

ART. XLVIII.—*On the formation of the features of the Earth-surface. Reply to criticisms of T. Sterry Hunt; by JOSEPH LECONTE, Prof. Geol. Univ. of California.*

IN the April number of this Journal, p. 264, Prof. Hunt reviews my paper "On the formation of the great features of the earth-surface,"* criticising *some* points and making reclamation of *others* for himself. In his criticisms he has sometimes misunderstood and sometimes, I think, hardly fairly represented me. In his reclamations, it seems to me, that, in his anxiety to press *yet once more* upon the attention of geologists his own labors, he has mistaken the *use of similar materials* for the *similar use* of materials. That I have used materials similar to those used by himself and many others, I admit; but I have used them certainly in a different way and for a different purpose. Before I commence to show this, however, let me at once acknowledge my deep indebtedness to Mr. Hunt for the many valuable suggestions which I have gotten from the thoughtful and repeated study of his numerous papers. I frankly confess that it was the study of these that first turned my attention to what I now regard as the true *starting point*, and for this I most heartily thank him; but from this starting point we have certainly worked in somewhat different directions, as I now proceed to show.

1. My starting point is the proposition that "the whole foundation of theoretic geology must be reconstructed on the basis of a solid earth." But Mr. Hunt states that this has been his starting point ever since 1858. This is very true. I did not say it was a *new* starting point. I am sure no reader of my paper could imagine that I made any such claim. On the contrary, this starting point is necessitated by a belief in the solidity of the earth, to which belief, I had just previously stated (p. 352), many of the most advanced geologists were fast coming. Among these advanced geologists, I had in my mind Mr. Hunt, but did not think it necessary to mention so obvious a fact. I am sorry, now, I did not. If I deserve any credit at all in connection with this much coveted starting point, it is in collecting and arranging the evidence against the *other* starting point, and in favor of this one, and thus justifying my selection as the only rational one.

2. Again: after leaving the starting point, one of the important steps in my march was *the aqueo-igneous fusion of deeply buried sediments and their consequent metamorphism by the rise of the geo-isotherms*. But Mr. Hunt claims that this, too, he has

* This Journal, vol. iv, p. 345 and p. 460.

been insisting upon for years. Very true, again. I suppose there is no geologist in this country, or in Europe, who is not fully aware of this. Mr. Hunt does not give sufficient credit to the good memory of geologists, or to the importance of his own papers, or he would not have thought it necessary to mention it again. If the idea had been original with Mr. Hunt, and confined to him, it would have been necessary to credit it to him; but it had already become the common property of science; it had been used by Herschel, and Scrope, and Lyell, as well as by Hunt and Hall. Surely, I did not claim originality, for I spoke of it as a well known *recent* view (p. 467); my only fault, then, was not mentioning Mr. Hunt. I am sorry, again, I did not do so, but I can assure him it was only because I regarded his writings as so well known.

3. Again, Mr. Hunt had stated in one of his early papers that the aqueo-igneous fusion of deeply buried sediments "offers a ready explanation of the phenomena of volcanoes and igneous rocks," and he now states that my views are similar to his. Mr. Hunt, it seems to me, has speculated somewhat vaguely on this subject. Sometimes he speaks of a *zone* or *region* of plastic matter separating a solid nucleus from a solid crust (p. 264), through which zone the shrinking nucleus would act on the solid crust precisely as if the whole interior were liquid; sometimes he speaks more truly of *local* masses of plastic matter beneath great *recent* accumulations of sediments. The generation of gases and vapors within the sediments is, according to him, the *force* of eruption. Now, according to my view, a zone of liquid matter would not do at all: the liquid mass is local, and is *squeezed out* by horizontal pressure through fissures, as great *massive eruptions*, often forming the great mass of mountain chains, and must be *local* in order that it should be squeezed out. Volcanoes, I suppose, are parasites on these great out-squeezed masses, produced by the access of water to their still hot interior. Let it be remembered, however, that my subject was a theory of *mountain chains*; my principal object, therefore, was not a general theory of volcanoes, but to explain the *association of both massive eruptions and volcanoes with mountain chains*.

4. I had stated (p. 468) that the aqueo-igneous fusion of sediments determined lines of weakness and, therefore, lines of yielding to horizontal pressure. Mr. Hunt now states that he had previously expressed similar views in similar language. I freely admit this, although I did not know it when I wrote; it had escaped my memory. This is the only sin I have to acknowledge. As far as I know, the idea is original with Mr. Hunt, and I ought to have known it and credited him with it. I now do so most heartily. But observe, my main object was

not the fact itself, but the *use* of the fact in sustaining my theory. All I claim here, therefore, is the connection of this fact with the *position and formation of mountain chains*.

5. I attribute the enormous *foldings* of the strata of mountain chains to horizontal crushing together, produced by the interior contraction of the earth. Mr. Hunt makes reclamation of this also. Let us compare our views on this subject. Mr. Hunt attributes the folding of the Appalachian chain to three causes; (a) Subsidence of a convex mass of sediments; (this I have shown (p. 461) could not take place if the sedimentation and the subsidence went on *pari passu*. (b) Contraction of the strata by metamorphism; (this I suppose could only produce foldings by producing subsidence of the convex surface, and therefore, is subject to the same difficulties as the last.) (c) Horizontal pressure produced by interior contraction. The first, Mr. Hunt says, is probably not the *principal* cause, but I cannot think he regarded the last as a great cause, for *he does not connect it with elevation of the chain*. Now, according to my view, *folding is produced entirely by the horizontal crushing; and indissolubly connected with this crushing together horizontally, is the up-swelling vertically, and the formation of the mountain chain*. Again, Mr. Hunt's Appalachian foldings were going on during the whole process of sedimentation; mine commenced only after the sedimentation was completed. That the latter view is the true one, is proved by the fact that the last layer of the Coal-measures is not only folded, but folded equally with the lower strata. That the folding took place only after sedimentation was completed, is not, of course, original with me: it is the common belief of geologists. All I claim is the *connection of foldings, on the one hand, with horizontal crushing as a sole cause, and on the other with the formation of the chain*.

6. I stated (p. 462), that Hunt and Hall leave the sediments just after the whole preparation had been made, but before the actual mountain formation has taken place, and, therefore, this theory had been characterized as a theory of mountains with the mountains left out. Mr. Hunt, referring to this statement, says that Hall and himself had been much misunderstood by myself as well as by others; that neither he nor Hall ever proposed any theory of mountain formations at all, but only a return to the views of Buffon and Montlosier, that "mountains are fragments of denuded continents."

In order to make my explanation of this point clear, I find it necessary to define my terms. The word *mountain* is loosely used, in scientific as well as in popular language, to express every considerable inequality of the earth surface, from a great mountain chain like the Andes or the Himalayas, to mere hills of circumdenudation like those on the upper Mississippi. The

result has been much confusion of thought. For it is evident that the great bulge which constitutes a mountain chain, and which can be seen only from a distance, is formed in an entirely different way from the smaller inequalities which constitute scenery; the former is evidently produced by general causes affecting the whole earth, the latter wholly by *erosion*. I think therefore, that it is necessary very carefully to distinguish these in our theories. In my own lectures I no longer divide mountains into two kinds, mountains of *upheaval* and mountains of *erosion*, but simply treat the whole subject of mountains under the two heads of *mountain formation* and *mountain sculpture*. All portions of continents, it is true, are sculptured in this way, but this is especially true of mountain chains, which are the great theaters of erosion as of igneous agencies. When I speak of mountain formation, therefore, I mean only the formation of the great bulge or convex plateau which constitutes the *chain*; but when Mr. Hunt speaks of mountains as "fragments of denuded continents," he refers, of course, not to the chain, but to the smaller inequalities, or the effects of *sculpture*. It is certainly one of the great glories of American geology, to have *clearly* shown by the study of the Appalachian chain the immensity of this work of erosion; that not only the smaller ridges and ravines, but great cañons, wide valleys and lofty peaks owe their origin to this cause alone. To Lesley, Hall and Hunt is chiefly due the credit of expounding these views. I confess their writings have been of immense service to me in my mountain studies. But I insist that a theory of these is not a theory of *mountain chains*. The older geologists, it is true, neglected far too much the effects of erosion, and attributed every peak, and ridge, and valley, to upheaval, or fracture, or engulfment; but there still remains the great bulge or convex plateau, the real chain, to be accounted for; for no one imagines *this* to be the result of erosion.

Now it is precisely this convex plateau which, I had supposed, Hall and Hunt attributed to *sedimentation*. I had supposed that they regarded the Appalachian chain as first a great *convex submarine plateau* produced by a line of sedimentation; then this raised into a *convex mountain plateau* by continental elevation; and finally this plateau sculptured into its present forms. I admit that all of this is not clearly and definitely expressed, but when I attempted to formulate their views clearly I could arrive at no other result. Others, I believe, have arrived at a similar interpretation of their views. Under the impression that this was their view, I proceeded to show that it certainly was not true, since the Appalachian region at the end of the Paleozoic era, and immediately before the formation of the chain, was not a *convex* plateau of any kind, but a *concave*

trough, and the whole bulging took place afterward by the crushing together of its strata. But now (if I understand him aright, for he is still not very clear) Mr. Hunt says that the Appalachian plateau or chain was formed by the same unknown process by which the continent was elevated; that it was formed by continental elevation, which from some unknown cause *was greater in the Appalachian region*. I wish much he had clearly expressed this at first; it would have saved much useless discussion. I confess, however, I can not find anything like this in his previous papers or in the writings of Prof. Hall. In the early presentation of a difficult subject, however, some want of clearness is pardonable.

7. According to my view, *foldings* are a necessary concomitant of mountain formation; but Mr. Hunt, p. 267, thinks both cleavage and foldings are mere *accidents*, unnecessary to mountain structure; and he cites examples of mountains on the upper Mississippi composed of *perfectly horizontal strata*, and of Catskill mountain composed of *nearly horizontal strata, uncomplicated with foldings*. I could add other examples from my own observations on the Sierra chain. Mt. Dana, a magnificent peak more than 13,300 feet high, on the very crest of the Sierras, is composed of strata which seem to be perfectly horizontal. But this is no objection to my theory; it is only an example of the confusion of thought of which I speak above. The explanation of the difference between mountain *formation* and mountain *sculpture*, between mountain chains and so-called erosion mountains, completely answers this objection. I was speaking of *chains*, not of isolated *peaks*. Mountain chains are, I believe, always folded; but in the extensive erosion of the *wide* folds of thick strata it would be strange indeed if *no* portions where the strata were horizontal were left as peaks. This is the case, doubtless, with Catskill and Mt. Dana. As for the erosion hills of the upper Mississippi, the explanation is still simpler. There is no mountain chain there at all; these hills are only fragments of denuded continental strata.

8. I hold the view (p. 464) that during the Paleozoic era there existed a continental mass, probably of considerable extent, to the eastward of the Appalachian region. Mr. Hunt again makes reclamation for himself and others. Surely there was no necessity for reclamation. If I had imagined that this was new, I would have devoted a whole paper to it, not have dismissed it in a single very short paragraph. But I certainly do not leave this point doubtful; on the contrary, I say "evidences are daily accumulating" on this point; not by *my* labors; for I was on the Pacific coast; but by the labors of others. I of course referred to the very evidence which Mr. Hunt mentions, but did not think it necessary to mention names in connection with facts so

well known. If I deserve any credit in this connection, it is in giving *something more of definiteness to the conception and especially in showing its connection with the formation of the Appalachian chain.*

9. In the last paragraph of my paper, I acknowledge my inability to explain 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 being that of the Post-tertiary period." After quoting this, Mr. Hunt goes on to say that it is precisely this *upward* movement which *constitutes the continental elevation of Montlosier, Hall and himself, and which give rise to plateaus* by the erosion of which are formed mountains. And since I regard plateau-formation the only true mountain formation, by my own admission (so argues Mr. Hunt) mountain formation is still unexplained. Thus Mr. Hunt makes me, after writing a paper to explain the formation of mountain chains, in the last paragraph acknowledge that it is inexplicable. I wonder Mr. Hunt did not rather suspect that he had entirely misunderstood me.

It was not *upward* movement or *downward* movement which I regarded as inexplicable, but *oscillation* or movement *upward and downward alternately in the same place.* All the causes of movement of which I had previously spoken would *continue to act in the same direction*, and therefore the continents ought to grow higher and larger and the sea-bottoms deeper. On the whole, this has probably been the case throughout the geological history of the earth, as has been so beautifully shown by Dana for the North American continent. The recent observations on the wonderful *persistence of deep-sea fauna and therefore of deep-sea conditions*, through many geological periods, go far to confirm this view for the sea-bottoms. Previously geologists had mostly regarded the earth's crust only as *oscillating*; Dana showed *gradual development* or evolution of continents in the midst of oscillation. Now I have attempted to explain the *development*, but acknowledge my inability yet to explain the *oscillations.*

10. Finally, Mr. Hunt criticises, and perhaps justly, my views as to the chemical cause of the intense heat so often found in lavas. Since the publication of Mr. Mallet's paper, I much prefer adopting his views on this subject. I now, therefore, regard the process of mountain formation to be briefly as follows: lines of thick sediments, rise of geo-isotherms and aqueo-igneous softening determine lines of yielding; then crushing together *horizontally* and swelling up *vertically* forms the chain; but once the yielding commences, then *mechanical energy is changed into heat*, which may thus be increased to any amount and produce true igneous fusion.