NOTICES OF MEMOIRS.

I.—On the Hypothesis of the Internal Fluidity of the Terrestrial Globe.

By M. Delaunay, Académie des Sciences, Séance du 13 Juillet 1868.

[Communicated by David Forbes, F.R.S., &c.]

A PROFOUND study of the several circumstances of the form, composition, and temperature of the materials which constitute the surface of the terrestrial globe leads to the admission that its interior possesses a high temperature, and, consequently, that the different substances of which it is composed are in great part in a state of fusion, so that the globe itself is essentially a liquid mass covered by a solid crust of but little thickness when compared with its diameter.

A formidable objection to this opinion was brought forward nearly thirty years ago by Mr. Hopkins in a series of memoirs inserted in the Philosophical Transactions of the Royal Society of London for the years 1839, 1840, and 1842. This objection, which is based upon the consideration of two astronomical phenomena, the precession and nutation, is as follows:—It is well known that the precession and nutation taken conjointly consist in a change of direction experienced by the axis of rotation of the earth. Without the precession and nutation, the axis of the earth would always remain parallel to itself, and if prolonged would always pierce the celestial dome in exactly the same point, at least if the dimensions of the earth's orbit be disregarded when compared with the distance which separates it from the stars.

In consequence, however, of the precession and nutation, the earth's axis becomes more and more inclined from the direction it previously possessed; the point in which it pierces the celestial dome, to which the name of the Pole is given, displaces itself slowly and by degrees amongst the stars, the precession causing it to describe a circle parallel to the ecliptic, whilst the nutation causes it to move in a very small ellipse, having for centre the position which it would have occupied if influenced by the precession alone.

This continual change in the direction of the earth's axis of rotation has been connected in a most happy manner with the grand law of universal gravitation,—Newton having demonstrated that the movement of precession follows as a consequence of the flattening of the earth.

The attraction which the sun exercises on the entire mass of the terrestrial globe would have no influence whatever on the rotary motion of the globe round its centre if the globe itself were spherical and homogeneous, or if it were made up of concentric and homogeneous spherical layers.

In consequence, however, of the swelling out of the globe along the equator this is not the case: the action of the sun upon the sort of pad formed by this equatorial swelling, causes little by little, a

change of direction in the rotary axis of the globe as a whole. The moon in its turn also produces an analogous effect by its action on this same pad, and it is the joint action of the sun and moon which ultimately produces the slow and complicated motion of the earth's axis, of which the precession and nutation are constituent parts.

In determining the effect due to these actions of the sun and moon upon the equatorial swelling of the terrestrial globe, the earth is regarded as a solid body, in which all parts are so intimately connected with one another that its entire mass is subject to the effects of these disturbing influences.

If, on the contrary, the earth consists of a liquid mass covered by a solid crust, this is no longer the case; the action of the sun and moon would be communicated to the entire solid crust of the globe; but the liquid interior, in consequence of its fluidity, could not participate in the effects of these actions. The disturbing forces in question, by only influencing the solid external crust of the globe, affect its total mass in a much less degree than if they influenced the entire terrestrial globe itself; wherefore the changes which take place in the rotary motion of the solid crust would be much greater than those which would occur if the globe is regarded as an entirely solid body; and these changes would be the more intense in proportion as the solid crust of the globe is supposed to be less thick.

This, then, is the basis of Mr. Hopkins' reasoning, from which he draws the conclusion, that in order to make the effect of the actions of the sun and moon upon the equatorial swelling of the earth agree with the magnitude arrived at by astronomical observations of the phenomena of precession and nutation, it becomes necessary to assign to the solid external crust of the globe a thickness of at least from 800 to 1000 English miles, or in other words of from one-fifth to one-fourth of the earth's radius; a result very different from the feeble thickness which geologists are wont to attribute to the solid external shell of our sphere.

This grave objection brought by Mr. Hopkins against previously accepted views has been followed up by Professor Thomson, of Glasgow, in his memoir on the Rigidity of the Earth (Phil. Trans. 1863, p. 573), where he introduces it as follows:

"1. That the earth cannot, as many geologists suppose, be a liquid mass enclosed in only a thin shell of solidified matter is demonstrated by the phenomena of precession and nutation. Mr. Hopkins, to whom is due the grand idea of thus learning the physical condition of the interior from phenomena of rotatory motion presented by the surface, applied mathematical analysis to investigate the rotation of rigid ellipsoidal shells enclosing liquids, and arrived at the conclusion that the solid crust of the earth must be not less than 800 to 1000 miles thick. Although the mathematical part of the investigation might be objected to, I have not been able to perceive any force in the arguments by which this conclusion has been controverted, and I am happy to find my opinion in this respect confirmed by so eminent an authority as Archdeacon Pratt (Figure of the Earth, 1860, § 85.) 2. It has always appeared to me, indeed,

that Mr. Hopkins might have pressed his argument further, and have concluded that no continous liquid vesicle at all approaching to the dimensions of a spheroid 6000 miles in diameter can possibly exist in the earth's interior without rendering the phenomena of precession and nutation sensibly different from what they are."

Thus it will be perceived that the objection brought forward by Mr. Hopkins against the ideas generally accepted by geologists as to the interior fluidity of the terrestrial globe, has been regarded by many learned Englishmen as perfectly established.

I am of an opinion diametrically opposed, and I believe that this conclusion of Mr. Hopkins is not based on any true foundation whatever. This is what I propose to explain to the Academy in all brevity.

When we apply the theories of rational mechanics to the study of natural phenomena, we immediately find that we have to deal with problems of the greatest complication. If we attempt to consider these questions with full rigour, it is impossible to succeed, for reasons which do not even require enumerating. We are obliged, therefore, to rest contented after resolving, not the problems themselves at which we aim, but other questions more or less bearing upon them, which in themselves present a degree of simplicity sufficient to enable us to arrive at some more or less rigorously correct solution.

It is thus that we are led to substitute the study of solids of absolutely invariable form, for that of those which actually are met with in nature; thus also we are accustomed to attribute to liquids the property of absolute fluidity, which in nature never exists, etc.

It becomes necessary, therefore, to place ourselves as it were side by side with the reality, and to remember always, that the results which we may have arrived at, may be vastly modified by circumstances which we may have neglected to take into account.

In order to concentrate our ideas on the subject, let us take a spherical vessel, a glass globe for example, filled with a liquid, say with water: if now we admit that the liquid is endowed with an *absolute fluidity*, and we impart to the globe a sudden movement of rotation round its central vertical axis, the globe should alone turn without at all carrying along with it the liquid which it contains, which ought to retain its pristine immobility.

This is easily verified by imparting a more or less rapid rotary motion to the globe; light substances floating on or suspended in the water will not appear to change place, notwithstanding the motion given to the globe. But will this always be the case, whatever be the rapidity of the motion given to the globe? Can we admit that the liquid will remain indifferent to the motion of the envelope which contains it should we revolve the globe very slowly?

In admitting the *absolute fluidity* of the liquid we forget to take into account its *viscidity*. Although this viscidity is extremely feeble in most fluids with which we are acquainted, it is never altogether wanting, and this explains why, provided that the rotary movement communicated to the globe be sufficiently slow, the liquid is carried

round along with the globe itself, the whole revolving together just as if the liquid had been frozen, and along with its envelope formed one entirely solid body.

Let us return to the terrestrial globe, and let us admit with the geologists that it is composed of a liquid mass covered by a thin solid crust. If the disturbing actions which produce the precession and nutation did not exist, the entire globe, both the solid envelope and its enclosed liquid, would revolve together as one, round the poles whose direction would remain constant in space.

Even if it is advanced, that at some particular epoch a difference might have existed between the rate of movement of the crust and of the liquid interior, the resulting friction must gradually have destroyed this difference and brought about a conformity in the motion of both parts.

The disturbing forces which give rise to the precession and nutation act upon the solid crust and tend to make it revolve round an axis, which deviates more and more from the direction of the axis round which it at first rotated; the rotary motion imparted to the solid crust by these actions is one of extreme slowness, and has to unite itself with the movement of rotation which it already possesses.

The question then is, whether the internal liquid mass participates in this additional movement, or is the solid crust alone affected by it without immediately carrying the liquid along with it. In my opinion there cannot be the least doubt as to what the reply must be, for the additional motion due to the before-mentioned causes is of such slowness that the fluid mass which constitutes the interior of the globe must follow along with the crust which confines it, exactly as if the whole formed one solid mass throughout.

The pressure to which the liquid mass which we suppose to exist in the interior of the earth is subjected to, is so enormous, that we cannot even form an idea of the influence which it may exert on the degree of viscidity of the fluid in question. But even if the fluid matter is present under conditions identical with those of the liquids which we see around us, this would suffice to cause the results to be such as we have already explained.

At the same time that I was perfectly convinced of the correctness of these views, I resolved nevertheless to confirm them by direct experiment. At my request, therefore, M. Champagneur, attached to the Philosophical Laboratory of the Sorbonne, has arranged a simple and complete experimental demonstration which removes every possible doubt on the subject.

I content myself at present with merely alluding to this experiment, as I leave it to that gentleman to make the Academy acquainted with its details.

After the preceding observations, it appears to me impossible to admit that the effect of the disturbing forces, to which the precession and nutation are due, only extend over a portion of the terrestrial globe: the entire mass, on the contrary, must be influenced by these disturbing forces, whatever may be the supposed magnitude of its fluid interior, and consequently it follows that the phenomena of

precession and nutation cannot furnish us with any data whatsoever relative to the greater or less thickness of the solid external crust of the globe.

[In directing the attention of our readers to this most important communication, which so thoroughly explains away the objections lately brought forward by mathematicians and astronomers, against the so-long accepted theory of the internal fluidity of the earth, we would express our entire concurrence in the remarks made by M. D'Archiac in the discussion of this paper, in which he expressed his lively satisfaction at seeing the subject thus ably investigated by an authority so doubly eminent both as an astronomer and mathematician as M. Delaunay. Further observations upon this interesting question will be found in a recent paper by Mr. David Forbes in the Chemical News of October 14, vol. xviii. p. 191, to which we also would refer.—Ed. Geol. Mag.]

II.—On the Formation of Mountain Chains.

By N. S. Shaler, Esq.[1]

NOTHING shows more clearly the imperfect nature of our knowledge of the forces which have brought about the existing condition of the earth's surface than the doubt which still exists as to the cause of mountain chains. There have been many views brought forward, some of which seemed to satisfy most of the facts, but none have been sufficiently broad to include all the phenomena, and the most clearly-defined result of the action of physical forces of the earth's crust still remains involved in obscurity. The main difficulty in the way of gaining an insight into the cause of all the dynamical phenomena of the earth's surface, is the doubt which has all along existed as to the physical condition of the mass of the earth. Until it is decided whether the sphere is rigid to the centre, or essentially fluid, with a crust floating upon its surface, it will scarcely be possible to attain to anything like certainty in our explanations of all the movements in the crust. Although in deference to the weight of opposing opinion, we must regard the question of fluidity or rigidity of the interior as still unsettled, there can remain little doubt in the minds of those geologists whose views are in no way influenced by the defence of long held opinions, that the earth is essentially rigid, and that the condition of mobility of the elements of the mass which perfect fusion gives, can not be the prevailing condition of the interior. The calculations of Hopkins[2] and Thomson[3] seem to make scarcely any other view possible, and the few investigations which have been made into the contraction of the igneous rocks, in cooling, make it impossible to conceive how a solid crust formed on a fluid interior could be sustained, subjected as it has been to innumerable shocks, sufficient to rupture it, and sink the fragments in the fluid below. Against

[1] From the Proceedings of the Boston Society of Natural History, June 6, 1866.

[2] Hopkins (Wm.) Phil. Trans. of the Royal Soc., 1836, p. 382.

[3] Thomson (W.) on the Rigidity of the Earth. Proceedings of the Royal Soc., Vol. xii. p. 103.

these facts we have to set those evidences of igneous action afforded by volcanoes and associated phenomena, and which have, not without reason, been supposed to give trustworthy evidence of a generally fluid condition of the interior. Fairly weighed, however, all that can be considered as proven by all the evidences we have is, that in that portion of the past history of the earth of which we have record, there has existed a condition of igneous fluidity beneath a large part, if not the whole extent, of the surface. That this igneous fluidity extends to the centre, or even that it is of more than a very few miles in depth, are suppositions which derive no valid support from igneous phenomena. The increase of temperature as we go from the surface towards the centre, and the extreme elevation of heat which must exist at considerable depths, can not be regarded as evidence of the general fluidity, until it has been shown that the internal pressure has not a greater influence in preventing liquefaction, than internal heat in producing that condition. In the present state of knowledge, or rather ignorance, of the physical questions involved in this problem, the safest position is that which conflicts least with the conclusions derived from the cognate sciences of astronomy and physics. The former science protests that certain observed facts could not exist if the mass of the earth was essentially fluid, and that tried by tests far more unerring than any the geologist is able to apply, the conclusion is reached that our planet is at least as rigid as glass, and probably as rigid as steel. From the physicist we hear that all the known materials which have come to us from the earth's interior, contract in cooling, and that the general internal fluidity would cause any crust to shatter to pieces and fall in fragments into the fluid below, as soon as it had attained any such thickness as we know the crust to have. If we attach to these calculations the importance they deserve, we are forced to admit that the idea of the igneous fluidity of the interior is quite untenable.

A much more satisfactory view than that just referred to, which will not conflict with the results of investigations in the exact sciences, may be obtained by a brief consideration of the possible conditions of solidification of the cooling earth. If the effect of pressure in promoting solidification at the earth's centre were greater than the effect of heat in resisting solidification, then the mass would congeal first at the centre, and solidification extend thence towards the surface. If, on the other hand, the effect of the pressure at the centre failed to overcome the tendency to liquefaction induced by the extreme heat of that point, then we must suppose that cooling went on until the whole mass was reduced to something like an equal temperature throughout, and the whole sphere became solid at once. During this process of cooling down, successive crusts might be formed, but they would necessarily be transient phenomena, breaking to pieces as soon as they began to attain considerable thickness.[1]

[1] See the Preliminary Observations to the paper of Hopkins above referred to, where these considerations will be found.

This last supposition seems to be excluded by the well-known fact of the increase of temperature as we go from the surface towards the centre; the rate of increase is such that we would attain a temperature sufficient to melt the most refractory substances in a few miles from the surface; this is far from the state of things we would expect to find if the whole interior had been reduced to the temperature at which solidification could take place at the surface before any part became rigid. On this account we are driven to adopt the other view as the more probable, and regard the superficial portions as the last to become solid, and the centre as the first rigid portion of the earth.

As solidification advanced from the centre towards the surface, there would be a time when the remaining liquid matter was of inconsiderable thickness, that the surface might also begin to solidify, and the intervening igneous matter being in a state of viscous fluidity, might so far uphold the solid outer crust, that it would not break up and fall into the fluid below. The further solidification of the interior would then take place in two directions outward from the central nucleus, and inward from the outer crust. If, however, this residual fluid matter was confined, beneath, say, one hundred miles of crust, cooling would proceed with such extreme slowness, that a very great time might elapse before it became lost in the already solidified surfaces above and below. It is not impossible that to this insignificant relic of an original molten condition, we owe all the phenomena of igneous action which have affected the crust since the beginning of the geological record.

There seems no point of conflict between this conception and those conclusions of geologists which are supported by any considerable amount of evidence; it only contravenes those hypotheses which have failed when subjected to critical examination, or which from their essentially undemonstrable character can not be either verified or disproven. At first sight it might seem difficult to account for the phenomena of corrugation of the earth's crust, as exhibited in the continental folds, and in mountain chains, if we reject the hypothesis of internal fluidity. The design of the present paper is to show some reasons for believing that both of these phenomena may be explained without the assumption of anything more than the trifling amount of igneous fluidity involved in the hypotheses we have just discussed.

Without any particular examination of the facts, it seems to have been assumed by most geologists that all the phenomena of corrugation, whether exhibited in mountain ranges, or in continents, are to be regarded as effects of one and the same cause, differing only in magnitude. It is manifest that it is a matter of first importance in seeking an explanation of the origin of these phenomena, to determine whether this assumed identity of cause is true or no. If it be the fact that continental elevations and mountain elevations are but degrees of effect of the same cause, then there should be no other differences in the phenomena than those of magnitude, or of features dependent directly upon the magnitude of the areas involved in the disturbance; furthermore there should be something like a series, at

one extremity of which could be placed the greatest relief of continental fold and oceanic depression, and passing gradually to the most inconsiderable flexures. It requires no very careful examination to bring the observer to the conviction that those essential features do not exist. The phenomena observable in the two actions are not cognate. There can hardly be said to be anything like a series or gradation connecting the whole assemblage of phenomena, and the inference seems strong that the cause is not the same in the two cases. We find, for instance, in continental folds, broad curves of the surface, which narrow without exception towards the south, and which exhibit in no part of their structure the evidences of powerful lateral thrust, which are the most conspicuous phenomena of mountain chains. In these latter, however, we perceive evidences of linear disruption of the crust, showing intense, but localized energy, with no tendency to increase of magnitude in any one direction. In the continents we behold curves of thousands of miles in diameter, showing an equal force acting throughout, in the mountain very powerful forces acting along one line, and inoperative a few tens of miles away. There seems nothing in common in the phenomena except that both are folds of the earth's surface. The great breadth, and comparatively gentle curves, characterising the continental folds, show that a great thickness of material is involved in the movement; their gradual development in successive geological periods, together with what we know concerning the loss of heat from the interior of the earth renders it eminently probable that they arise from the accommodation of a hardened outer crust to a diminished nucleus. All the fluidity required in this view of the effect of the contraction of the mass upon the contour of the crust, is given by the hypothesis which claims that solidification began at the centre, and that all that remains in any sense liquid, is a very small portion comparatively near the surface.

While the contour of the continental folds, as exhibited both in land surface and sea floors, evinces the gradual operation of the general contraction of the earth on a crust of great thickness, we have in mountain chains another effect of contraction, which cannot, from the evidence, be properly referred to the shrinking of the whole mass. It is evident that if the continental folds are compensative wrinkles formed in the adaptation of a crust to a diminished nucleus, the mountain chains can not be of the same nature; it is not to be believed that a crust would bend from the action of the same force into the broad, low curves of the continents, and into the sharp defined and narrow fractures of a mountain range.

Accepting, as established, the fact that mountain chains are the result of lateral pressure, and indirectly of contraction from loss of heat, and denying that they are the result of the accommodation of the crust to the nucleus, it is at once manifest that we must seek their origin in the changes going on within the crust itself, and in no way connected with the regions below. And within that crust we can find forces operating to produce contraction quite sufficient to account for all the facts.

According to the computations of Thomson,[1] we may assume that at the close of ten thousand years after solidification of the surface of the earth had taken place, the rate of increase in temperature would be 2° Fahrenheit for each foot of descent, and with the lapse of time the rate of increase in going towards the centre would be less and less rapid in about the proportion indicated in the table below.

10,000	years after	freezing of	surface,	2° Fahr.	for each foot.[2]
40,000	,,	,,	1	,,	,,
160,000	,,	,,	$\frac{1}{2}$	,,	,,
4,000,000	,,	,,	$\frac{1}{10}$	,,	,,
100,000,000	,,	,,	$\frac{1}{50}$	,,	,,

If this calculation is correct, (and that it is in a general way correct, does not seem to admit of much doubt, provided we accept the hypothesis of original igneous fluidity,) it follows that the gradual cooling of the deeper portions of the crust must result in the formation of a strong lateral pressure at every point near the surface. The truth of this proposition is readily seen, when we consider that while the original surface, which in ten thousand years after the hardening of the crust had been reduced to the temperature of the atmosphere, retained the same temperature in the ages which followed, the portions of the crust beneath were constantly parting with their heat, and approaching nearer to the thermal condition of the surface. There would be no shrinkage of the surface layer from the loss of heat, while from this cause the contraction of the deeper portions would be considerable. This would give precisely the conditions requisite to produce a rending and upfolding of the superficial strata of the outer shell. Immediately after the formation of a crust, the progressive diminution of the interior heat would begin to produce a tension on the surface, which would augment as the ceaseless flow of heat went on, until either a rupture of the contracting beds, or the folding together of the superficial layers, relieved the strain. Both these methods of accomplishing the movement of contraction, have been most probably operative at different times and places in the earth's history. Furthermore, as the upper portions of the crust, or region of slight contraction, is of much less thickness than the region which, by its considerable contraction, produced the tension, we

[1] Thomson (Wm.) on the Secular Cooling of the Earth. Trans. Roy. Soc. Edinburgh, xxxiii. sec. 1.

[2] The effect of these changes in temperature may be estimated from the following table of the expansion of various substances under the influence of heat:

For each degree of Fahrenheit,

Granite	expands	about	000004825
Marble	,,	,,	000005662
Sandstone	,,	,,	000009532

A stratum of granite five hundred miles in diameter would contract, on passing from a temperature of 3,000 degrees Fahrenheit to the average temperature of the earth, about seven and a half miles; in the case of a sandstone area of the same diameter, the contraction would amount to about fifteen miles.

The computations on which these estimates are founded were based on experiments made by Mr. H. C. Bartlett, of the United States Engineers, and published in the Amer. Jour. Science, vol. xxii. p. 136. See also for other data on this point Ninth Bridgewater Treatise, C. Babbage, 2nd edit. Appendix, p. 221.

would expect the fracture to take place on the surface, rather than below. There is one thing which could operate to prevent the certain contortion of the superficial portions of the crust, and that is the horizontal position of its beds; as ordinarily constituted, the resistance which the upper few miles of the crust could oppose to the action of any force tending to throw it into folds, is very great. When the contortion has once begun, and this resistance fairly overcome, all further changes would meet with comparatively little resistance.

We have spoken only of those cases where the original surface had continued to exist from the beginning, while the isogeothermals beneath them had gradually sunk deeper and deeper towards the centre of the earth. This being a very unlikely condition, it remains to be seen what would be the effect where the actions of denudation, or deposition, are going on. It is evident that whenever the rate of denudation was such that the removal of the crust took place with the same rapidity as the recession of the isogeothermals from the surface, there could be no lateral strain produced by the loss of heat. Where, on the other hand, rapid deposition of materials was taking place, and the isogeothermals, on that account, were rising towards the surface, there would also be no such strain on the upper part of the crust. It thus appears that the conditions of tension competent to produce mountain chains, would only be found strongly developed in regions where the rate of denudation was less than the rate of recession of the isogeothermal lines, or where the rate of deposition was not sufficiently rapid to prevent the recession of the lines of equal heat.

Accepting this hypothesis of the origin of mountain chains, it is at once seen that they should have their region of greatest development on the land surfaces, and seldom or never originate on the ocean floors. On the land areas we would expect to find them originating at those points where there were some forces operating to favour the displacement of the beds constituting the crust, from their normal position, for at such points the contracting force would most easily produce corrugations. The author has elsewhere given a brief notice of a view of the origin of continents, from the tendency of all regions where deposition is going on, viz., sea bottoms, to subside.[1] This view, if correct, will warrant us in believing that shore lines are points where fracture and dislocation of the crust are likely to occur. The distribution of volcanic vents of the present day, and the instructive fact that volcanic outlets of former geological periods ceased to be active when left inland, in the progress of geological changes, would of themselves indicate a peculiar liability to rupture of the superficial portions of the crust along shore lines. Let us suppose that the recession of the isogeothermal lines had placed the superficial portion of the crust in a state of tension, which could only be relieved by the formation of mountain elevations, and that the laying down of sedimentary materials had, at the same time, prepared that portion of the crust beneath the ocean floor for subsi-

[1] See these Proceedings, Vol. x. p. 237.

dence, then the moment this latter action is effected, it is likely to bring about fractures along shore lines, attended by the escape of gaseous and igneous materials. This dislocation of the crust would be attended by the pushing together of the superficial portions from either side, and the resulting elevations might be complicated by the intrusion of a greater or less amount of igneous matter.

This view of the origin of mountain chains seems to be reconcilable with some of the most prominent features which are to be found in their structure and distribution. Their usual, if not invariable, origin along shore lines, the suddenness of their formation, the variable amount of igneous action exhibited in their masses, are explicable on this hypothesis. On the other hand it is not to be denied that some considerable objections can be urged against it. In the first place, in order that any considerable elevation be formed by this action, it would be necessary to have the upper and lower beds slide one upon another, to a certain extent; but it is to be borne in mind that the power we are hypothecating is practically illimitable, since it would, by the supposition, continue to accumulate until the force became sufficiently great to overcome resistance. The sliding of beds upon each other under the influence of great lateral pressure, from the contraction of the lower portions of the crust, has fewer objections to be urged against it than the view which assigns the origin of mountain chains to the passage of great waves of translation through the crust, and their fixation by the intrusion of molten matter.

It is scarcely necessary for the author to state that no claim whatever is meant to be made in this paper to the hypothesis of the origin of the features of corrugation of the crust from the influence of contraction from loss of heat, one of the oldest and most generally accepted theories of the science. It having been denied by very high authority that there existed any cause competent to produce lateral thrust, and thus to originate mountain chains, it has seemed desirable to direct attention to the fact that the recession of the isogeothermals would be attended by such lateral strain.

The points which have been suggested in the foregoing consideration, may be briefly summed up as follows:

1. That the most probable hypothesis in the present state of our knowledge of the earth, is, that it consists of an immense solid nucleus, a hardened outer crust, and an intermediate region of comparatively slight depth, in an imperfect state of igneous fusion.

2. That the continental folds are probably corrugations of the whole thickness of the crust.

3. That mountain chains are only folds of the outer portion of the crust caused by the contraction of the lower regions of the outer shell.

4. That the subsidence of ocean floors would, by producing fractures and dislocations along shore lines, tend to originate mountain chains along sea borders, and approximately parallel to them.

[In connection with this subject, see also paper by the Rev. O. Fisher, *ante*, p. 493; also Delaunay, *ante*, p. 507.]