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GEOLOGICAL RESULTS

OF THE

EARTH'S CONTRACTION

IN

CONSEQUENCE OF COOLING.

BY

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GEOLOGICAL RESULTS, &c.

There are few geological writers at the present day who do not admit the former igneous fluidity of our globe.* In this belief they recognize the fact that the earth has undergone contraction as a consequence of cooling, and acknowledge a readiness to receive as geological truth, whatever may be shown to be the natural effects of such contraction. Yet why, after attributing to this cause, in a general way, much of the unevenness of the earth's surface, should the subject then be dropped, as if no such cause had operated? It is certainly of the highest importance that an agency so universal and so fundamental in its nature, should be followed out in all its bearings to the very limits of its possible effects.

* As matter of history, and on the principle also of "honor to whom honor," we cite here the following passages from the Protogæa of Leibnitz, written in 1691, giving his views respecting the origin of the saltness of the sea, and the formation of mountains and of rock strata.† On the first point he offers the true explanation; and although his views on the other points require some modification, they exhibit the wonderful depth and penetration of his mind. Alluding in the outset to an original state of igneous fluidity, he says (§ iv):—

"Ex hac *genesi rerum* jam observata hactenus procedet *salsi maris origo*. Nam ut perusta, ubi refriguere, humorem attrahunt, unde olea per deliquium Chemicis nascuntur in cella; ita pronum erit credere, sub rerum initiis, nondum separato a luce opaco, *cum globus noster adhuc arderet*, pulsum ab igne humorem abiisse in auras, deinde vero destillationum exemplo renatum, mox remittente æstu in aquosos vapores iterum fuisse densatum, et cùm a congelascente terrestris superficiei massa resorberetur, in aquam denique rediisse, quæ terræ faciem abluens vasta recentis empyreumatis vestigia, salemque fixum in se recepit. Unde natum est *lixivii genus*, quod deinde in mare confluxit."—"Postremò credibile est, contrahentem se refrigoratione crustam, ut in metallis, et aliis, quæ fusione porosiora fiunt, *bullas* reliquisse, ingentes pro rei magnitudine, id est, sub vastis fornicibus *cavitates*, quibus inclusus fuit aër humorve; tum etiam in folia quædam discessisse, et varietate materiæ calorisque *inæqualiter subsedisse massas*, quin et *dissiluisse* passim, fragminibus in *declivia vallium* inclinatis, cùm partes firmiores, et velut columnæ, supremum locum tuerentur: unde jam tum *montes* superfuere. Accessit pondus aquarum, ad alveum sibi parandum in molli adhuc fundo. Denique vel pondere materiæ, vel erumpente spiritu, fracti fornices, maximæque, humore cavitatibus per ruinas expulso, aut sponte montibus effluente, secutæ inundationes, quæ cùm deinde rursus sedimenta per intervalla deponerent, atque his indurescentibus, redeunte mox simili causa, strata subinde diversa alia aliis imponerentur, facies teneri adhuc orbis sæpius novata est. Donec quiescentibus causis atque æquilibratis, *consistentior* emergeret *status rerum*. Unde jam *duplex origo* intelligitur firmorum corporum; una, cùm ab ignis fusione refrigescerent, altera cùm reconcrescerent ex solutione aquarum. Neque igitur putandum est *lapides ex sola esse fusione*. Id enim potissimum de prima tantùm massa ac terræ basi accipio; nec dubito, postea materiam liquidam in superficie telluris procurrentem, quiete mox reddita, ex ramentis subactis ingentem materiæ vim deposuisse, quorum alia varias terræ species formarunt, alia in saxa induruere, e quibus strata diversa sibi super imposita diversas præcipitationum vices atque intervalla testantur." Here we have in general terms the just mean between Wernerism and Huttonism attained long before either Werner or Hutton lived.

Again, he remarks as follows, after some explanations, on the elevation of mountains, § xxii:—"Ego ut facilè admittam, initio cùm liquida esset massa globi terræ,

† An abstract of this passage is given by Lyell in his Principles of Geology, and with illustrative remarks by Conybeare, in the Rep. Brit. Assoc., 1832, p. 366; also a brief abstract of the Protogæa by Prof. E. Mitchell, may be found in this Journal, xx, 56, 1831.

Among English geologists, the subject has received little attention except in the writings of De la Beche; and in the Treatises on Geology in our own language the absolute rising and sinking of the continental lands and the stability of the waters are usually set down as established truths.* In this country, Prof. W. W. Mather† has made the theory of "secular refrigeration" a subject of special consideration: and an account of its supposed bearing on the magnetic variation of our globe and on the tides, has been published by Prof. J. H. Lathrop.‡ In the Geological Society of France, this theory of a cooling globe has been a frequent subject of discussion, owing perhaps, in a great degree, to the attention called to the subject by the elaborate mathematical essay of Cordier.§ M. Elie de Beaumont, the great champion of "soulèvement" theories, appeals to contraction to explain the direction and

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luctante Spiritu superficiem variè intumuisse, unde illi mox indurescenti primæva inæqualitas; neque etiam diffitear, firmatis licèt rebus, terræ motu aliquando vel ignivomâ eructatione, monticulum factum. Sed ut vastissimæ Alpes ex solida jam terra, eruptione surrexerint, minus consentaneum puto. Scimus tamen et in illis deprehendi reliquias maris. Cùm ergo alterutrum factum oporteat, credibilius multo arbitror, defluxisse aquas spontaneo nisu, quàm ingentem terrarum partem incredibili violentiâ tam altè ascendisse." In § vi, he explains the oscillation of the water and land by supposing the existence of great arched cavities ("fornices," obviously considered as a result of contraction on cooling,) which were afterwards broken. He says: "Ita priore rupto aqua in montes ascenderit, mox, posteriore fracto in abyssum ulteriorem penetrarit, terrestribusque habitatoribus iterum indulserit in sicco locum verisimile est." There is certainly some approach to the views we advocate, in his rejecting the idea of a bodily lifting of mountains by force beneath, and also in the suggestion that oscillations were produced in the water level by subsidences, though we know nothing of his "fornices."

* De la Beche, one of the profoundest geologists of the age, appeals to contraction for the production of fissures, depressions, and elevations by lateral action; he also considers fractures and elevations to form from matter struggling to free itself, from earthquakes of great intensity or from elastic forces acting beneath. Lateral pressure is attributed to contraction, but in the case of the Alps and other cases mentioned, to the extrusion of material from below.

We observe in the memoir by Prof. Sedgwick, on the Cambrian Mountains, (Geol. Trans., ii Ser., iv, 47, 1833,) the following remark: "As the earth has apparently diminished in temperature, we have a right to look for some indication of a contraction in its dimensions. May not some of the great parallel corrugations of the older systems of strata have been produced by such a partial contraction?"

† See this Journal, xlix, 284, 1845. Prof. Mather, in his valuable paper, attributes changes of level to contraction causing depressions and elevations and lateral displacements, to "a subterranean force tending to elevate parts of the earth's surface," to waters gaining access to opened fissures, and to paroxysmal variations in the angular velocity of the earth, the latter causing paroxysmally a westward motion in the internal fluids, and through them, the same motion in certain parts of the crust of the globe, which are consequently dislocated and folded.

‡ This Journal, xxxviii, 68, and xxxix, 90. Prof. Lathrop, (now President Lathrop, of the University of Missouri,) besides recognizing the general effect of contraction in causing a change of water level, endeavors to prove that the fluids of the interior have a slow westward movement, correspondent with the change of magnetic variation, and also a tidal motion, which acting on the crust is a cause of the marine tides.

§ Essai sur la Temperature de la Terre, 4to, pp. 84. Read before the Academy of Sciences, June 4, and July 9 and 23, 1827. Also, the same, translated by the Junior class in Amherst College, 1 vol., 12mo, 94 pp.: Amherst, 1828. See also an abstract in this Journal, xv, 109.

origin of mountain chains, and the same view is adopted by M. Omalius d'Halloy and others, who appear to consider no farther the results that may flow from this cause. MM. Leblanc,* Angelot,† Roys, and Rozet,‡ reason more freely upon the subject, and derive from the theory explanations of volcanic and other phenomena. Prevost§ has the credit of priority in many principles adduced, and of greater precision and comprehensiveness in his deductions. Cordier alluded only in general terms to dislocations from contraction. Prevost shows not only that the cause should produce displacements, but points out ways in which these displacements should take place; and he concludes that the

* Leblanc (Bull. de la Soc. Geol. de France, xii, 137, 1841) endeavors to show by calculation, what are the effects of this contraction in depressing certain parts of the crust and swelling others, the swelling producing, as he argues, void places below and fractures of the crust; he remarks that the largest depressions thus formed are the oceans, while the folds are the mountains, and the plains are the parts "qui correspondent aux exhaussements lents qui ont dû precéder des rides."

† M. Angelot reasoning upon similar grounds, accounts for volcanic action by supposing that the void spaces produced by contraction become filled by water, which water feeds the fires, and may be at times a source of earthquakes and of much metamorphic action, as for instance the production of serpentine. (Bull. Soc. Geol. de France, xi, 178, 245, 1840; xiii, 377, 400, 1842; xiv, 43, Nov. 1842.) The hypothesis of the existence of such void spaces is opposed by M. Roys, and others. (Ibid., xiii, 238, 249.) M. Angelot quotes Bischof's investigations, in Leonhard und Bronn's N. Jahrbuch, 1841, pp. 565, 566, which show that granite *contracts a fourth* of its volume on cooling from a liquid state, trachyte *a fifth*, and basalt *a tenth*, or respectively in decimals, 0·7481, 0·8187, 0·8960. The *lineal* contraction of granite is hence *one-tenth*.

‡ Bull. de la Soc. Geol. de France, xii, 176, xiii, 175, 1841, 1842. Rozet agrees with Cordier with regard to volcanic eruptions; but he attributes some of the great geological changes to a change in the earth's axis of rotation.

§ Prevost's views have been presented in various discussions before the Geological Society of France during the twenty years past, but are most fully detailed in volume xi, of the Bulletin, pages 183 to 203, March, 1840, from which we cited his general deductions in the last volume of this Journal, page 355. The "*Elevation*" theory of craters, which constitutes a part of the views opposed to his theory, is also discussed in the same place, and in the volumes preceding, and following. To give a more just exhibition of his views, and that he may not be charged with any modifications of them, or peculiar deductions, for which the writer alone is responsible, we give here a translation of several paragraphs from his memoir.

"If a cause analogous to that which, according to the theory of *Elevation*, (soulèvement,) is supposed to have raised the Alps or Andes, should elevate the bottom of the South Seas and cause a continent to appear above the waters, what effect would this event have upon the land? It is evident that a quantity of water equal to the volume of the submerged part of the new continent would be thrown over the shores of America, Asia and Europe, and after the oscillations had ceased, parts now dry would remain submerged.

"But passing from these suppositions and reasonings to actual geological facts, do we not observe over all lands, continents as well as islands, ancient marine beaches and thick deposits of marine origin, which have been left dry and still preserve their normal position? The general level of the waters has then been lowered; and in order to this effect, either the waters have diminished, (which few will suppose,) or else in consequence of displacements in the earth's crust, the depressions formed are much more considerable than the elevations.

"If upon all shores, from New Holland to England and Iceland, both of islands and continents, and on the banks of rivers, we recognize undeniable marks of a previous water level at different heights, all nearly parallel, it is very difficult to attribute the successive elevations of such extent, to an absolute elevation of the surface, the different parts of which surface retain the same relative positions as agency of contraction alone, without the causes of "soulèvement"* usually appealed to, will account for the various changes of level which the continental areas have undergone. He rejects the idea of an *elevating* force which can raise mountains or continents, except such as is incidental to contraction. The principal points in Prevost's theory have already been presented in the preceding volume of this Journal, on page 355; and below we have given in a note a part of his explanations.

The reader will perceive that although the main principles of Prevost are sustained by the writer in this and his former paper, the manner in which these principles are carried out, is in some respects a little different, especially in the idea that the *oceanic areas* have been the more igneous parts of the globe, and for this reason have contracted most;† that certain orographic changes over the continents are due to contraction beneath the oceanic regions, and that the fissurings and mountain elevations have for this reason taken place in some instances near the margin of a continent, or near the limit between the great contracting and the non-contracting (comparatively non-contracting) areas. The efficiency of the cause of contraction has appeared to the writer to be wider and more evident, as the subject has received closer attention; and the study of it has naturally led to modifications of former views.

The theory if true does away with the most incredible of geological dogmas,—the idea of a force acting beneath the continents which can raise them bodily with their load of mountains. The mind unprejudiced naturally asks, where does this force reside? and how does it act? What fills the void left by the raised continent? Why, after an earthquake has passed, should not a mass of rock as large as the Andes and half of South America, sink back again to its place?

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before the change of level. If, on the other side, we view as submerged all the parts of existing continents and the islands on which marine deposits occur in horizontal position; if we place beneath the water the great part of the points of the surface now existing as mountains, whatever is supposed to have *risen* since the formation of these marine deposits, we cannot but see that there would be no place for vegetation or terrestrial animal life, none for the great lakes with their freshwater animals and plants, and none for the rivers, the remains of whose numerous organic productions are met with in ancient deltas.

"Are we not then forced to admit that while the bottom of the sea has been raised above the level of the sea and made dry land, by a series of displacements, still larger terrestrial areas have disappeared from submergence; and in such a way that the depressions formed were greater than the elevations, a condition without which, I repeat it, the low parts of our existing continents could not have been emerged, a condition requiring for its fulfillment, no aid from the supposed agent of "soulèvement," since this would produce a contrary effect."

* The word "*soulèvement*" in French has a force which does not belong to our English word elevation, as it implies an upheaval from force applied *beneath*.

† Lyell translates and cites the following from Strabo: "We must therefore ascribe the cause [of changes of level] to the ground,—either to *that* ground which is under the sea, or to that which becomes flooded by it, but rather to that which lies beneath the sea, for this is more movable, and on account of its humidity, can be altered with greater celerity." (Principles of Geology, vol. i; Strabo, Geog., lib. 1.)

It is said that waters gain access below, and by a sudden expansion to a state of vapor, the land is thrown up: but, again, why should not the vast weight cause it to sink back as the vapors are condensed? Surely an injection of liquid lavas into any cavities or opened fissures—a material that cools with such extreme slowness—would be a poor support for a chain of mountains. Is the water to gain access through opened fissures? but it would meet molten material rising from below to fill the fissure, and how then could the water thus intercepted make its way, in any large body, *under* the crust, so as to *lift* the surface into mountains? How can such an elevating force get beneath when there is no "beneath" to the fluid column short of the antipodal crust?* The expansive force of contained vaporizable substances pent up in the liquid interior, can be a no more effectual cause; for it does not appear that such a force can act against the incumbent pressure, except by making the lavas somewhat lighter and causing them to swell up into an opening; it can give no eruptive force to the igneous fluid.†

It is urged again that the crust below may possibly be acquiring heat from the internal fires, so as to become elevated by expansion. But there is little to satisfy the mind in this assumed *possibility*, especially when it is considered that through past times the elevation of the land has been on the whole increasing, and yet facts and reason evince that there has also been a gradual cooling below and a thickening of the crust. With such a theory we should have, therefore, the incongruity of an average *increase* of heat through past ages, and a cooling of the crust, that is, a *diminution* of heat, going on at the same time.

If after all, we can account for facts without calling upon any special force for lifting continents;—if this effect may be a simple result of contraction, we are relieved of many improbable assumptions. We can well conceive that fractures should take

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* We have not alluded here to the reservoirs of water, which, according to M. Angelot, have been so important a means of elevations and metamorphism, because we see no evidence that such cavities exist. The slow cooling and consolidation within produce a gradual thickening inward of the crust; and no cavities of much size would form till the crust was too thick to yield to the tension by fracturing; and this is a condition which, possibly, is yet hardly attained, for the crust, even if a hundred miles thick, is relatively less than a fourth the thickness of the skin of an orange.

† Speaking of lava in a crater, Prevost says significantly, (loc. cit. p. 188,) "La lave n'est donc pas plus *soulevée* par une force qui serait placée sous l'extrémité inférieure de la coulonne qui s'élève, que la mousse de la bière n'est soulevée par le fond de la bouteille." Again, speaking of volcanic mountains, he says with some humor and with truth, "Il ne faudra pas dire que ces masses sont soulevées, pas plus qu'on ne dit que la *pâte de froment*, qui a été pétrie avec du levain, et qui *lève*, est *sous-levée*." In a note to page 96 of this volume it was incorrectly implied that Prevost attributed all ordinary volcanic eruptions to the collapsing of the earth, consequent on contraction; on the contrary, he recognizes the influence of the process of tumefaction ("boursouflement") in volcanic operations.

place as a consequence of contraction below a stiffened crust; we know them to be a necessary effect. We see also that depressions would somewhere follow a fracture, and the lateral pressure exerted would be likely to dislocate, often raising and necessarily propping or supporting as it raised. We understand that such fissurings, whether internal or external, would cause shakings of the earth (*earthquakes*) of great violence, and in all periods of the earth's history, and it might be over a hemisphere at once. We comprehend too how the continued contraction of vast areas like the oceans would draw off the waters from the land; and by the several combined effects of the cause under consideration, oscillations in the water level would take place. These effects have been briefly stated in a former article in the American Journal of Science.* The theory appears to us to be more worthy

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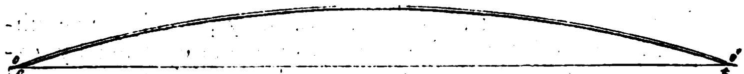
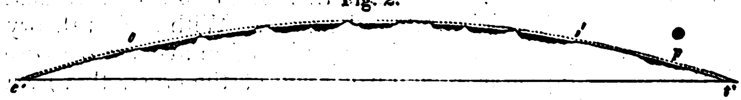
* Vol. iii, ii Ser., p. 95.—The principles may perhaps be rendered more clear by means of the following figures. In fig. 1, the crust (*ct*) is represented covered with

Fig. 1.



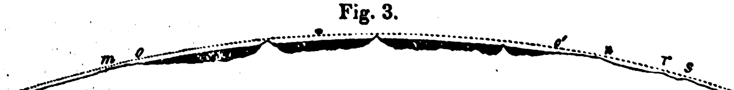
water (*oo'*). In fig. 2, the globe has contracted from the dotted line to *c't'*; *c'o*, *o't'*, are the portions free from volcanic action, (as was the case almost entirely with the parts corresponding to the continents in the Silurian period;) *p* is an area of wa-

Fig. 2.



ter upon *o't'*. *oo'* represents the incipient oceanic depression, over which, owing to its igneous character and thinner crust, (this Journal, ii, 352,) contraction went on the most rapidly, and where, at the same time, igneous ejections and displacements (which result from contraction beneath the crust, causing a drawing down of the crust upon a diminishing nucleus) were frequent. It is evident that the depression would at first be too shallow to contain all the water; but as subsidence proceeded, and most rapidly over the oceanic areas, the capacity of the cavity would increase and tend to drain the forming continent. This result might, however, be long delayed by the eruptions and upliftings throughout the area *oo'*, an effect which would diminish the capacity of the oceanic basin, and so compensate for the contraction going on. The land would finally emerge; but the same causes (eruptions and upliftings over the oceanic areas) might make the water rise over it again, and occasion for ages, successive submergings and emergings of the continents. Temporary cessations of subsidence over the oceanic areas might take place from increasing tension preceding a paroxysmal relief by fractures, and this would be another cause of a rise and fall in the water level.

Fig. 3.



As the crust below the oceanic depression becomes thicker by cooling, the contraction, not now causing fractures and upliftings over its own area alone, would produce a tension laterally against the non-contracting area and occasion pressure, fissures, and upheavals; and thus, in connection with contraction beneath the continental part, the elevations *m*, *n*, *r*, *s*, fig. 3, would result. From this figure, the fact will be appreciated that the amount of effect, claimed for of favor the more closely it is applied, and we would fain believe that the following explanations will be found to secure it some additional attention.

I. *Folding of Strata.*—In our last article on this subject, allusion was made to the foldings of the Appalachian strata, and from the fact that the plications were more abrupt, and the effects of heat more decided, towards the ocean, and also in view of the correspondence observed with analogous facts on the Pacific side of the continent, it was urged that the foldings resulted principally from a subsidence of what is now the oceanic part of the earth's surface. But the peculiar features of the folds present points for consideration which the theory, if true, should explain. We refer again to the admirable paper of the Professors W. B. and H. D. Rogers on the Appalachian chain for the details of the structure there presented.† These geologists have shown that the folds or plications are many and vast. Towards the southeast they are as closely compacted as is in any way possible, so that the annexed figure 4 is given as a just representation of it. To the northeast, the undulations become more and more gentle.

Fig. 4.

S.E.

Fig. 5.

Cacapon Mountain.

The following outline figure, though having too few folds, (fig. 6,) will present some idea of their extent, and (in connection with figs. 4 and 5,) shows the characteristic forms of the plications, as ascertained by these able geologists.

Fig. 6.

S.E. N.W.

From the Southeast across the Appalachians to the Northwest.

Though the regularity is somewhat exaggerated, the general facts are not so. The surface of the country has since been

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lateral action is of no improbable magnitude; for the height *n*, though so slight on the scale here given—a diameter of a foot—actually corresponds to a mountain twelve miles in altitude above the sea. It could not well have been made less on the scale adopted. The reader can judge how small an elevation would represent the average height of the continents above the sea; for this height, according to Humboldt, is somewhat less than a thousand fet, or, on the scale in the above figures, about a seventieth part of the elevation *n*.

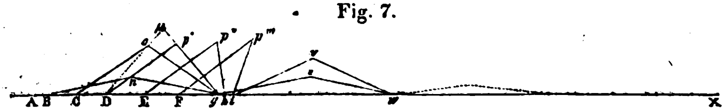
† Trans. American Geol. and Nat., p. 474.—We owe to these geologists, in addition to their exposition of the system of curves in the Appalachian strata, the fine generalization that the *southeasterly* dip in the metamorphic rocks through the regions east of these mountains, has arisen from a close plication of the strata as illustrated in fig. 6. It is apparent from this figure that the strata of both slopes of the fold dip alike to the southeast, as shown also in figure 4.—See this Journal, xliii, 177; xliv, 359; xlv, 341, 346; xlvii, 276.

greatly denuded, and has almost wholly lost the wave-like features, which are so distinct in the stratified beds of rock.

The principal peculiarity of these plications to which we would now ask attention, is the following;—*the greater abruptness of the northwestern slope of each fold, in connection with the diminution of the undulations to the northwestward:* and it will be our endeavor to show that *this peculiarity, and the irregularities which exist, are necessary results of the action of a force laterally exerted.**

This point may hardly require a formal demonstration; yet as other explanations have been offered, we propose to present it in brief detail. In the following figure the folds are represented for convenience of illustration, angular at summit.

Fig. 7.



Let AX represent a bed of stratified clay and sand, in alternating layers, say a thousand feet thick and many miles long; the material either not at all indurated or imperfectly so.

Suppose the *force* to be exerted from the left against A, in a direction varying very little from horizontality.

Resistance to this force will proceed *from gravity*, each vertical square yard pressing with a weight in the case supposed, of *one*

* The Professors Rogers in accounting for the facts they have so skilfully developed, admit a degree of lateral action; but they argue that this action proceeded from the propelling force or thrust of moving waves of molten material beneath the earth's crust, the material of the interior being supposed to be in a state of free liquidity and subject to undulations. With regard to the northwest slopes being steepest, they say (loc. cit., p. 512), "This forward thrust operating upon the flexures formed by the waves, would steepen the advanced side of each wave precisely as the wind, acting on the billows of the ocean, forces forward their crests and imparts a steeper slope to their leeward sides;" and alluding to the position of the operating force, as determined by the greater dislocations to the southeast, they say (p. 517), "the progressive rise of the whole belt towards the side which anciently lay near the shore of the Appalachian ocean, accords entirely with the belief that under the now rent and dislocated margin of the chain there was a vast accumulation of fluid rock charged with compressed gaseous matter, which exerted on the crust an enormous disrupting tension."

Prof. W. W. Mather, in his remarks on the secular refrigeration of the earth, (this Journal, xlix, 284,) accounts for the foldings and for the steeper northwest slopes on the ground of "a paroxysmal elevation and the action of inertia," this paroxysmal elevation, as he urges, arising from a change in the rapidity of the earth's rotation consequent on an abrupt change of dimensions from cooling, (see note, p. 177.) He says, p. 299, "If the earth has at any time become more oblate in consequence of increased angular velocity, inertia would tend to make the solid matter of the exterior of the globe press to the westward." And again, with reference to the steeper northwest slopes, (p. 292,) "Suppose the *sudden* elevation of a mountain mass one mile in height; it would still retain the linear velocity it had when a mile nearer the axis of rotation, while the *proper* linear velocity at this increased distance would be 3·1415÷24 miles, or 694 feet greater per hour than that which it had before its elevation. Inertia therefore would cause the mass at the top to press to the westward with a force proportioned to its mass and the above mentioned velocity."

and a half millions of pounds, or 750 tons;—also *from cohesion* within the bed, and below.

The force will travel slowly from A towards X, on account of the gravity, cohesion and partial compressibility of the mass: the first dislocation will hence take place towards A, and it will therefore produce a bulging, as B*n g*, A at the same time advancing to B. (The distance A*g*, for a specific direction of the force, will depend on the thickness, gravity and cohesion of the bed.)

The force continuing in action, *part* of it will be transmitted towards *g* and X, owing to the difficult flexibility of the bed arising from cohesion and gravity: *another part* will cause B to advance towards C, and tend to raise B*n g* to C*o g*. In the same manner, C*o g* will tend to change to D*p g*.

But the action upon *g* is increasing from two sources, viz:— 1. the propagation of the original force through the bed, which is enhanced as the elevation rises;—2. a new force of vast amount proceeding from the gravity of the inclined bed *p g*. Owing to the last mentioned cause, in connection with the yielding nature of the material, *p g* sinks to *p′ g*, and D*p′ g* becomes the actual position of the bed instead of D*p g*. The sinking of *p g*, and the primal force together, (if the latter were not before sufficient,) would cause *g w* to rise to *h u w*.

The force continuing, the position D*p′ g* is changed successively to E*p″ h*, F*p‴ i*. The greatest propelling power is exerted by the gravitation of the inclined bed *p g*, when its angle of inclination is between 45 and 60 degrees. Beyond 60° the action is increasingly downward, and the propelling part of the action becomes small. At 90° and beyond, the action is wholly downward, so that in this position, *p g* shortens only from the compressibility of the mass. Now, the action on *g w* is simply the primal force, nearly or quite the whole of which acts upon *g* X. Thus *h u w* rises to *i v w;* and this again, continuing to rise, changes in form in the manner just illustrated.

By this process, therefore, a *series of folds would be produced each with the inclination steepest on the side farthest from* A; *and moreover, these folds would be necessarily most abrupt the nearer they are to* A.

In the above, the lateral force has been supposed to act directly upon the borders of an oceanic depression. When the contraction in progress produces *fractures over the interior of a continent*, the continued contraction and increasing lateral pressure, still operating upon the same yielding area, might *produce plications parallel with the line of fracture, which would be most abrupt near it, and diminish in the distance,* a fact illustrated in the Urals.* The plications would differ in extent on the two sides of the line, provided the force or the material were different.

* Geology of Russia and the Urals, R. I. Murchison, i, 462.

II. *Reasons why this action should not produce perfectly regular and uniform folds.*—Irregularities would proceed—

1. *From a variation in the thickness of the bed,* in consequence of which there would be a difference in the gravity of the mass in different parts.

2. *From a want of uniformity in the material* or its state of induration, causing the cohesion to vary, and hence also the flexibility or frangibility of the bed.

3. *From an inequality in the action of the force upon the different parts of the line against which it operates.* If the main cause of this force is contraction beneath the oceanic parts of the earth, such inequalities must have existed. For we know that igneous vents have been localized to a great extent over these oceanic areas, and generally they occur in lines, as groups of islands evince. Consequently the effects of contraction could not be equal along a given line.

4. From any irregularity which there might be in the contraction going on (for there should be some such contraction) beneath the area which is subjected to the lateral pressure.

A *fifth* reason might be added, but it is of a general nature and will form the subject of another communication. The four specified are sufficient to set aside any objections to the view urged on the score of the irregularities which exist.

III. *Effects of gravity on the inclined strata.*—When the beds become very much inclined, or dip at a large angle, the more sandy layers if not too much indurated, would settle bodily downward; the clayey layers would also settle, but owing to their cohesion when moist, they would become flexed or crimpled. *Thus plications would be produced,* from gravity alone; a fact abundantly illustrated in the metamorphic rocks of New England and other countries; and it might happen that small plications should in the same manner be produced between non-plicated beds.

IV. *Effect of lateral action where there is no plication, or but a limited amount of it.*—If the material subjected to lateral pressure be not capable of folding, or *only partially so*, the region operated upon instead of rising into a series of elevations, would be raised into one or more ridges of much greater height. Has not this last been the case on the Pacific side of the continent? or, is the elevation owing to a less nearly horizontal direction of the lateral force? or to a greater amount of oceanic depression?

V. *Intruded igneous rocks occurring with plicated beds.*—The occurrence of dikes or intruded masses of igneous rock in a plicated region, is no certain evidence that the intrusion was the cause of the plication, as the two effects, on the principles explained, might be concomitant results of the same general operation.

VI. *The folding of strata by subsidence of the plicated region can be only of small extent.*—This subsidence may or may not be attended by a *general contraction* of the earth's crust below the plicated bed. If not, then the bed, before straight, must be lengthened by the action to fit the curve of depression: a curve of a semicircle would require an extension of one half, in the bed, and a more abrupt plication, a greater extension. It is well known that clays and sand layers would not bear such a stretching, and the result could be accomplished only by fractures and openings. The material moreover would be drawn off from the summits of the convexities, or very much thinned out in those parts; a supposition not warranted by facts to the extent required in the explanation. The hypothesis moreover would not account for the greater steepness of the northwest slope.

But if the material beneath may be supposed to have contracted correspondingly with the amount of plication, then folds might have been produced by the process. The hypothesis however has many weighty objections. It is at variance with the fact that this same region remained unplicated, at least in the parts occupied by the coal formation, till after the coal epoch, although the contraction must have been more rapid during the preceding epochs of the earth's cooling.* The non-plication of the Silurian rocks of the centre of our country, adds force to this objection. Why this long delay in the action of those violent forces supposed to be imprisoned beneath the earth's crust?

Farther, a stiffened crust cannot be much folded by mere *shrinkage*, where the material is like that of the earth's crust. The fact that the Silurian rocks of the interior are not plicated by contraction below them, is evidence of this. Instead of becoming plicated, they have probably aided by lateral action in producing the elevations on the east or west, or the Ozark Mountains or other heights intermediate.

Moreover, the very *close compacted* folding illustrated in figs. 4 and 6, is a result which only lateral pressure could effect.

VII. *Position of volcanoes.*—The occurrence of volcanoes mostly in the neighborhood of the sea, is a necessary result of these principles. For we have already stated that fractures of the earth would be likely to take place near the limits between the contracting and non-contracting areas:† *here* they would have that depth and extent which is necessary in order that they should remain open as the seat of perpetual eruptions; for there is necessarily a wide difference as regards extent between those fissures

* The writer has offered as an explanation of this non-plication till after the coal epoch, the suggestion that the crust over the oceanic (or igneous) portions, had so far cooled by that time, that the pressure or strain arising from contraction was no longer relieved to the same extent as before by rents and upliftings over the igneous region. This lateral action was exerted long previously, but its greatest effects on the earth's features date subsequently to the carboniferous epoch.

† This volume, page 96.

that only allow the material to escape and form dikes, and those great fractures from which an Etna, or a range of Chimborazos, has originated. We have remarked in another place,* and the fact is sufficiently important to be again repeated, that the absence of the sea is no reason for the absence of volcanoes from the interior of our continents; since this same freedom from volcanoes existed in the Silurian epoch, when these very continents were mostly under salt-water.

VIII. *Geological epochs.*—This subject suggests a cause for the transitions marking geological epochs. The formation of the Appalachians was attended by rendings and emissions of heat on a vast scale, and the baking and crystallizing of the metamorphic rocks of the region, as well as the debituminizing of the mineral coal rendering it anthracite, are attributed by the Professors Rogers to this action. It is not a matter of surprise that there should have been an abrupt cessation with this event, of preëxisting forms of marine life. The period when the effects of dislocation began to be transferred from the oceanic areas to the continents, appears to have been the era of this catastrophe; and it was an era of similar changes in various parts of the globe. The previous epoch no doubt had its violent convulsions, but still there was *comparative* quiet favoring the continuance of Silurian life.

This era was probably followed after a while by another of similar quiet to that which preceded it, along the eastern portions of our country; and during this elapsing time, tension (from the progressive contraction,) may have been slowly increasing. The opening of the trap fissures, and their injection with the molten rock, may mark the termination of this period of quiet—the extensive fractures being a result of the increased tension. The parallelism of the dikes to the Appalachians, alike in Nova Scotia, the Connecticut valley, and New Jersey, and farther south, renders it probable that the same grand cause which produced the elevation of those mountains, produced also this result. These igneous eruptions and the vibrations which the ocean must at times have experienced, are adequate to explain the occurrence of a second era in the geological history of this country.

We know not how widely the last catastrophe extended over the globe, or whether it belonged to this continent alone, for we may not say with certainty whether displacements and fissure-ejections of the same general era in Europe, belonged to this particular period in the era. The above facts are brought forward to illustrate the general principle, already admitted by some writers, that such grand crises,—by causing wide emissions of heat, a change of level in the sea, and violent shakings of the globe with its mobile waters,—were in early times a necessary result of the

* This Journal, ii Ser., ii, 353.

contraction in progress. Facts on record prove farther, that these grand catastrophes had their widest influence after the coal era, and became less and less general as succeeding ages rolled on.

IX. These principles give us some *data for comparing the energy of forces in past times in the earth's history, with those now in action.*—In admitting them as a basis of geological reasoning, we may be considered as proceeding on an hypothetical basis. Yet in reasoning without reference to them, is the ground assumed any the less hypothetical? With those who believe in the former igneous fluidity of our globe, contraction is the grand and fundamental agency to be first considered after the general principles of solidification.

X. *Tides and paroxysmal movements beneath the crust of the globe.*—In the course of this article we have not alluded to the effects of tidal and other motions in the heated interior of the globe, leaving it for those who can prove their occurrence to modify thereby the explanations here offered. Several difficulties have appeared to the writer to strengthen the opinion advocated by Lyell and Poisson, that the globe, before its crust had consolidated, had become so stiffly viscid as not to admit of tides, a condition believed to be essential to the formation of a permanent crust. If there were *daily tides*, or a *westerly movement*, or if *undulations* were possible, sufficient to throw up the Appalachians, why, as we have asked before, were these mighty and resistless agents nearly dormant in this part of the globe till after the coal era? Why did they not act violently upon the Silurian rocks of the west, before the period that originated the Appalachians? and why not also more decidedly at the time of this great catastrophe? These questions are, perhaps, in part answered by Prof. Mather, by the argument that there would be grand paroxysmal effects attending contraction, causing at long intervals, a violent westerly movement beneath the crust. But, again, why if the cause of the mountain elevations is a westerly movement (that is a movement from the *east*) beneath the crust, why should we have mountains on the *west* side of the continent, while the wide interior is nearly flat? And why should these western mountains have attained such an altitude? Why should the areas of greatest igneous action be to the west of the summit on the Rocky Mountains, and to the east over the Appalachian region; that is, on the *oceanic* side in each case? These are among the objections to the hypothesis, that internal tides or undulations have been a prominent agent in geological dynamics since the beginning of the Silurian epoch; and if the explanations of phenomena, offered in this article, are at all satisfactory, they contain a still weightier argument against the view.