ON THE EVIDENCE

THAT THE

Earth's Interior is Solid.

BY

DR. M. E. WADSWORTH.

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IT is well known that the facts of geology require some mode of escape from the now generally-received opinion that this earth is a solid globe. This is especially shown by the introduction of so many hypotheses to avoid the logical consequences incident upon such a globe. Some of these hypotheses are lakes of still unsolidified material; masses of readily fusible materials; aqueo-igneous solution of sediments; fusion of the earth's interior on the removal of pressure; a solid crust and a solid core with a zone of liquid matter between, etc., etc. All these views are simply compromises between the known fact that some part of the earth must be liquid in order to account for geological phenomena and the supposed physical and mathematical demonstrations of the earth's solidity. These compromises are unnecessary if it can be shown that the claims for the earth's solidity are not well based.

The facts of petrography likewise seem to the writer to demand the belief that all eruptive rocks have come from the earth's interior, below any of the sedimentary deposits. Also that they must have come from material that has either never been solidified or else has been reliquified.

This paper has arisen out of the fact that to the writer's mind there appeared a diversity between the commonly taught hypothesis of the earth's structure and observed petrographical facts. He therefore undertook an examination of the principal published data in order to see whether the error resided in the petrographical observations or in the data for the hypothesis of a solid earth. The result led to the writing of this paper nearly two years ago, and it now stands essentially unchanged. Its object is to place before others the chief lines of argument in behalf of a solid earth and to indicate that geology and petrography have as yet the right to assume any structure for the earth's interior consistent with known facts, without regard to the so-called physical and mathematical demonstrations of its solidity.[1]

The Evidence of the Earth's Solidity derived from Precession and Nutation, and the Tides.—The most prominent of the early advo-

[1] *Science.* 1883. I. 127–150.

cates of the essential solidity of the earth, was Mr. William Hopkins, who based his conclusion upon the phenomena of precession and nutation. In order to mathematically investigate this question, he made two assumptions regarding the earth: 1. That it was composed of a homogeneous fluid mass contained in a homogeneous solid shell. 2. A heterogeneous fluid inclosed in a heterogeneous solid shell. In both cases the transition was assumed to be "immediate between the entire solidity of the shell to the perfect fluidity of the interior mass." He further assumed that the circulation would go on in every portion of the mass until it had lost its perfect fluidity throughout the entire mass at nearly the same instant. This of course could only be correct if the liquid was homogeneous. He did, however, state that a viscid condition of the liquid would prevent the descent of the cooled exterior portions towards the interior. With a globe constituted as above for his basis, Hopkins concluded that the phenomena of precession and nutation did not demand on the earth a crust of over 1000 miles in thickness, but they did require one at least 800 miles thick. In order to account for volcanic phenomena, he assumed that there were in the solidified crust, lakes of molten material whose origin was to be ascribed to a greater fusibility of the material composing them, than of that forming the surrounding solid crust.[1]

Professor Hennessy later held[2] that in a fluid globe the denser particles would sink through the lighter, while the lighter ones would ascend through the heavier, until an equilibrium of the mass would be reached, so that the globe "would consist of a series of spheroidal strata, each of uniform density throughout its own mass." He further remarked: "The exterior portions of the fluid would cool first, until they would acquire, according to the particular circumstances which may influence their cooling, certain densities. If the effect of refrigeration be in general an increase in density of the matter cooled, then the cooled portions of the fluid will sink. The higher temperature of the matter yet unexposed to cooling influence would then tend to change the acquired densities of the cooled matter. The as yet uncooled fluid would have its temperature reduced from contact with the cold particles from above, and it would tend to change its posi-

[1] Philos. Trans., 1839, pp. 381–423; 1840, pp. 193–208; 1842, pp. 43–55.

[2] Philos. Trans., 1851, pp. 495–547.

tion in a similar manner to the portions first cooled. As each cooled portion of the fluid descends, the three following causes would therefore impede its descent:

1st. From the arrangement already indicated of the denser strata about the center of the mass, and from the nature of the law of density of the strata, each stratum into which the cooled fluid would descend, would be denser than the preceding.

2d. If the general effect of the refrigeration be to increase the density of the cooled matter, each stratum would have its density augmented by the passage through it of the cooler matter from above.

3d. The descending portions will have their densities diminished by the increase in their temperature.

With an earth thus constituted for his starting point, Hennessy concluded that its crust could not be less than eighteen miles thick nor more than 600 miles. He further claimed that owing to friction, viscidity, and pressure it was probable that the fluid interior and the solid exterior would rotate nearly as if the earth was solid from its center, thus denying the validity of Hopkins' conclusions.

Professor Samuel Haughton, starting from the assumed premise that the earth was "composed of a solid shell, having the density of the rocks at the surface and a fluid homogeneous nucleus," also having a fluid heterogeneous one, deduced 768 miles as the probable thickness in both cases.[1]

The conclusions of Hopkins were sustained by Sir William Thomson. He starts with the hypothesis that the earth is composed of a thin shell which passes abruptly into a homogeneous incompressible fluid, mobile like water, which forms the interior. He also assumes that this liquid interior is heterogeneous and viscid. Taking for his starting points globes such as these, he concludes that the earth is more rigid than a homogeneous globe of glass and probably than one of steel.

Thomson further claimed that it was demonstrable that the earth solidified from the center, because melted rock contracted on solidifying, and that hence the crust would sink to the center as soon as formed.[2]

[1] Trans. Roy. Irish Academy, 1851, XXII, 251–273.

[2] Trans. Roy. Soc. Edin., 1864, XXIII, 157–169; Phil. Mag., 1863 (4), XXV, 1–14, 149–151; Phil. Trans., 1863, pp. 573–582; Thomson and Tait's Natural Philosophy, 1867, I, 670–727; Trans. Geol. Soc. Glas., 1878, VI, 38–49.

Thomson's views were republished and reaffirmed in 1872,[1] but in 1876 he says, regarding his previous argument from precession and nutation, as the result of a conversation between himself and Professor Newcomb: "Trying to recollect what I had written on it fourteen years ago * * * my conscience smote me, and I could only stammer out that I had convinced myself that so-and-so and so-and-so, at which I had arrived by a non-mathematical short cut, were true. He hinted that viscosity might suffice to render precession and nutation the same as if the earth were rigid, and so vitiate the argument for rigidity. This I could not for a moment admit, any more than when it was first put forward by Delaunay.[2] But doubt entered my mind regarding the so-and-so and so-and-so, and I had not completed the night journey to Philadelphia, which hurried me away from our unfinished discussion, before I had convinced myself that they were grievously wrong."[3]

Thomson, however, strongly affirmed the correctness of his views regarding the rigidity of the earth as determined by the phenomena of the tides. It is permitted, since he still retains his view that the crust when formed would sink, from its weight, to the center, to think that if he would affirm the precession and nutation theory, for fourteen years, so often as he had, without giving it sufficient thought, that possibly with the very imperfect tidal data at his command, he has not looked at this question in all its bearings.

Professor Hennessy indeed in 1872 pointed out that Thomson had assumed a spheroidal homogeneous elastic shell filled with incompressible fluid, and that all the latter could claim to have proved was, "that the earth does not consist of an elastic solid envelope enclosing a mass of the ideal substance called an incompressible liquid." Hennessy also justly calls attention to the fact that Thomson's method of proving the rigidity of the earth by assuming, first, that it is a homogeneous mass of glass, and again of steel, is an argument of the same kind, as if one should attempt to prove that rapid locomotion in railway trains was impossible, on account of the shocks the passengers would

[1] *Nature*, 1872, v, 223–224, 257–259.

[2] This is an error of Thomson's, since Hennessy advanced this view in 1851, seventeen years before Delaunay. See *Nature*, 1871, III, 420; *Geol. Mag.*, 1871 (1), VIII, 216–218.

[3] Report Brit. Assoc., 1876, XLVI (Sect.), 1–12.

be subject to, by assuming as his premise that the carriages were rigidly attached to one another.[1]

Delaunay further objected to the precession and nutation theory of Hopkins and Thomson, on account of the slowness of the motion and the viscidity of the liquid interior, which would cause the earth to act as if it were a solid body.[2]

From observations made on the deflection of the plumb-line by the Himalaya mountains, Sir George B. Airy held, "that the whole of that country is floating upon a dense fluid, and that the thick mass of the lighter mountain-matter sinks deep in the fluid, and that the displacement of denser matter neutralizes almost entirely the attraction of the lofty mountains. The form of the earth is not such as would be taken by a solid structure, but such as would be taken by a fluid mass with solids floating upon it."[3]

In the case of Mr. Geo. H. Darwin's papers, it is difficult in some cases to understand exactly what he regards as the constitution of his assumed mathematical globes, and to express his ideas in non-mathematical language. His investigations appear to be made on the suppositions that the globe is a viscous non-elastic spheroid, an elastico-viscous one, and one either elastic, plastic or viscous. All these, if he is rightly understood, are considered to be homogeneous. Darwin then states that the chief practical result of this paper may be summed up by saying that it is "strongly confirmatory of the view that the earth has a very great effective rigidity," the term earth being substituted for the hypothetical globes mathematically investigated by him."[4]

In one paper he says that the word earth is used as an abbreviation for "a homogeneous rotating viscous spheroid."

In a later paper "On the stresses due to the weight of continents," he assumes that the earth is a homogeneous elastic sphere, of which two conditions are possible, one that it is incompressible, the other that it is compressible. He then proceeds to show that in an earth thus constituted a state of stress must exist, owing to the inequalities between the continents and sea floors. From the discussion of this supposed condition, he says: "It appears that if the earth be solid throughout, then at a thousand miles from the surface the material must be as strong as

[1] *Nature*, 1872, v, 288, 289.

[2] *Geol. Mag.*, 1868 (1), v, 507–511.

[3] *Nature*, 1878, XVIII, 41–44.

[4] Philos. Trans., 1880, CLXX, 1–35, 147–593.

granite. If it be fluid or gaseous inside, and the crust a thousand miles thick, that crust must be stronger than granite, and if only two or three hundred miles in thickness much stronger than granite. This conclusion is obviously strongly confirmatory of Sir William Thomson's view that the earth is solid throughout."[1]

This statement of Professor Darwin seems to me misleading, for if his paper is understood aright, he has proved nothing of the *actual earth*, but only of a *hypothetical homogeneous elastic incompressible or compressible globe*, and for this assumed globe he has substituted the term *earth*, the same as the algebraist uses the terms x and y.

Is there a single geologist who believes that it is possible for this earth to have any such structure as that assumed for it by Thomson, Darwin and others? The difficulties placed by the physicists in the way of a belief in the earth's liquid interior, seem to be of their own making. These difficulties arise from the assumptions and limitations that the physicists have to impose upon the problem to bring it within the range of mathematical analysis. They have taken premises that no geologist would take, and having proved their point regarding these assumed premises, then claimed that they have proved it for this *earth*. What physicist has taken as his basis the most probable condition of the earth if its interior is liquid; a heterogeneous, viscid, elastic, liquid interior, irregularly inter-locked with and gradually passing into a lighter heterogeneous crust? The problem is believed to be beyond the power of any transcendental mathematics now known, and it is not believed to be possible mathematically to prove, at present, anything regarding the actual state of the earth's interior.

Our conclusions as to that state appear to be dependent on the evidence that can be derived from geological and petrographical studies. Professor Hennessy appears to have taken, as the basis for his mathematical discussion, data that are nearer the probable constitution of the earth than those assumed by any one else, and his results are entirely consonant with the hypothesis of a fluid interior.

It is as necessary that physical and mathematical discussions of the state of the earth's interior should conform to geological facts, as it is that geological theories should conform to physical and

[1] Philos. Trans., 1882, CLXXII, 187-230.

mathematical laws. It is incumbent on the physicist to explain earthquake motion, the sinking and rising of different portions of the earth's crust, volcanic phenomena, the uniformity in composition of lavas, the structure of volcanic rocks, sedimentation, faulting, vein formation, etc., etc., by his theory of a solid globe. Geological *facts* are just as positive as physical ones, and it is as necessary for the physicist to reconstruct his theory of a solid earth to suit geological facts, as it is for geologists to reconstruct their theories to suit the so-called physical demonstrations of a solid earth—demonstrations that merely show that under the assumed conditions and hypotheses the physicist's imaginary globe must be a solid one.

Are not speculations, speculations still, even if threaded together by a long series of mathematical formulæ, and are not all so-called mathematical proofs of the earth's solidity the mere working out of certain assumptions? Huxley was indeed right when he said, as the present writer would like to say: "I do not presume to throw the slightest doubt upon the accuracy of any of the calculations made by such distinguished mathematicians as those who have made the suggestions I have cited, * * * but I desire to point out that this seems to be one of the many cases in which the admitted accuracy of mathematical processes is allowed to throw a wholly inadmissible appearance of authority over the results obtained by them. Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff of any degree of fineness; but, nevertheless, what you get depends on what you put in; and as the grandest mill in the world will not extract wheat-flour from peascods, so pages of formulæ will not get a definite result out of loose data."[1]

Not only in mathematical and physical science, but in all science, no amount of discussion can give the conclusion any greater strength than the premises have, but attracted by the conclusion or by the chain of argument, the data are apt to be overlooked. Were this not the case many a structure, reared with great care and now regarded as established, would be found to rest on flimsy foundations.

So far then as the mathematical and astronomical determinations of the earth's solidity are concerned, it would seem that great doubt exists as to the correctness of the premises as applied to the earth, and hence like doubt regarding the conclusions.

[1] Presidential address. Geol. Soc. London, 1869, p. l.

When calculations shall be based upon the best data obtainable as to the materials of the earth's interior and their arrangement therein, then, and not till then, will mathematical calculations have real weight that geologists are bound to respect, but that day is a far distant one.

In the mathematical discussion of the state of the earth's interior, it has been customary to select a homogeneous globe of uniform density, but when the earth has been assumed to be heterogeneous and of varying density, it has been regarded as composed of layers, the layers being of unequal density when compared with one another, but of equal density throughout their own mass.

The more probable condition is that of a globe with a gradual increasing density from the exterior towards the interior, but with the materials heterogeneously arranged in the so-called layers. While the heavier materials would increase in abundance and the lighter diminish as the interior was approached, and *vice versa*, these materials at any one point would be found to be mixed together in varying proportions, the same as they are in meteorites and terrestrial eruptive rocks. While the extremes of the series may, indeed, be free from one another's components, the intermediate layers would contain more or less of the materials that predominate in the other portions of the earth's interior.

(To be continued.)

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(*Continued from page 594, June number.*)

The Evidence of the Earth's Solidity derived from the behavior of matter under the combined action of Heat and Pressure.—The key-note of this problem was given by Mr. William Hopkins in stating that if the tendency of the temperature to liquify the interior portion of the earth increases more rapidly than the pressure tends to solidify it, that interior would be in a state of greater or less perfect fluidity; but if the tendency to solidify from pressure is greater than the tendency of the temperature to prevent it, the earth would solidify from the center. In other words, whether the earth's interior is solid or not, depends upon the relative increase of the temperature and pressure, and on the behavior of the earth's materials under increased heat and pressure. Some experiments were made by Hopkins to ascertain the relation of solidity and fusion under the combined influence of heat and pressure on certain substances. He found that for spermaceti, wax, sulphur, and stearine as the pressure increased the point of fusion was raised, but irregularly, and with a diminished ratio, while metallic alloys showed no elevation of the fusion point. He did not regard his experiments as satisfactory, particularly those on the alloys.[1]

The subject was later discussed by Sir William Thomson, who held that from the "thermo-dynamic law of his brother, James Thomson, the earth must have solidified from the center outwards. This law asserts that all materials which contract on congelation have their melting point raised by pressure, while bodies that expand on freezing have their melting point lowered by pressure.

This law was experimentally verified by Wm. Thomson in the case of water. Hence, accepting Bischof's experiments as correct, which indicated that the earth's materials contracted from ten to twenty-five per cent on solidification, Thomson claimed that even if the internal heat was very great, the pressure would increase more rapidly than the tendency to liquify, and hence the earth must have a solid center.

The discussion then rests largely on the question of fact: Do

[1] Report Brit. Assoc., 1854, XXIV (Sect.), 57, 58; see also Bunsen, Ann. Physik Chemie, 1850, LXXXI, 562–567.

the earth's materials contract or expand on solidification? Mousson experimented further on the action of water according to James Thomson's law, and showed that it required enormous pressure to lower the melting point of ice a few degrees.[1]

So far then as any conclusion can be drawn from these experiments, it would show that the pressure must increase to an enormous amount to keep pace with the supposed increase of heat—a pressure that the earth's specific gravity negatives.

It can well be claimed that the known rate of the elevation of temperature, as the interior is approached, far surpasses the known rate of change of the fusion point by pressure in the few cases investigated.

David Forbes has further pointed out that, since the substances mainly experimented upon by Hopkins and Bunsen are not, except sulphur, components of the earth, experiments made with them do not necessarily apply to the materials of which the earth is composed. He also suggested that if the pressure is raised to great heights the reverse may be true regarding the effect of pressure on the melting point—it may lower instead of raise it. He also thought that we should look rather at the results obtained by Hopkins from the alloys instead of those from the wax, etc.[2]

In discussing the state of the earth's interior, Dr. T. Sterry Hunt advanced the argument that the material of the earth, when in its former fused state, would solidify from the center on account of the congealed mass being much heavier than the liquid: "We may say in a few words that the process of cooling in a mass like this would be just like the cooling of a great bath of metal or of sulphur; in other words, the condensation or congelation would commence at the center and extend outward toward the surface, so that the temperature of the center would therefore be the temperature of congelation."[3]

David Forbes, in reply to Dr. Hunt's argument, very pertinently remarked that no one "had ever seen a mass of metal or sulphur crystallize or solidify in the interior first, since the interior of such masses, it is well known, remain liquid after the

[1] Ann. Physik Chemie, 1858, CV, 161–174; Everett's Deschanel's Natural Philosophy, 1872, pp. 312, 313; 1883, pp. 331, 332.

[2] *Pop. Sci. Rev.*, 1869, VIII, 121–130; *Geol. Mag.*, 1867 (1), IV, 431–444.

[3] *Geol. Mag.*, 1877 (1), IV, 357–369.

crust has formed upon the surface; and furthermore, that the crust always remains upon the surface and does not sink."

He also remarked that "a crust of the specific gravity of 2.65 cannot sink deep down into the fluid mass of a globe possessing the mean density of 5.3."[1]

In looking over the data for the assumed contraction of the materials forming such portions of the earth's crust as we are able to examine, it will be seen that the early experiments of Bischof indicated a contraction in passing from the liquid to the crystalline state for basalt of some ten per cent, of trachyte eighteen per cent, and of granite twenty-five per cent; or tabulating the results, we obtain, according to David Forbes:[2]

VOLUME.

	Fused.	*Glass.*	*Crystalline.*
Basalt	1000	963	896
Trachyte	1000	888	818
Granite	1000	888	748

The question of the contraction of igneous rocks in cooling, was further taken up by David Forbes,[3] although none of his observations appear to lead to any more definite conclusions than that Bischof's results were much too high, and that his work was too crude to be of any great value.

In a later paper Forbes pointed out that in nature there was no evidence of a contraction of from one-tenth to one-quarter of the volume, such as Bischof had held, when the rocks in the form of dikes, etc., were studied *in situ*—a statement that probably every petrologist will sustain.[4]

Some further experiments on the contraction of siliceous materials in cooling were made by Robert Mallet. He claimed that the result showed "that the difference in specific gravity, less than that between ice and water, between red-hot but solidified or even cold slag (or analogous fused rocks) and the same in liquid fusion, is so slight that, coupled with the viscous or pasty condition which intervenes between the two states, it would readily admit of a thin or a thick terrestrial solidified crust being supported by and upon the surface of the liquid globe beneath, and

[1] *Geol. Mag.*, 1867 (1), IV, 431–444; *Chemical News*, 1868, II (Amer. reprint), 110–113.

[2] Neues Jahr. Min., 1841, pp. 565, 567; 1843. pp. 1–54; *Chemical News*, 1868, XVIII, 191–194.

[3] *Chemical News*, 1868, XVIII, 191–194.

[4] *Geol. Mag.*, 1870 (1), VII, 1–4.

lends no support to the view of terrestrial consolidation at the center first, by continual subsidence of such crusts, as imagined by Poisson, nor to the notion as to the nature of volcanic action which Sir W. Thomson has based on that assumption. * * * We * * * may be permitted to conclude that rocks consisting of acid silicates contract still less than those of basic silicates and that a terrestrial crust of the former is still more capable of floating upon the same in fusion beneath."[1] However much Mr. Mallet's views may agree with those of the present writer, it is but just to point to the fact that the slags experimented upon differ essentially from basaltic rocks, and it is not permissible to assume that one certainly gives the law for the other, however probable it may be that they act alike under similar conditions. This also applies in less degree to Mallet's comparison between plate glass and the rhyolites.

Further studies were made, in 1874, by Mr. Mallet on the alleged expansion of various substances on solidification. His experiment of filling a conical wrought iron vessel with molten iron seemed to show that liquid iron when heated far above its melting point has a specific gravity of 6.65, while the same iron in the cold state is 7.17. Mallet claims, therefore, that cold cast-iron is always heavier than molten iron. It is to be observed that no comparison was made between iron near the melting point and the same when just above that point.

He further tried some experiments regarding the flotation of solid iron upon molten iron, which must be regarded as conclusive upon that point. In all cases the iron either did not sink, or if when first immersed it sank, it afterwards rose again. Mallet says: "A piece of cold cast-iron which floats on liquid iron of its own quality, if forcibly thrust to the bottom and rapidly and at once released, rises again rapidly to the surface with all the *appearance* of a *buoyant* body, which it certainly cannot be."

The same experiments made with lead showed that it always sank when immersed in the molten lead. In this case when flat pieces were carefully laid upon the fluid lead they floated until the equilibrium was disturbed, so that the fluid could wet the upper surfaces when they sank. This flotation of the lead seems to be a case of the same kind as the flotation of a needle upon water. Mr. Mallet endeavored to explain the phenomena in the

[1] Philos. Trans., 1872, pp. 147–227.

case of iron and lead by the assumption of a repulsive force, much the same kind of explanation as "Nature abhors a vacuum," for here the repulsion exists for iron but not for lead. He seems to have lost sight of the true problem before him: Is solid metal at or near the melting point of a greater or less specific gravity than the same metal liquid at or near the point of solidification, and substituted for it the question is cold solid metal heavier or lighter than the same metal when heated to *any* temperature above the melting point. Surely the tendency of most bodies to expand on the application of heat would, in the case of iron and rocks, cause the difference between the cold solid and the liquid states, to be greater than it would be between the hot solid and the liquid states, a point that does not seem to have been properly appreciated in discussions of the earth's structure. Mallet's experiments show clearly that iron near the point of fusion is lighter than the same when melted, but that lead is heavier. He calls attention to one very important point: That owing to the difference in specific gravity in the various grades of cast-iron, it is necessary that in these experiments the liquid and solid should be of the same quality, otherwise real buoyancy or the reverse may occur from this cause. Mallet further seems to admit that liquid slag will allow solid slag to float upon it, either on account of a certain vesicularity, whatever care may be taken, or on account of his hypothetical repellant force.[1]

Some other experiments were made by Centner, W. J. Millar and Joseph Whitley.[2] The general result of these would be to show that cold steel, iron, brass and probably slag will sink in the same material when molten, but when hot and solid they become lighter and will float; also that lead and perhaps zinc possess a higher specific gravity when solid, whether hot or cold, than the molten metal.

In 1875 Mr. W. C. Roberts made some experiments by Mallet's method, on the density of silver in the cold state and in the molten condition. These showed that it was more dense in the former than in the latter state.[3]

[1] Proc. Roy. Soc., 1874, XXII, 366–368; 1875, XXIII, 209–234; *Nature*, 1874, X, 156, 157.

[2] *Nature*, 1877, XV, 529, 530; XVI, 23, 24; 1878, XVIII, 397, 398, 464.

Whitley's experiments are erroneously credited to Muirhead in Dana's Manual of Geology, 1880, 3d ed., p. 810.

[3] Proc. Roy. Soc., 1875, XXIII, 349, 350, 481–495.

Sir William Thomson in 1878 again took up the discussion of the earth's solidity, based on the assumed greater density of the solid than liquid rock. He seems at that time to have only been aware of Bischof's experiments, but in a note subsequently added by him, Whitley's experiments appear to be accepted as decisive against his views. Starting with the, to him, probable supposition that the solidified crust of the earth would sink towards the center, Thomson says: "As soon as the surface began to freeze, and to freeze in sufficient quantity not to be floated up by mere superficial solidified foam, the mass of rock would fall down towards the center. More would then solidify at the surface. This also would fall down, and the same thing would go on again and again. Gradually a sort of honey-combed solid would be formed. By-and-bye [*sic*] a skeleton or frame-work through the whole would mount up to an extent sufficient to build up piers, as it were, to the surface, and the spaces between these piers, when close enough, would, in the continued freezing of the lava, be bridged across by solid rock thick enough in proportion to breadth not to break down and sink. There would, again, be breaking away of the piers and upheavals of the liquid material below; but by degrees the honey-combed mass would become nearly like a solid throughout with comparatively small interstices of liquid lava. * * * The conclusion to be drawn respecting the internal condition of the earth is, that we are not to infer liquidity of the interior, even if we should find evidence of a much higher internal temperature than that which would melt the rocks under ordinary pressure."

The interior heat, Thomson states, "may be 4000° F., or 5000°. It may possibly be 8000° or 10,000°."[1]

In 1879 investigations were made by Messrs. J. B. Hannay and Robert Anderson on the expansion of cast-iron when solidifying. The chief method used was the flotation of a sphere of cast iron in a molten bath of the same. The latter was cooled near to the solidifying point and then the solid spheres dropped in. "They were found to sink at once when dropped in cold, and they remained under the metal till they had acquired a temperature just approaching visible red; but at that temperature they rose to the surface, and as they gained more and more heat from the liquid metal, their line of flotation rose higher and higher. Some-

[1] Trans. Geol. Soc. Glasgow, 1878, VI, 38–49.

times, if dropped in suddenly, the spheres did not float until they had begun to melt, but this was owing to their having cemented themselves to the bottom of the pot. When dropped in cautiously, or suspended by a wire, they sank only for the space of twenty to twenty-five seconds, and rose to the surface when barely red hot."

These experimenters found that liquid cast-iron expands at least 5.62 per cent of its volume in solidifying. A result that they regarded as under the truth, since the maximum density of the molten iron seemed to be little if any above the melting point, while they were obliged to use it at a temperature above the point of maximum density.[1]

Experiments upon the relative volumes, solid and liquid, of tin, lead, zinc, bismuth, cadmium, antimony, iron and copper have been made by Nies and Winkelmann. These show that tin, zinc, bismuth, antimony, iron and copper in passing from the fluid to the solid state expand; that is, if both the solid and liquid densities of the metals are compared when both are at a temperature near the melting point, the solid has the less specific gravity. Hence these metals when hot and solid would float on the liquid, and pressure, according to Thomson's law, would lower their fusion points, thus they would remain liquid at a lower temperature in the earth's interior than on its surface,[2] a fact which may assist in explaining the eruptive origin of some iron ores.[3]

Further experiments by Messrs. Roberts and Wrightson on bismuth and iron showed that "iron expands rapidly (as much as six per cent) in cooling from the liquid to the plastic state, and then contracts seven per cent to solidity, whereas bismuth appears to expand in cooling from the liquid to the solid state about 2.35 per cent.[4]

From the above detailed experiments and observations it is rendered quite clear that the chief portion of the metals expand in passing from the liquid to the hot solid state, both having their density taken near the melting point. That this is the case with the rocks, also, there seems but little doubt, although further

[1] Proc. Roy. Soc. Edin., 1879, x, 359–362.

[2] Sitz. Akad. München, 1881, pp. 63–112.

[3] Proc. Bost. Soc. Nat. Hist., 1880, xx, 470–479.

[4] *Phil. Mag.*, 1881 (5), xi, 295–299.

careful experiments are needed. Practically this seems to be the case in nature of the Kilauea lavas at least.

The question which now presents itself is: What are the probable components of the earth's interior?

So far as can be told from petrographical study, it appears probable that the portion of the interior mass lying nearest the center, concerning which we have any data, is composed of iron[1] either with or without nickel. As we recede from the central portions pyrrhotite is united with the nickel and iron. Then these minerals are found united with olivine or olivine and enstatite in varying proportions, until a portion is reached composed almost entirely of one or both of these silicates, with or without diallage. This portion passes on into the common basaltic rocks, then into the andesites and so on outward into the trachytic, rhyolitic, and jaspilitic forms.

However true this order may be for the liquid earth, that is, the liquid material would form rocks of this character, it is certain that in the solid portions these materials are interlaced now with one another in every conceivable way, and in the chemical and sedimentary deposits they have now been intimately mingled.

What may be the composition of the earth's mass nearer the center, if there be any, besides the iron and nickel, we have no clue, except that it may be some of the rarer elements now found mixed with the iron.

Now, while we know experimentally almost nothing of the behavior of such materials, as probably compose the earth's interior, under the combined action of heat and pressure, it seems most probable, from Thomson's "thermo-dynamic law" and the experiments on their relative hot solid and liquid densities, that the pressure to which they are subject would cause them to liquify at a lower temperature in the earth's interior than on its surface. It may also be justly claimed that if the earth's interior is solid, its liquefaction may be brought about by an increase instead of a diminution of pressure. In this way a sinking area loaded with sediments might thus liquify the rock beneath it. Is this not as consistent a view as the theory of liquefaction through the removal of pressure?

[1] Whitney's Metallic Wealth of the United States, 1854, p. 434, Judd's Volcanoes, 1881, pp. 307–324.

The best test of the relations in density between molten and solidified rock is apparently to compare the density of the rock just before fusion, or at least near that point, with the density of the same rock after melting. This would give a comparison between the crystalline and liquid states, while the usual method only affords a comparison between the liquid and the glassy or semi-glassy states. It would also save any error arising from cells in the cooled rock, if a solid mass was chosen in the first place. Again the fresh unaltered varieties of a rock should be chosen instead of such old and altered ones as those usually experimented upon.

In all discussions relating to the question of the liquidity of the earth's interior, it is to be borne in mind that the chief portion of our knowledge of the properties of liquids is derived from the study of water, a mobile liquid—while liquid rock, as lava or melted iron, is viscous, and its laws and properties may on experiment be found to differ considerably from those of water, under like conditions. Also in these solids the passage from the solid to the liquid state or the reverse is not abrupt as is the case with water, for every grade of viscosity exists between the normal solidity and the approximately perfect liquid condition. This is especially the case with iron and seems to be so for the common rocks.

(*To be continued.*)

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ON THE EVIDENCE THAT THE EARTH'S INTERIOR IS SOLID.

BY DR. M. E. WADSWORTH.

(Continued from page 686, July number.)

Conclusions.—Starting with the common belief that the earth was once an intensely hot gaseous body, it follows that when cooled from a gaseous to a liquid state, convection would cause the intermingling of all the liquid portions only so long as the heat kept every part at the same density. As soon as an especial difference in density manifested itself (if it had not already done so in the gaseous state) the heavier materials would sink towards the interior and the lighter pass outward towards the exterior. So soon as these materials became viscous the interchange would be retarded. Now, when convection no longer caused the heterogeneous materials of the earth to mingle, the cooling rate would change from the comparatively rapid rate of convection-cooling to the very slow rate of cooling by the conduction of liquids.

It is to be remembered that to have convection in liquids at all there must be some external source which shall, at some point, continually supply an increment of heat, but for a cooling globe no such supply exists. These are facts that ought to be taken into account in all discussions relating to the age of the earth or sun.

It would seem, however, that Thomson's view of the age of the earth is based upon the supposition that the earth during its liquid state was homogeneous and cooled throughout by convection, and that later it became solid and likewise cooled by the ordinary conduction of a solid body.

The writer would hold, in contradistinction, that after the earliest stages the liquid earth cooled by conduction in a heterogeneous liquid, and after the superficial crust was formed, by conduction not only through a heterogeneous liquid, but also a heterogeneous and, at least in its exterior portion, a more or less discontinuous or fragmental solid. In this way it would seem as if biologists might gain a portion, if not all, the time desired, which is now denied them by the physicist.

In the same way, if the heavier gases tend to lie nearest the center in a hot gaseous body, the exceedingly slow rate of cooling on account of the poor conductivity of gases ought to be taken into account in all discussions relating to the age of any body formerly gaseous. Another factor would be the heat disengaged by the chemical unions necessary to form the mineral combinations, now existant on the earth, out of the once disassociated gases.

But to return; when the lighter surface material of the earth had cooled sufficiently, a crust would be formed which, owing either to its lighter state in its hot condition, or to its scoriaceous character and the viscidity of the material beneath, would not sink. It is to be remembered that on account of the passage of rocks through the softened or viscous state to the solid, that the viscous material immediately below the solid crust would be in nearly the same condition and temperature as the overlying crust into which it would gradually pass. It is not probable that the crust would break up and begin to sink, because even if its surface grew cold it would always have this hot, solid base, lighter than the underlying viscous liquid, which, owing to the increasing specific gravity as the interior was approached, would probably be more dense than any of the overlying cold crust.

Even if the crust should become heavier, break up and begin to sink, this sinking would be very slow on account of the viscosity of the liquid and its constantly increasing density, while the heat imparted to the sinking crust would tend to bring it to about the same specific gravity as the liquid, as the sinking mass neared its melting point. But above and beyond all, it would soon reach a point at which the liquid, being of different composition, had a higher specific gravity than the crust, and no farther sinking could take place. We should thus expect to have formed on the earth's surface a crust which never would sink, or if it sank at all, would for only a comparatively short distance, giving rise at that point to a solid crust floating upon a denser heterogeneous liquid. While willing to admit that the crust when cold would be heavier than the liquid out of which it was formed, it is denied that the exterior would cool to such an extent as to be heavier until solidification had taken place to sufficient depth to render the contraction of the exterior portion of but little effect; that is the increased density of the liquid immediately beneath the hot lighter interior portion of the crust would more than counterbalance the increased density of the cold exterior portion of that crust.

Sir William Thomson's idea of a crust on solidification sinking to the center of the earth and building up a honey-combed mass, is only applicable to a homogeneous liquid globe of but slight viscosity, whose material contracts in passing from the liquid to the solid state.

In such a condition of the earth as the writer supposes, a gradual passage from the cooled surface crust towards the hotter interior portions of that crust, thence into the plastic and viscous condition, no opportunity would exist for the generally supposed shrinking away of the nucleus from the rigid crust, but the entire earth would contract as a whole, causing a linear shortening of the crust through compression. This would occasion a crushing together of this crust, causing it to be depressed in some places and elevated in others. The depression of any portion of the crust into the viscous liquid beneath would cause the elevation of an equivalent weight of the liquid material; as in the case of ice, the depression of the ice on one side causes the heavier water to overflow unless it can escape in some other direction. The simple sinking of a portion of the crust on one side with its corresponding but less elevation on the other, with the attendant fissuring, affords all the dynamic agent needed to raise lavas to the top of the highest mountains;[1] while if in any way the yielding to the lateral compression should be sudden, instead of gradual, owing to fracturing and slipping of the parts, an earthquake shock would result.

If the general views of the compression of the material in the interior of the earth are correct, then if from any cause the pressure were removed, the natural expansion of the material, if liquid, would cause it to rise to some extent in any vent or opening.

During the earlier times when the crust was thinner and the internal heat stronger, a greater variety and amount of materials raised as lavas through the fissures would be expected, and not improbably outflows of two different kinds might take place at the same time, as it would seem had taken place on Lake Superior.

The up thrust of the still liquid and yielding interior portions through the fissures in the overlying crust, and the subsequent solidification of the intruded material, would cause that crust to be tied through and through with the underlying mass.

Neither is it to be expected that the contraction would be equal in every portion, while the depression of the crust into the interior would give rise to unequal thicknesses of that crust, to which the liquid outflows would add. The great irregularity of the under surface of the crust, coupled with the gradual passage from the solid to the viscous liquid interior would conspire to prevent any of the supposed slipping of the crust over the interior, as many physicists have assumed would take place if the earth had a liquid interior.

If it is held that volcanic rocks are derived from the re-liquefaction of the original crust of the earth, would not the best theory be, in the light of what is now known of the behavior of

[1] Whitney, "Earthquakes, Volcanoes and Mountain-building," p. 90.

rocks on their solidification, that increased pressure, brought about by contraction of the crust in cooling or by sediments deposited on a sinking area, or by some other cause, produce a lowering of the fusing point, as in the case of ice, and thus enable the natural heat of the rocks themselves to cause their passage into the liquid state?

It has been claimed with apparent justice that the simple depression of any portion of the earth's crust into the still liquefied portion of its interior, would tend to cause the base of the depressed portion to re-liquefy through the greater heat to which it would be then subjected to, thus making the re-fusion the natural result of the earth's contraction.

It appears to the writer that so far as any evidence now exists regarding the earth's interior, it is allowable to assume its present liquid state. A state that in his judgment accords better with the facts of petrography than any other assumption that has been made.

It is true that if the materials of the earth's interior were solid, but could be liquefied by diminution or increase of pressure, this liquefaction would perhaps be consonant with what is now known of the internal structure of rocks, especially the partial dissolving of the olivine of basalts, the hornblende of the andesites, the quartz of the rhyolites, etc. One of the greatest difficulties in the way of this supposition is to understand why the same lava should produce different crystals when it was in the interior from those yielded on the exterior of the earth.

It is difficult to see how, if the earth is solid, that any relief from pressure could take place otherwise than from the crushing together of the overlying rocks, the tearing up of these from the underlying ones, and elevating them into the air; that is, the relief from pressure would come from an elevating instead of a sinking process. In truth it would seem that eruptions and mountain building or elevations arose rather from the sinking of large masses causing smaller ones adjacent to rise, or, as announced by Dana, the highest border is on the side of the greatest ocean.[1] It would seem that elevation followed subsidence, instead of subsidence following elevation. If this is the case, it is difficult to explain how subsidence could be brought about first in a *solid* globe.

[1] "Man. Geol.," 1880, p. 28.

We cannot imagine that matter so rigid as the earth's interior is claimed to be, could yield to the pressure of sediments, glaciers or lava flows, as has been advocated. This view is based chiefly on the fact that areas of thick detrital formations must have been areas of subsidence, hence, it is argued, the deposit itself has been the cause of the sinking. The reverse appears rather to be true, that only areas of extended subsidence can be areas of great deposition. May it not then be claimed that the subsidence was the cause of the deposition instead of the deposition being the cause of the subsidence; and is not the former view more natural than the latter?

The deposition of sediment in any locality requires that one portion of the earth's crust should be lower than another. In the theory of a solid globe this would be brought about by the elevation of a portion of the crust, while in the theory of a liquid globe by the depression of a portion of that crust.

In a viscous mass, such as the earth's interior next the crust is here supposed to be, coupled with the irregular thickness of the crust, no especial connection could be expected to exist between different vents, even if near one another, until after the lapse of considerable time — the viscidity itself preventing any rapid motion of the interior mass.

Whatever water was met, on the welling up to the surface of the lava, would naturally render the latter more liquid, so far as it entered into the lava. The intervention of water in a volcanic eruption seems to be mainly its action on the lava during its passage upwards, instead of its being the cause of the eruption. It, indeed, plays a striking rôle in volcanic phenomena, but it does not seem to be the *primum mobile.* It is difficult to see how lava in ascending to the earth's surface could reach it without meeting water somewhere on its way. When the water was met could the results be different from those now witnessed? Does it not seem that water is the accident rather than the cause of the eruption, and do not most observers transform an effect into a cause?

It may be said that the physical evidence advanced in behalf of its essential solidity is violated by the premises and limitations chosen as the basis of the mathematical discussion; while the petrographical and geological facts demand either an interior that is liquid or one that can readily become so.

It may indeed be said with Professor Dana: "Among geological facts none appears to demand for its explanation a rigid globe. The demand has come through the supposed requirements of physical laws, studied with the aid of the highest mathematics, whose methods and conclusions are sure only when all the modifying conditions of the problem are thoroughly understood.

"It is now admitted by some of the best of physicists that no arguments have yet been presented which prove the earth to be a rigid globe, or to have a rigid crust a thousand miles or so thick; and it is also admitted by some mathematicians and physicists of eminence, including Airy, the astronomer royal, that the hypothesis of a thin crust over a liquid interior is probably the true one.

"The science of geology is, therefore, free to adopt the conclusion which seems best to suit known facts."[1]

For further discussions of the state of the earth's interior the reader is referred to

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[1] "Man. Geol.," 1880, p. 812; see also Whitney's "Earthquakes, Volcanoes and Mountain-building," 1871, p. 74.