it seems difficult to avoid the conclusion that a very large proportion of the time since the appearance of life upon the globe had elapsed before the dawn of the Paleozoic era. The oldest Eozoic rocks have probably never been seen, and below them come the vast thicknesses of Azoic stratified rocks which it is certain were formed after the appearance of the ocean on the globe and before the advent of life.

Now, if Professor Dana's theory means anything, it means that the continents stood where they now stand during the long Azoic and Eozoic eras as well as during the Paleozoic and later eras. One of the most striking and important facts in historical geology is the profound lithological, stratigraphical, and paleontological break observable almost every where between the oldest Paleozoic strata and the underlying Eozoic. What does this signify, if not that there was a general interchange of land and sea at that time? Before the deposition of the Potsdam sandstone nearly the entire continent of North America, so far as we know, was dry land, and had been dry land for long ages. How far this land extended beyond the present limits of the continent we have no means of knowing. The Potsdam beds rest every where upon old land surfaces.

At the beginning of the Cambrian the land subsided and nearly the whole of what is now North America was covered by the Paleozoic sea. How deep this sea became we are unable to determine with exactness. Along the eastern shore its bottom subsided to a depth of eight miles; and over the central portions the principal sediments were impalpable clays and limestones which might have been formed, so far as we can judge from their lithology and paleontology, in a sea two or three miles deep. This Paleozoic sea covered the major part of the continent till the Carboniferous age — a period estimated by geologists at from ten million to fifty million years, or three times as long as the Mesozoic and Cenozoic eras combined. It is not easy to see how North America, during these millions of years, was any more continental than Oceania is now.

One of the most important questions in American geology is that as to the source of the Paleozoic sediments in the Appalachian region. The volume of these sediments is enormous, measuring 30,000 to 40,000 feet in thickness for a breadth of probably 100 miles and a length several times greater. The Paleozoic shore line lay to the southeast and all this mass of material evidently came from that direction. But the only pre-Paleozoic land now visible between New York and North Carolina is a narrow belt of crystalline rocks varying in width from nothing to sixty or eighty miles. This has been broadened by the erosion of the Paleozoic sediments; though narrowed somewhat by the deposition upon it of the Mesozoic and Tertiary beds on the east. If it never extended beyond the present shore line, it certainly cannot be regarded as an adequate source of the vast piles of Appalachian sediments. Some geologists, appreciating this difficulty, have supposed that this narrow belt of Paleozoic land was renewed by elevation as fast as destroyed by erosion, the elevation required being not less than the thickness of the derived sediments, or eight miles. But eight miles of subsidence on one side of the shore line and eight miles of elevation on the other side implies a pretty flexible crust; and the advocacy of this violent hypothesis by eminent geologists shows how far some of the believers in stable continents are willing to go rather than disturb the ocean floor; although, according to this theory, the oceanic crust is newer and more flexible than the continental. Most American geologists, however, including Professor Dana, have solved this problem of the Appalachian sediments by broadening the belt of Paleozoic land. Professor Dana conceives that it extended as far east as the existing shore line, perhaps beyond it (although on his map showing the land at the beginning of Paleozoic time there appears in this part of the continent only a very narrow, broken strip, lying far within the present continental borders); while others have considered it as of continental extent. As I have before remarked, if it extended more than one hundred miles beyond the present coast line, a part of it now forms the floor of the deep sea.

There is another important consideration, besides the thickness of the Paleozoic sediments, demanding an extension of the Paleozoic land to or beyond the existing border of the continent, and that is the entire absence of Paleozoic rocks on the Atlantic seaboard south of New England. Very plainly, the Blue Ridge belt of crystalline rocks was not, during the Paleozoic era, a narrow strip of land washed by the ocean on both sides, but it formed the western border of the Paleozoic continent. And all analogy requires us to suppose that this Paleozoic land had breadth like the continents of to-day. But this is equivalent to saying that the greater part of it is now covered by the deep sea.

As I have elsewhere pointed out, the submarine contours of the Gulf of Maine show that it must have been once something like the modern Hudson Bay, land-bordered on the east as well as on the west; for a broad submarine ridge or plateau extends over nearly nine-tenths of the distance between Nova Scotia and Cape Cod, forming a nearly complete barrier between the comparatively deep water of the Gulf of Maine and the greater depths of the ocean beyond. If the sea bottom were elevated fifty fathoms, the Gulf of Maine, although still three hundred miles long, and having a maximum depth of 110 fathoms, would be changed from a broad-mouthed bay to an almost completely land-locked gulf. The Paleozoic rocks observable around the Gulf of Maine show that it was in existence in Paleozoic time, when its eastern border probably formed a part of a great Atlantic continent.

At the close of the Paleozoic era the Appalachian sediments yielded to the horizontal pressure in the crust and the Alleghany Mountains were formed. It is plain that when a zone of the earth's crust is thus plicated a subsidence of adjacent portions is a necessary consequence. Professor Dana, holding that the continents are stable and fixed, attributes the folding and crumpling of the strata in mountains in general, and in the Alleghanies in particular, to the subsidence of the sea bottom. But the continents are the portions of the crust lifted above the general level and held in unstable equilibrium, and their tendency to subside is so great as, according to Professor Le Conte, to be almost irresistible. While it would seem equally natural and necessary for the ocean bed to rise when the strain is relieved which holds it down.

Professor Dana regards the floor of the great interior Paleozoic sea between the Blue Ridge and the Rocky Mountains as having been essentially a part of the continent during all the millions of years when it was submerged, and not as having been the bottom of a true ocean. Therefore he could not attribute the Alleghanies to its subsidence. Besides, this sea-floor *rose* when the Alleghanies were made, and all geologists agree that the character of the Alleghany foldings shows conclusively that the pressure producing them came chiefly from the southeast. Hence, Professor Dana, believing that the Atlantic existed with nearly its present outlines during Paleozoic time, says that the Alleghanies were formed by the subsidence of the great arch of the Atlantic. But, as we have seen, he admits that the Appalachian sediments were separated from the Atlantic by at least one hundred and possibly several hundred miles of firm land. In other words, Professor Dana tells us that the sediments deposited in one ocean were plicated by the subsidence of the floor of another and entirely distinct ocean. This is very much the same as saying that the sediments now accumulating in the Gulf of Mexico will some day be compressed and folded by the subsidence of the bottom of the Pacific, Central America and Mexico remaining undisturbed as at present.

The Triassic and other Secondary deposits upon our Atlantic sea-board make it impossible to doubt that a very general and extensive subsidence of the land in this quarter did take place at the time of the Alleghany revolution. Here is positive proof of the subsidence of a very extensive land area, and of land, too, immediately adjoining the Alleghany sediments: but where is there a vestige of reliable evidence showing that the floor of the Atlantic subsided at this epoch, or even that the Atlantic was then in existence?

The Atlantic continent helps us over many difficult points in

American geology, and against it, so far as I am aware, no important arguments have ever been advanced. It provides, in the first place, an adequate source for the Appalachian sediments. It explains the absence of Paleozoic strata from the Atlantic seaboard; and renders intelligible the contours of the Gulf of Maine. Finally, in the subsidence of this continent we have a complete explanation of the structure of the Alleghany Mountains. The pressure was entirely adequate for the work, it came from the right direction, and was delivered at the right point, namely, upon the eastern edge of the new sediments.

If, as it seems necessary to believe, the folding and crushing of the Paleozoic deposits was attended by the subsidence of an adjacent portion of the crust, it is reasonable to suppose that the subsidence was proportional to the crushing. Now the Appalachian foldings and disturbance generally culminate in the Pennsylvania region; and it is both interesting and instructive to observe that the Appalachian belt of crystalline rocks — the Paleozoic land — is lowest and narrowest in eastern Pennsylvania and New Jersey; showing that the subsidence of the Atlantic continent was most profound and extensive in this latitude, submerging all but the actual shore line of the ancient land and producing the great concave curve of the Atlantic coast line between Cape Cod and Cape Hatteras.