ART. LVI.—*A Theory of the Formation of the great Features of the Earth's Surface;* by JOSEPH LECONTE, Prof. Geol. and Nat. Hist. University of California.

[Concluded.]

As already stated, every other theory fails to account for the immense crushing together shown by *plication* and *slaty cleavage.* Many theories take cognizance of this crushing, but in all it is a *subordinate accompaniment* instead of the *cause* of the elevation. Let us examine very briefly some of the more recent theories, and show their inadequacy.

* For a discussion of this law see Baird, in this Journal, vol. xli, March, 1866, pp. 21, 22.

Prof. Hall undoubtedly deserves the thanks of geologists for first strongly drawing attention to the fact that mountain chains consist essentially of immense masses of sediments, much thicker, indeed, than the height of the mountains themselves. His views on this subject form, I believe, an era in the history of geological science. Nevertheless, I think his theory entirely fails to explain the actual process by which mountain chains have been formed, and especially to account for the immense horizontal crushing and plication of the strata. According to Hall's view, as explained by himself and by Sterry Hunt,* the Appalachian chain has been formed as follows: This chain consists of sediments 40,000 feet thick, which thin out as we go west, until at the Mississippi river they are only 4,000 feet. We may regard the whole, therefore, as originally an immense *convex* mass of submarine sediment. By *slow subsidence* of this *convex* mass the upper strata were subjected to horizontal squeezing and thereby thrown into folds. *Continental* upheaval exposed the *still somewhat convex* mass of plicated strata *as a great plateau.* Subsequent erosion formed the ranges and ridges with their intervening valleys. Thus the Appalachian chain, and, in fact, mountain chains generally, become mere fragments of denuded plateaus upheaved by continental elevation only. This, I think, is a brief but fair exposition of the theory.

Now, as Whitney† and Hunt, and Billings, and others have shown, when we recollect the breadth of the Appalachians (at least 100 miles), and therefore the gentleness of the supposed convex curve, the amount of crushing together by subsidence would be inadequate to account for the immense plication. But the degree of inadequacy is, I think, scarcely appreciated. We have already said that slaty cleavage shows in many cases a crushing of 2½ miles into 1 mile. To produce such crushing by subsidence alone, the height of the convex mass would have to be greater than its base. Or even making every allowance for the fact that the area of principal plication is only 20 or 30 miles wide, still the height necessary would be enormous.

Besides, it is certain that the sedimentation was not finished first, and then afterward the subsidence occurred, but these two phenomena went on together *pari passu*; and, therefore, the surface was never convex at all, but nearly or quite horizontal all the time. Subsidence under such circumstances might produce horizontal *tension* or stretching of the lower strata, but could not produce horizontal *crushing* and plication of the upper strata.

* "Some points in American Geology," this Jour., May, 1861.

† Whitney, Mountain Building, p. 101. Hunt, American Geology, this Jour., May, 1861.

In addition to this, after the whole of the Appalachian sediments had been deposited, at the end of the Paleozoic era and immediately before these mountains were formed, the Appalachian region was nearly or quite on a level with the sea, being, in fact, during the Coal period, alternately a coal marsh and an estuary, and therefore *lower than the regions east and west of itself.* The mountain formation was a process entirely distinct from and subsequent to the sedimentation and the subsidence. The whole process seems to have been, first, an immense sedimentation and subsidence going on *pari passu* during the whole Paleozoic era; then, at the end of that era, a horizontal crushing together and folding of the strata, and an upswelling of the whole mass. Hall and Hunt leave the sediments just after the whole preparation has been made, but before the actual mountain formation has taken place; and, therefore, in the language of Dana, "it is a theory of mountains with the mountains left out."

Whitney,* in his admirable essay on Mountain Building, if I understand him aright, thinks plication the result of the subsidence of a mountain axis, previously elevated by other agency. The subsidence of such an elevated axis would indeed produce powerful horizontal thrust, and might therefore produce some plication. But why suppose a *previous* elevation at all, when the horizontal thrust necessary, by his own view, to produce the elevation, would itself produce the plication? Or how high must have been the axis to have produced by subsidence such plication as we often find! Or how was this enormously elevated mass supported? It is evident that the objection to Prof. Whitney's view is precisely the same as to Prof. Hall's.

Mountain chains and mountain ranges are therefore, I think, beyond question, produced by horizontal thrust crushing together the whole rock mass, and swelling it up vertically; the horizontal thrust being the necessary result of secular contraction of the interior of the earth. The smaller inequalities, such as ridges, peaks, gorges, and, in fact, nearly all that constitutes scenery, are produced by subsequent erosion.

I feel considerable confidence in the substantial truth of the foregoing statement of the mode of formation of *mountain chains.* As to the mode of formation of *continents* and *sea bottoms*, I feel less confidence. It is possible that even these may be formed by a similar unequal yielding to horizontal thrust, and a similar crushing together and up-swelling. If so, it would be necessary to suppose the amount of horizontal yielding in this case much less, but the depth effected much greater, than in the case of mountain chains. But, as we find no un-

* Mountain Building, &c., p. 106.

mistakable structural evidence of such crushing, except in the case of mountain chains, I have preferred to attribute the formation of continents and sea bottoms to unequal *radial* contraction.

I wish next to show that this theory of mountain chains *explains* in a satisfactory manner not only the mountain elevation and the phenomenon of plication and slaty cleavage, but also *all the most conspicuous phenomena of mountain chains and of igneous agencies.* The satisfactory explanation of these become, of course, strong evidence of the truth of the theory. The further development of the theory will be best undertaken in connection with the explanation of these phenomena.

(A.) *Thick sediments of mountain chains.* It is a well-known fact, first brought prominently forward by Prof. Hall, that mountain chains are composed of enormous masses of sediments. This fact forms the basis of Hall's sedimentary theory. Prof. Whitney,* it is true, thinks that the sedimentary theorists have mistaken cause for effect,—that thick sediments are not the cause of mountains, but mountain chains are the cause of thick sediments. He believes that a granite axis upheaved out of the sea has furnished by erosion the sediments which have been deposited on their flanks. But when we remember the immense thickness of these sediments and their extent, and the comparative narrowness of the granite axis which furnished their materials, we may well ask what must have been the original altitude of this granite axis! It seems impossible that the granite axis of a chain should have furnished by its erosion the immense mass of sediments involved in the structure of the whole chain. Not only so, but in many chains the strata are not only found on the flanks, but even the highest peaks are stratified. And not only so, but many chains, like the Appalachians and the Jura, have no granite axis at all from which to obtain their sediments. Whitney regards these latter as exceptions, and as always comparatively small chains, and probably formed in a different manner from the great chains with granite axes. My own belief is that all, smaller and greater, have been formed in a similar manner. Mountain sediments, I believe, are not the débris of the *granite axis* of the chain; they are evidently the débris of *continental erosion. Mountain chains*, strata, granite axis and all, *are off-shore deposits.* To state the proposition more definitely: *Mountain chains are formed by the mashing together and the up-swelling of sea bottoms where immense thickness of sediments have accumulated; and as the greatest accumulations usually take place off the shores of continents, mountains are usually formed by the up-pressing of marginal sea bottoms.* We will make this plainer by some illus-

* Mountain Building, &c., pp. 102 and 103.

trations taken from the history of mountain chains in North America.

Appalachians.—The area now occupied by the Appalachian chain was, during the Silurian and Devonian ages, the *eastern margin of the bed of the great interior Paleozoic sea.* During all this time the whole Paleozoic sea, but especially this *eastern margin*, received sediments from a continental mass to the northward (the Laurentian area), and also especially from a *continental mass to the eastward.* Besides the marks of shore deposit found abundantly in the Appalachian strata, other evidences are daily accumulating that the area to the east of the Appalachian chain, left blank in the geological map of the United States in Dana's text book—the so-called primary or gneissic region of the Atlantic slope—is Laurentian, and therefore was probably *land* during the Paleozoic times. The size of this eastern continental mass it is impossible for us now to know, as it has been partly covered by later deposits, and perhaps even partly covered by the sea; but, judging from the quantity of sediments carried into the Paleozoic sea, and especially from the thickness of the sediments (30,000 feet) along its eastern margin, derived probably wholly from this source, it must have been very large.

At the end of the Devonian age, much of the middle portion of the interior Paleozoic sea was upheaved and became land (see Dana's map, Manual, p. 133); and the Appalachian area now became alternately a coal marsh and an estuary emptying into the sea southward. Into this estuary or marsh, during the whole Coal period, sediments were brought from land north, east, and west, until 10,000 feet more had been deposited. During the whole of this time (Paleozoic era), while the 40,000 feet of sediments were depositing, this area—whether sea-margin bottom, or estuary bottom, or coal marsh—slowly subsided, so that nearly the same level was maintained. It was either shallow water or marsh all the time. This is certain for the Coal period, and almost equally certain for the previous periods. Moreover, it seems to be a general law throughout the whole geological history of the earth, that areas of great sedimentation have been also areas of subsidence *pari passu.* The same seems to be true now. Nearly all great river-deltas are slowly subsiding. In fact, in all shallow water deposits, and therefore in all shore deposits, the accumulation would soon cease, and therefore never become thick, but for the subsidence which constantly renews the conditions of deposit. The subsidence of the Appalachian area, therefore, must have been 40,000 feet vertical.

During the Coal period, therefore, the Appalachian region was still nearly on a level with the sea. So far from being a convex plateau, it was a north-east and south-west trough. So far from being a mountain chain, it was evidently lower than the regions east and west of itself. At the end of this period occurred the Appalachian revolution. *The great mass of sediments which had been accumulating for so many ages, with their included seams of coal, yielded to the horizontal thrust*, was crushed together, and folded and swelled upward to a height proportionate to the horizontal crushing. Thus was the Appalachian formed—subsequent denudation has made it what it now is. It is probable that in the process of the up-pushing of the chain (or possibly at a later time) the eastern continental mass was diminished by subsidence.

Sierras.—We have good reason to believe that, at least some portion of the area now occupied by the Rocky Mountains was dry land even during the Paleozoic era. To what extent or what height we do not know. I shall say nothing of the formation of this the oldest portion of the North American Cordilleras, as the history of its formation is little known. I will commence with a considerable body of land which certainly existed in this region at the beginning of the Mesozoic era. Now, during the whole Triassic and Jurassic periods, *the region now occupied by the Sierras was a marginal sea bottom, receiving abundant sediment from a continental mass to the east.* At the end of the Jurassic, this line of enormously thick off-shore deposits yielded to the horizontal thrust, and the sediments were crushed together and swelled upward into the Sierra range. All the ridges, peaks, and cañons—all that constitutes the grand scenery of these mountains—has been the result of an almost inconceivable subsequent erosion.

Coast range.—The up-squeezing of the Sierra range, of course, transferred the coast line farther westward, and the region now occupied by the coast range became the marginal sea bottom. This in its turn received abundant sediments from the now greatly enlarged continent until the end of the Miocene, and then it also yielded in a similar manner and formed the coast range.

Thus I think it quite certain that the places now occupied by mountain chains have been always previous to their formation places of great sedimentary deposit, and therefore most usually marginal sea bottoms, since this is the most usual place for great deposits. In some cases, however, probably in many cases, *the deposits in interior seas or mediterraneans have yielded in a similar manner, giving rise to more irregular chains* or groups of mountains. This may have been the case with some of the more irregular mountains of Europe.

(B.) *Position of mountains along the margins of continents.*—The view that mountain chains are the up-squeezed sediments of marginal sea bottoms completely explains the well-known law of continental form, viz., that continents consist of interior basins with *coast chain rims*. In fact, the theory necessitates this as a *general* form of continents, but at the same time prepares us for exceptions in cases of mountains formed from mediterranean sediments. The view is best illustrated from the American continent, because of the regular manner in which this continent has been developed. Nearly all geological problems seem to be reduced to their simplest terms, and therefore are most easily studied and understood in America.

Prof. Dana, in a paper on "the plan of development of the American continent,"* brings out some grand views on the relation of the heights of coast chains and their position, to the size and depth of the oceans which they overlook. From these formal laws, and proceeding on the hypothesis of a fluid interior, he concludes that sinking sea bottoms, determined by interior contraction, is the force by which continents are elevated. According to him, the sinking sea bottoms, together with the lateral thrust produced by interior contraction, push up the continents, at the same time crumpling up their margins into mountain chains. Such a process might certainly account for coast chains, for their position at right angles to the greatest expanse of ocean, and for their heights and crumplings being in proportion to the size and depth of the contiguous oceans; but the mechanics of the process is, it seems to me, untenable. For observe: this subsidence cannot be gravitative subsidence; for this could not raise continents. It is evidently a concave bending of the sub-oceanic earth-crust pressing on the liquid interior, and through it pushing up the continental crust. Now I have already shown that no stiffness of crust—not even if the crust were several hundred miles thick—could stand such strain over such immense areas. While I admire, therefore, the formal laws of Prof. Dana, I cannot accept his physical explanation.

(c.) *Parallel ranges.*—Whitney, in his essay on Mountain Building, already referred to, has drawn attention to the fact that the celebrated law of Elie de Beaumont, that parallel ranges of mountains are of the same age, so far from being true is nearly the opposite of the truth. Parallel ranges, at least of the same great system, are nearly always successively formed; and I would add *successively formed coastward.* He illustrates this by reference to the three great ranges of the North American Cordilleras, viz., the Rocky Mountains, the Sierras, and the Coast range—and by the several ranges forming the South American Andes. The theory I have presented at once explains this fact, and erects it into a law. It is a necessary result of the theory.

* This Jour., II, vol. xxii, p. 335.

In this connection, I will throw out a suggestion. Attention has been often directed to the truly wonderful submarine ridges and hollows brought to light by the U. S. Coast Survey, as occurring in the course of the Gulf stream, and extending all along the coast from the point of Florida to the coast of New England.* These ridges are truly submarine mountain ranges running parallel with the coast, and to the Appalachian.

7.

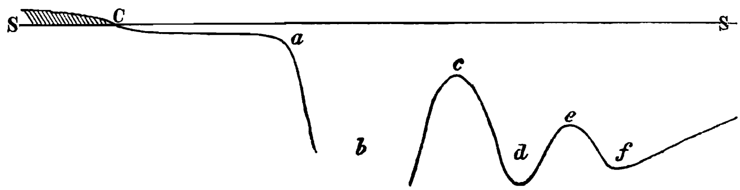

Fig. 7 is a rude section diagram illustrating the submarine configuration. Commencing at Charleston, C, the sea-bed slopes very gradually, so that at the distance of 50 miles (*a*) it attains the depth of only 20 fathoms. From this point it slopes very abruptly, so that it quickly attains unfathomable depth (*b*). At the distance of 100 miles from shore, at the depth of 300 fathoms, is found a ridge (*c*) rising from unfathomable depth on the coastward side, and 1,500 feet above the hollow (*d*) on the seaward side. At the additional distance of 20 miles is another ridge (*e*) 500 feet high, followed by another hollow (*f*) from which the bottom rises gently. The Gulf Stream is parted into three streams by these two ridges. I once (1856) threw out the suggestion that these ridges might be formed by sedimentary deposit from the Gulf Stream itself. I now throw out another, and perhaps a more probable suggestion, in connection with the theory now under consideration. May not these wonderful ridges and hollows be parallel ranges of mountains *now in the course of formation* by the process described? Or else they may be *ranges formed* long ago *on the Atlantic border of the old eastern continental area*, as the Appalachian was formed on the interior basin margin of the same area. In this case, we may suppose they have become submerged in the partial subsidence of this continental area which subsequently took place.

(D.) *Metamorphism of mountain chains.*—From the evidence thus far brought forward, I think it almost certain that mountain chains are formed by the squeezing together and upswelling of lines of off-shore deposit. But the question naturally arises: *Why does the yielding to horizontal pressure take place along these lines in preference to any other?* I believe that the answer to this question is to be found in the recent views

* Prof. Bache, Proc. Am. Assoc., 1854, p. 140.

on the subject of *the aqueo-igneous fusion of deeply buried sediments.*

The accumulation of sediment, as first shown by Babbage, and afterward by Sir John Herschell, necessarily produces a rise of the geo-isotherms and an invasion of the sediments by the interior heat of the earth. From this cause alone, taking the increase of interior heat at 1° for every 58 feet, or about 90° per mile, and adding the mean surface temperature (60°), the lower portion of 10,000 feet of sediments must be at a temperature of about 230°, and of sediments 40,000 feet thick, like those of the Appalachian chain, must be nearly 800° F. Even the former moderate temperature, long continued in the presence of the included water of the sediments, would be sufficient to produce incipient change—at least *lithification*, if not metamorphism. In fact, lithification of sediments will probably take place under heavy pressure even at ordinary temperature, but is no doubt hastened by high temperature. The *latter* temperature of 800° is certainly sufficient to produce not only metamorphism, but aqueo-igneous pastiness, or even complete aqueo-igneous fusion. With a small quantity of alkali in the included water of such sediments, all these changes would take place at much lower temperature.

Suppose, then, sediments accumulating along the shores of a continent: The first effect is lithification, and therefore increasing density, and therefore contraction and subsidence *pari passu* with the deposit. Next, if the sedimentation continue, follows aqueo-igneous softening, or even melting, not only of the lower portion of the sediments themselves, but of the *underlying strata upon which they were deposited.* The subsidence probably continues during this process. Finally, this *softening determines a line of yielding to horizontal pressure*, and a consequent up-swelling of the line into a chain. Thus are accounted for, first, the *subsidence*, then the *subsequent upheaval*, and also the *metamorphism* of the lower strata so universal in great mountain chains. By this view, of course, the exposure of the metamorphic rocks on the surface is the result of subsequent erosion. Even the granite axis, I believe, in *most cases*, is but the *lowermost*, and therefore the most changed portion of the squeezed mass, exposed by subsequent erosion; although it is by no means impossible that in some cases the granite may be *squeezed out* as a pasty mass through a rupture at the top of the swelling mass of strata.

The theory, as will be observed, strongly inclines toward the metamorphic origin of granite, but does not require it. For there is nothing to hinder the aqueo-igneous fusion of an original granite crust by the accumulation of sediments upon it, and the consequent yielding of the crust along the line of accumulation.

(E.) *Fissures and slips.*—The enormous foldings of the strata which must occur in the formation of mountain chains by lateral thrust would, of necessity, produce fractures at right angles to the direction of thrust, or parallel to the folds, *i. e.*, to the range. The walls of such fissures would often slip *by readjustment by the force of gravity;* or else might be *pushed one over the other by the sheer force of the horizontal thrust.* The first case would give rise to those slips in which the foot wall has gone up and the hanging wall down, which are by far the most common slips in gently folded strata. The latter would give rise to those slips, often found in strongly folded strata, as in the Appalachian, in which the hanging wall has been pushed upward. The sudden rupture of the earth's crust, under the accumulating forces tending to bend it, sufficiently account for the phenomena of *earthquakes.*

(F.) *Fissure eruptions.*—The theory may, with much probability, be pushed so as to include volcanic phenomena also. There can be no doubt that the liquid and semi-liquid matters ejected by volcanoes vary in temperature and in kind of fusion in every degree from hot volcanic mud, through all stages of aqueo-igneous fusion, to pure or almost pure igneous fusion. Perhaps all the stages of aqueo-igneous fusion may be accounted for by the invasion of sediments and their included waters by the interior heat of the earth, as already explained. But the enormous temperatures often found in lavas cannot thus be accounted for. But it seems not unlikely, nay, even almost certain, that the invasion of sediments by interior heat would induce slow chemical action, which might increase the heat to almost any extent, so as even to produce true igneous fusion. If these views be correct, then beneath every great line of sediments, such as the off-shore débris of a continent, there exists a mass of partially or completely fused matter. When the line of sediment yields, and the strata are folded and fissured, the underlying fused mass is squeezed into the fissures to form dykes, or through the fissures and outpoured upon the surface as great *fissure eruptions*, which sometimes form the great mass of mountain chains.

(G.) *Volcanoes.*—There can be no doubt, I think, that the foundation of a true scientific geology was first laid by Lyell, in the study of "Causes now in operation." Nevertheless, the assimilation of agencies in previous geological times to those now in operation may be carried too far. As an example of this, I would mention the tendency among the most careful geologists to make our present volcanoes the type of all igneous ejections in all times. But I think no one who has examined the so-called volcanic rocks on this coast, both in the Sierras and in the Coast chain, but especially in the former, can for a moment imagine that these immense floods of lava have issued from craters. The lava floods of the Sierra and Cascade ranges are, it seems to me, among the most extraordinary in the world. Commencing in middle California as immense but separate lava streams, in northern California it becomes an almost universal flood several hundred feet thick; in Oregon the flood becomes universal, and at least 2,000 feet thick, and this continues through Washington Territory and into British Columbia, how far I know not. An area 700 to 800 miles long and 80 to 100 miles wide seems to be almost universally covered with lava, and the thickest part where it is cut through by the Columbia river is not less than 2,000 to 3,000 feet thick. Over this immense area are scattered a dozen or more extinct volcanoes—mere pimples on its surface. It is simply incredible that all this lava has flowed from these volcanoes. There is no proportion between the cause and the effect. I am compelled to adopt the view of Richthofen* and of Whitney, that such great masses of lava, often constituting, as it does in this case, the *chief bulk of mountain chains*, have come, *not from crater eruptions*, *but from fissure eruptions*,—and that volcanoes are only secondary phenomena produced by the access of meteoric water to the still hot interior portions of these great fissure eruptions. Thus, as monticules are parasites on volcanoes, so are volcanoes parasites on massive eruptions, and massive eruptions themselves parasites on an interior fluid mass. This interior fluid mass, however, according to Richthofen and Whitney, is the supposed *universal incandescent liquid interior*, while I believe it is *the sub-mountain reservoir* locally formed as above explained.

By this theory, as by every other theory of mountain formation, it is necessary to suppose that there have been in the history of the earth *periods of comparative quiet*, during which the forces of change were gathering, and *periods of revolutionary change*—periods of gradually increasing horizontal pressure, and periods of yielding and consequent mountain formation. These latter would be also periods of great fissure-eruptions, and these, during the more quiet subsequent period, would be followed by volcanoes gradually decreasing in activity. The last of these great fissure-eruption periods on this coast was the Post-tertiary. The great lava flood which forms the Cascade range, where it is cut through by the Columbia river and its tributaries, *is every where underlaid by the northern boulder drift.*† Since that time we have been in what might be called a crater-eruption period, which was once extremely active but has gradually decreased, until now only geysers and solfataros remain.

* Richthofen, Natural System of Volcanic Rocks; Memoirs of Cal. Acad. Science, vol. i, part 2d.

† I hope soon to give the evidence of this in a separate communication.

I confess I do not see how either volcanoes or massive eruptions can be accounted for, except by the mode now explained. It is now, I think, generally conceded that lavas and other igneous ejections, at the time of their ejection, were in most cases only in a state of aqueo-igneous fusion, and therefore, cannot be regarded as evidences of the interior liquid. It must also be conceded that the focus of earthquakes and volcanoes are too superficial to have any immediate connection with an interior liquid, supposing such to exist. Volcanoes therefore must have their origin either in locally formed accumulations of liquid, as maintained in this paper, or else in local extensions of the general interior liquid, partially or entirely isolated within the solid crust.

In regard to fissure-eruptions, nothing but general contraction and a squeezing out of liquid matter can account for them. Whitney* thinks this squeezing out the result of subsidence of areas on either side of the mountain chain. I confess I do not understand the mechanics of this. Of course it could not be subsidence by weight, for this is inconsistent with the principles of hydrostatic pressure. It could only be by a concave bending of a stiff crust pressing on a fluid interior; but this over a large area is impossible, for the reasons already given in the early portion of this paper. Besides, pressure on a general interior liquid would be propagated equally to every portion of the interior surface of the solid crust, which would therefore yield not necessarily in a contiguous part, but at the weakest point wherever that may be. In fact, if we admit the interior fluidity of the earth, the mechanics of igneous agencies is surrounded with insuperable difficulties on every side. The more we try to arrive at clearness the more the difficulties seem to accumulate.

The theory which I have just presented accounts, it seems to me, for all the principal facts associated in mountain chains. This is the true test of its general truth. It explains satisfactorily the following facts. 1. The most usual position of mountain chains near continental coasts. 2. When there are several *ranges* belonging to one system, the ranges have usually been formed successively coast-ward. 3. Mountain chains are masses of immensely thick sediments. 4. The strata of which mountains are composed are strongly folded, and where the materials are suitable, affected with slaty cleavage; both the folds and the cleavage planes being usually parallel to the mountain chain. 5. The strata of mountain chains are usually affected with metamorphism, which is great in proportion to the height of the mountains and the complexity of the foldings. 6. Great

* Mountain building, etc., p. 90.

fissure-eruptions and volcanoes are usually associated with mountain chains. 7. Many other phenomena—such as fissures, slips, earthquakes, and the subsidence preceding the elevation of mountains, it equally accounts for.

It will be remarked that the theory, though in its general features, not dependent upon, yet strongly inclines toward and is powerfully supported by, the views of Rose, Bischof, Hunt and others as to the metamorphic origin of granite and even of igneous rocks; the view that surface materials have passed by perpetually repeated cycles, through all the stages of rocks and soils; igneous rocks disintegrated to soils, carried away and deposited as sediments, consolidated into stratified rocks, metamorphosed into gneiss, granite or even into lavas, to be again after eruption reconverted into soils and re-commence the same eternal round; and thus we look in vain for the *original material.* I confess I lean strongly to this view.

I am fully aware that there are some phenomena of movement of the earth's crust which are not explained by the foregoing theory. I refer especially to those great and wide-spread *oscillations* which have marked the great divisions of *time*, and have left their impress in the general unconformability of the strata. The last of these great oscillations took place during the Post-tertiary period. I cannot explain these oscillations. I am also painfully aware that the theory just presented, rests upon an insufficient knowledge of the structure of the earth. It is possible that the state of knowledge is not yet such as to warrant any attempt at a general theory. I feel quite sure that a perfect or even a satisfactory theory is not yet possible. I can only hope therefore that the theory here brought out may at least look in the right direction and will therefore serve as some guide in further investigation; that amid the modifications which theoretic geology must undergo in the advance of knowledge, some remnants of its outline will still remain visible. In any case, even if entirely wrong, it is at least a little more definite than anything we have. It is at least something tangible which may be attacked and overthrown by facts and by physical reasoning. We have had enough of vague theorizing in geology; of vague shadows through which the trenchant sword of science passes with no effect. It is time that the more perfect methods of physics were applied to geology.

Oakland, Cal., May 15, 1872.