limbs succeeding it, are chelate, all the five pairs of legs being monodactylous, as in *Palinurus*, *Scyllarus*, and *Thenus*: but in this division the carapace and the abdominal segments are not arched, but expanded laterally; whilst this Lias Crustacean, like the *Astacidae*, *Palæmonidae*, etc., has the carapace compressed laterally, and the segments of the abdomen are not flattened, but are well arched. The antennules are not like those of *Palinurus*, which have a few long articuli, but are multiarticulate like those of the *Astacidae* and *Palæmonidae*.

From a careful comparison, made in 1867, of its general characters, I was led to consider the fossil before us as probably most near to the recent genus *Atya* of Leach, from South¹ America, and I then proposed to name it *Prætya scabrosa*, but in all the recent species of the genus *Atya* the third and fourth pairs of thoracic appendages are modified so as to subserve, like the first and second pairs, rather the office of maxillipeds or mouth-organs than of feet; the fifth, sixth, and seventh pairs alone remaining as simple monodactylous ambulatory legs. Whether this modification of the third and fourth pairs of thoracic appendages in the genus *Atya* has taken place since Liassic times, and so the fossil form be really ancestrally related to that modern crustacean, can only be a matter of conjecture, but bearing in mind this important difference in the modification of the thoracic limbs, they have nevertheless still many points of resemblance. I have therefore retained the original name *Prætya* (conferred upon it in 1867), and by that appellation I now beg leave to introduce it to palæontologists and especially to those who are interested in Liassic fossils.

EXPLANATION OF PLATE VII.

- Fig. 1. *Prætya scabrosa*, H. Woodw., Lower Lias, Barrow-on-Soar (drawn nat. size). The original specimen in the British Museum.
 „ 2. *Prætya scabrosa*, H. Woodw., Lower Lias, Bath. The original specimen in the Collection of Charles Moore, Esq., F.G.S., of Bath.
 „ 3. Outline of *Atya scabra*, Leach (recent), South America.

II.—ON THE POSSIBILITY OF CHANGES IN THE LATITUDES OF PLACES ON THE EARTH'S SURFACE; BEING AN APPEAL TO PHYSICISTS.

By O. FISHER, Clk., M.A., F.G.S.

MR. HILL'S paper in the June Number of the MAGAZINE has incited me to recur to the great question of the possibility of changes in the earth's axis of rotation within itself. Mr. Hill is well known to be an accomplished geologist; but he writes as if he were simply a physicist, without sympathies for the difficulties of his brethren of the hammer. Yet we feel certain that such is not the case. We know that he has studied in the field the tremendous movements which the strata have undergone, being often compressed into a small part of their original length: that he has appreciated the almost ubiquitous presence, either in past or present time, of volcanic activity: that he must feel how unsatisfactory all explana-

¹ Incorrectly marked as N. America on the Plate.

tions short of a change in latitude are to account for the flora of a warm climate within the arctic circle. We therefore appeal to him, and to others similarly qualified, to look at this subject from both sides.

Dr. John Evans, in a paper which he read before the Royal Society in 1866,¹ suggested a cause why, in his opinion, a mountainous region, newly upheaved above the general surface of the earth, would have a tendency to travel towards the equator, moving the rest of the crust along with it. "My hypothesis," he said ten years later, in his Presidential Address of 1876, "was founded on the assumption of the globe consisting of a comparatively thin crust with an internal fluid nucleus, over which the crust could slide when from any geological cause its equilibrium was disturbed."² But the hypothesis under which he then appealed to physicists for a solution of the question, if not intended by him to be different from the above, has, at any rate, been so taken by Mr. Twisden,³ and Mr. G. Darwin,⁴ and, following them, by Mr. Hill; for all these gentlemen have supposed the globe *solid* from the surface to the centre. I do not think that any geologist can easily entertain this opinion; and when, in handling the subject, one omits all reference to any other possible supposition as to the state of the earth's interior, it seems to me that he is not rendering that help to his less accomplished brethren which he might render.

The causes, which lead physicists to regard the problem from their point of view, may partly be, first, that the great problem of Precession and Nutation has always been so treated. They are, therefore, already some way on their road towards the elucidation of any other question of this class. Secondly, the perhaps greatest living physicist in this kingdom has pronounced in the strongest language against the doctrine of a fluid interior. He appears, in consequence of a conversation with Professor Newcomb, to have given up the original argument against it from precession and nutation. "Interesting in a dynamical point of view as Hopkins' problem is, it cannot afford a decisive argument against the earth's interior liquidity."⁵ But there remain the tides. "The solid crust would yield so freely to the deforming influence of the sun and moon, that it would simply carry the waters of the ocean up and down with it, and there would be no sensible tidal rise and fall of water relatively to land."⁶ I have inquired⁷ whether, taking their lagging into consideration, it is certain that the crests of the tidal waves in the ocean and in the crust would occur simultaneously at the same place: because, unless they did so, we might still have a tide relatively to land. Recently, at the Cambridge Philosophical Society, when Mr. Hill gave some illustrations corroborative of Mr. G. Darwin's results, Mr. Darwin told the meeting, in reply to some remarks of mine, that he had

¹ Proc. Roy. Soc. No. 82, 1866.

² Quart. Journ. Geol. Soc., vol. xxxii. Presidential Address, p. 108.

³ Quart. Journ. Geol. Soc., vol. xxxiv. p. 35.

⁴ Trans. Royal Soc. vol. 167, part i.

⁵ Sir W. Thomson's Sectional Address, *Nature*, vol. xiv. 1876, p. 429, 2nd column.

⁶ *Ibid.*

⁷ Quart. Journ. Geol. Soc., 1875, p. 470.

been investigating the above question raised by me. We may, therefore, hope to receive a decisive answer to it.

It does not, however, appear necessary that geologists should be concerned to inquire whether the earth is *wholly* fluid within. If there be granted a layer of fluid matter of a thickness even very small compared to the radius of the earth, that will suffice us. May not the argument for rigidity drawn from the tides, after it has received that definite weight which proper observations (yet to be made) are expected to give it, be satisfied with a rigid nucleus of radius nearly approaching to that of the entire globe? Such tides as would be formed within the liquid substratum of the crust would not be of the nature of the tides, contemplated by Sir W. Thomson as affecting the entire spheroid, but more nearly analogous to the ocean tides; since they would involve a horizontal transference of fluid backwards and forwards, and might be expected to be of small amplitude, owing to the viscosity of the substance and its confinement beneath the crust. However, even the modest requirement of a fluid substratum is denied to us. "But now thrice to slay the slain. Suppose the earth this moment to be a thin crust of rock or metal resting on liquid matter. Its equilibrium would be unstable! And what of the upheavals and subsidences? They would be strikingly analogous to those of a ship that had been rammed; one portion of crust up and another down, and then all down. I may say with almost perfect certainty that whatever may be the relative densities of rock, solid or melted, at or about the temperature of liquefaction, it is, I think, quite certain that cold and solid rock is denser than hot melted rock, and no possible degree of rigidity in the crust could prevent it from breaking in pieces and sinking wholly below the liquid lava."

If we now turn to Mr. Hopkins' *Researches in Physical Geology*,¹ we find that he had gone into this question somewhat fully, and held an exactly opposite opinion from the above. He considered that, so long as the matter of the earth retained a sufficiently high state of fluidity to admit of the circulation of convection currents, no crust could form: but that when, by those means, the temperature had been so far reduced that convection became arrested, immediately a crust would be formed. "Since the heat increases with the distance from the surface, while the mass is cooling by circulation, the tendency to solidification, so far as it depends on this cause, will be greatest at the surface, and least at the centre. But on the other hand, the pressure is least at the surface, and greatest at the centre; and consequently the tendency to solidify as depending on this cause will be greatest at the centre, and least at the surface." For want of experimental evidence, "the only conclusion at which we can arrive is this, that if the augmentation of temperature with that of the depth be so rapid that its effect in resisting the tendency to solidify be greater than that of the increase of pressure to promote it, there will be the greatest tendency to become *imperfectly fluid*,

¹ Trans. Roy. Soc. part ii. 1839, quoted in his Report to the British Association, 1848.

and afterwards to solidify, in the superficial portions of the mass : whereas, if the effect of the augmentation of pressure predominate over that of temperature, this transition from perfect to imperfect fluidity, and consequent solidity, will commence at the centre."

Assuming the latter to be the case, when the mass should have arrived at that stage of cooling that "a solid nucleus had been formed, surrounded by an external portion of which the fluidity would vary continuously from the solidity of the nucleus to the fluidity of the surface, where, at the instant we are speaking of, it would be just such as not to admit of circulation ; . . . a change would take place in the process of solidification which it is important to remark. The superficial parts of the mass must in all cases cool the most rapidly, and now (in consequence of the imperfect fluidity) being no longer able to descend, a *crust* will be formed on the surface, from which the process of solidification will proceed far more rapidly downwards, than upwards on the solid nucleus." And in a note to the Report,¹ he writes thus decidedly : "Supposing the earth once to have been fluid, it must be now, or have been at some antecedent epoch, in that state in which a solid exterior rests on an imperfectly fluid and incandescent mass beneath. It is important to know that this state of the earth, assuming its original fluidity, is one through which it must necessarily have passed in the course of its refrigeration, whatever might be the process of its solidification."

These remarks of Mr. Hopkins deserve serious consideration, and are not lightly to be set aside. They are entirely independent of his subsequent calculations by which he considered he had proved (and was until very lately indeed considered by the greatest mathematicians to have proved)² that this crust was not at the present time a thin one, but had grown to the thickness of at least not far short of a thousand miles ; even if its downward growth had not already met the upward growth of the solid central nucleus.

The reasoning by which Mr. Hopkins concluded that a crust would form (and he clearly supposed it would be supported also), seems to be assailable only on the supposition that upon solidification a sudden considerable contraction, and consequent increase of density, would occur, which would enable the fragments to sink in a fluid, too viscous to admit of the sinking of portions cooled to the *verge* of solidification.

It is practically seen that a crust can be formed and supported upon liquid lava by what is known to happen in the crater of Kilauea. This is of an elliptical form, three miles in its longer diameter : consequently the attachment to the cliff-like sides can have little effect in supporting the crust over so large an area. Moreover, this has been observed to rise and sink with variations in the level of the lava, showing that it really rests upon the fluid support. "The floor of the pit is described as usually presenting a crust over a vast pool of lava, which is from time to time broken through by a fresh upboiling of the incandescent mass beneath.

¹ Page 48.

² *Vide supra*, p. 292.

It cools and hardens so rapidly on exposure, that it may be walked upon within a few hours after coagulating. Sometimes upwards of fifty small cones and craters, more or less in activity, have been counted on the floor of this great pit. They were from fifty to a hundred feet high."¹ "It is nine miles in circumference, and its lower area, which not long ago fell about 300 feet, just as ice on a pond falls when the water below it is withdrawn, covers six square miles."² In the case of the earth's crust, it seems probable that the lateral pressure, to which the upheavals and subsidences are due, would have the effect of keeping the fragments of the crust close pressed together, and so checking anything like a general intrusion of lava between them. Its exit would probably be confined to here and there volcanic vents, more numerous, no doubt, and larger, in ancient times, but still in general character like those of the present day.

The argument for the existence of a fluid layer beneath the present crust appears to me to be considerably strengthened by a comparison which I have myself made between the actual inequalities of the earth's surface and those which could have been formed by lateral pressure if the globe had cooled as a solid body.³ This has led me to the conclusion that some condition has been present which has admitted of its shrinking much more than it could have done if it had cooled as a solid; nay, probably more than mere cooling alone can account for. I surmise therefore that the interior has contained water, which has escaped through volcanic vents as superheated steam, and that the earth's volume has been diminished in that manner by the actual transference of matter from beneath to above the crust. What may be the exact amount of decrease of volume which might be obtained by transferring a given quantity of water from beneath to above the crust, it is not possible to say; because we do not know the properties of water as it may exist under such circumstances of heat, pressure, and chemical combination as obtain within the earth. But it is obvious that a larger contraction can be accounted for by aid of this supposition than without it. I received a letter from Mr. Scrope in October, 1876, very shortly before his death, in which he commented on this suggestion. "There is one of the points you put forward which never struck me before, but which now appears to me most valuable; viz., that the enormous amount of steam that has escaped from the interior in early times, as well as down to the present, has been, and is, the cause of those *subsidences* of the crust, to which the basins of seas and oceans, and the crumplings of the terrestrial rocks are owing, far more than to any general contraction of the nucleus by cooling."

It is known also that mountain chains have most of them been ridged up and folded much more than the country bordering upon them. Now this peculiarity implies a slip of the superficial strata

¹ Scrope's *Volcanos*, p. 477, ed. 1862.

² Miss Birt's *Hawaiian Archipelago*. See *Nature*, vol. xi. p. 324.

³ *Trans. Cambridge Phil. Soc.*, vol. xii. part ii., abstracted in *GEOL. MAG. N.S.* Vol. I. p. 60.

over whatever it be that underlies them. It is obvious that a more or less perfectly fluid substratum is required to give this capability of slipping. Indeed the nearest analogy to the formation of a chain of mountains seems to be found in the crushing together of the adjacent edges of two sheets of floating ice.

I have thus, I think, shown that the supposition of a liquid layer beneath the crust of the earth is not a supposition that is so wholly untenable as has been represented, and that, as far as *authority* (that blind guide) can determine the question, we have it on both sides. I recur therefore to my original proposition, that physicists should be moved to consider what elements of change in the position of the earth's crust as regards latitudes might be introduced under the influences acting upon it, by the existence of a considerable degree of mobility between the crust and a solid nucleus owing to the intervention of a liquid stratum. The solidity of the crust being supposed due to cooling, and that of the nucleus to pressure, I would consider the crust thin, and the fluid substratum also thin. I would consider the crust and the fluid of sensibly the same thicknesses at the equator as elsewhere, taking the equatorial bulge as existing in the solid nucleus. This supposition would seem admissible, because we have volcanos in low as well as in high latitudes, showing that, if they be connected with a liquid substratum, that cannot be much more deep down in the one region than in the other. Moreover, the ratio of the centrifugal force to gravity being small, it is not likely that the upper limit of the pressure necessary to induce solidity should be at an appreciably greater depth below the surface at the equator than at the poles. I would consider the crust flexible, but *for the purposes of the problem* not horizontally compressible further than would allow it to fit the spheroid in any new position so that it should move all together. Then I would inquire, a portion of it having become thickened by assemblages of foldings near together, in other words by the elevation of continents, what effect these would have upon the position of the crust as regards the axis of rotation of the whole, supposed fixed in space.

This is, in short, Dr. Evans's original problem, which he endeavoured to solve by experimental means.

But the question I would put is, whether, supposing the crust free to move under forces as small as those which this change of form would bring to bear upon it, the effect would be (not that the protuberances would be brought towards the equator, as in Dr. Evans's wheel), but that the crust would tend to be carried into such a position that the axis of symmetry of the solid nucleus and of the crust together (*i.e.* their axis of greatest moment of inertia) would tend to coincide with the axis of rotation. If this were so, the nucleus being spheroidal, the crust would eventually become so placed with regard to the axis of rotation that that would coincide with its axis of principal moment of inertia. If, however, the crust were free to obey this tendency only to a certain degree, being checked, partly by the rigidity of its parts not

allowing it to adapt itself freely to its altered position on the spheroid, partly by the viscosity of the liquid, and partly by decrease in the moment of the forces shifting it; then, the crust having progressed as far as it was permitted to do, would stick fast, and the whole would commence to rotate as a solid body, and the principal axis, being that neither of the crust alone, nor of the solid nucleus alone, but of the whole mass, would come to coincide with the axis of rotation.

Now when I look at the distribution of land on the globe, and observe it chiefly confined to one hemisphere, and arranged in a manner, not symmetrical, but with an approach to symmetry as regards moment of inertia about the axis of rotation, I fancy I see an indication of the truth of the supposition advanced.

It is possible that a future generation of geologists may be able to gather some information regarding the interior of the earth from the mysterious phenomena of magnetic variation. But at present we cannot hope for much help from this quarter. The temptation, however, is too great to forbear quoting from Captain Evans's lecture a very striking passage.¹ "These are a few facts relating to secular changes going on in two magnetic elements within our own time; and what are the inferences to be drawn therefrom? They appear to me to lead to the conclusion that movements, certainly beyond our present conception, are going on in the interior of the earth; and that so far as the evidence presents itself, secular changes are due to these movements, and not to external causes. We are thus led back to Halley's conception of an internal nucleus or inner globe, itself a magnet, rotating within the outer magnetised shell of the earth." This is, to say the least, in very remarkable accordance with the conclusion I have proposed to draw from geological and other considerations.

III.—A LIST OF THE MADREPORARIA OF CRICKLEY HILL, GLOUCESTERSHIRE, WITH DESCRIPTIONS OF SOME NEW SPECIES.

By ROBERT F. TOMES, F.G.S., Corr. M.Z.S.

A CONSIDERABLE part of the material forming the present communication was collected as long ago as 1862, but the specimens obtained at that time, with the notes thereon, remained in abeyance until quite recently. On a renewed examination of them, it appeared that much of what had been noted down had not been superseded, and this induced me to go through the whole of it again, and make such additions and modifications as seemed necessary to bring it into its present form. And I have done this in the hope, and indeed in the belief, that it will serve a twofold purpose. It will make known some forms which are new, not only to the particular locality, but also to the catalogue of English species; and it will furnish a more complete list than has yet appeared of the species from one of the best-known Coral-reefs of the Inferior Oolite.

The Crickley Coral-reef, according to Dr. Wright, is the lowest of

¹ Lecture at the Royal Geographical Society, March 11, by Capt. F. J. Evans, C.B., F.R.S., Hydrographer to the Admiralty, *Nature*, May 16, 1878.