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"Post-Cambrian Igneous Rocks of older date than the Great Thrust-Movements of the Region: their Petrography," pp. 440-52.

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"The Eastern Schists." General Description (with J. Horne), pp. 595-601.

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1897. "Geology of Cowal." General Remarks on the Petrography of the Cowal District, pp. 295-9. Description of an Albite Schist, pp. 299-300.

3. *Sheet Memoirs.*

Ireland.

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Scotland.

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1907. Sheet 20 (Islay, etc.).

England and Wales.

1902. Sheet 325 (Exeter).

1904. Sheets 355 and 356 (Kingsbridge and Salcombe).

1907. Sheet 353 (Mevagissey).

II.—ON CONVECTION CURRENTS IN THE EARTH'S INTERIOR.

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

IN this Magazine for November Mr. Mellard Reade referred to the difficulty of imagining the adequacy of any known force to accomplish such tremendous effects as to produce overthrusts like those which have been lately described as occurring in the North-West Highlands,¹ and he consequently appears to be a little doubtful of the reality of the phenomenon. I think, however, that the conclusion of so many competent field geologists must be accepted, and that our theories of mountain-building must be made to fit the facts rather than the facts the theories.

An overthrust is only one of the ways in which a region of the earth's crust may be compressed into a smaller area by horizontal pressures acting in opposite directions, and enormous as the stress must be, it does not seem that it need be greater to cause an overthrust than what would be required to produce the folding and consequent elevation, which is the more usual manner in which the compressing force has been satisfied; for if we suppose the mean elevation of a tract produced to be the same in the two cases, the work against gravity would be the same, and what we have to compare is, in the one case the friction along the thrust-plane, and in the other the bending and fracturing of the rigid rocks. It is not obvious that the former need require more force than the latter.

I have elsewhere endeavoured to explain the mechanism of an overthrust by the analogy of what may be observed in the behaviour of ice on a frozen river.² If the edge of a piece of floating ice gets

¹ Survey Memoirs, *The Geological Structure of the N.W. Highlands of Scotland*, 1907.

² *Physics of the Earth's Crust*, 2nd ed., p. 321.

to overlap the edge of a fixed sheet, the stream pushes the former onwards and causes an overthrust. But if an overthrust in the rocks is produced in a similar manner, we must postulate a liquid substratum, whose surface is locally moving horizontally; and if the earth is a cooling body, and if there is liquid matter in the interior, this kind of movement must needs occur. The reason of this is that the mode of cooling of a mass of liquid is by convection, the currents ascending in some places and descending in others, while the liquid flows horizontally at its surface from the ascending to the descending portions.

I am aware that at the present day (at least in Britain) a belief in the liquidity of the earth's interior is heresy. The orthodox opinion appears to be that it is neither liquid nor solid, but both at once, and so we run with the hare and hunt with the hounds. But such a state of affairs would not admit of convection currents.

I will briefly mention some phenomena which appear to be explicable on the hypothesis of a liquid substratum holding water-gas in solution and cooling by convection—

(1) Obviously volcanic action stands first, where we see frequent ebullitions of fresh lava, varying in character from one eruption to another, and building up great mountains as if supplied from an inexhaustible reservoir below.

(2) The same agency, viz. horizontal flow, which, as above suggested, might cause compression to be satisfied with an overthrust, would also account for other forms of compression, such as are more usually characteristic of mountain ranges.

As to whether the friction of a flow of the substratum would be adequate to supply the force which has compressed the crust in mountain ranges, we may again refer to the analogy of ice. Nansen found that in the Polar regions the thickness of the ice under direct freezing does not usually exceed 4 metres. But it becomes very much thicker by the piling up of broken ice-sheets, driven together and mounting one above another, and he says that the pressure is largely dependent upon the tidal current, especially at the margin of the ice-field.¹ Now the ratio of the densities of the crust and substratum is probably very close to that of ice and water, so that a similar action of currents in the two cases would be likely to produce similar effects of compression.

(3) Variations of level, especially such as have been described by Professor Hull and Dr. Spencer, appear to be most readily accounted for by convection currents, the surface rising over upward and sinking over downward currents, as I have endeavoured to explain elsewhere.²

(4) Another phenomenon which may possibly be due to convection currents is the irregularity in the small changes of latitude which arise from slight variations in the position of the axis of rotation within the earth. Mathematicians tell us that the anomalies of these movements can be explained by elasticity. But it seems that, although elasticity might modify the period of a regular movement of the pole of

¹ *Nature*, February 11, 1897.

² *American Journal of Science*, 1906.

rotation around its mean position, it would not explain the remarkable irregularity both in amplitude and period with which its wanderings are affected.¹ Professor Milne is of opinion that some connection exists between these irregularities and the frequency of large earthquakes.² Might not convection currents, by slightly altering the distribution of mass within the earth in an irregular manner, account for these irregular wanderings of the pole?

(5) Observations upon the force of gravity in India have revealed the remarkable result that changes appear to have taken place within the last thirty years. At Dehra Dun, at the foot of the Himalayas, there has been found a notable increase of gravity corresponding to four swings of the second's pendulum in twenty-four hours.³ If the two sets of observations that show this are to be depended on, we have here an absolute proof that there has been a change of density and consequently movement in the substratum of that neighbourhood; and it is noticeable that, in the interval between the two dates, a severe earthquake was experienced at that place.

(6) Making use of the best data newly available, I have calculated by a mathematical method that, on the hypothesis that the crust of the earth is being formed by congelation out of a liquid subjacent magma, the age of the world would not be greater than about eight million years unless there are convection currents in the interior.⁴ Dr. See, Professor of Mathematics to the U.S.A. Navy, has accepted my calculation, and also the conclusion at which I have arrived regarding the corresponding age of the world, and, not believing in the existence of convection currents, he has been led to argue that geological phenomena do not require a longer period for their accomplishment than about eight million years.⁵ Few geologists, I think, will be willing to accept so short a time. It follows that we have the opinion of an accomplished mathematician to endorse the result of my calculations, which, on any reasonable estimate of the world's age, go far to prove the existence of convection currents.

The two principal objections to the hypothesis of a liquid interior appear to be (1) the existence of a fortnightly tide, and (2) the two phases of the preliminary tremors of a world-shaking earthquake. The argument for rigidity from the fortnightly tide is so extremely technical and refined that it is not for the ordinary geologist to follow it.⁶ It is based on the average of observations taken in India, Britain, and France;⁷ but, confining our attention to India, it seems doubtful whether that tide can be detected.⁸

The argument for rigidity from the different velocities of the two phases of the preliminary tremors of an earthquake assumes that

¹ *Nature*, February 27, 1896, also May 12, 1898.

² British Association Report, 1903 and 1906.

³ Colonel Burrard, *Phil. Trans.*, vol. clv, A 394, p. 294.

⁴ *Physics of the Earth's Crust*, 2nd ed., Appendix, ch. xxvii.

⁵ *Proc. American Phil. Soc.*, 1907, vol. xvi, p. 221.

⁶ Thomson & Tait's *Natural Philosophy*, 1883, pt. ii, p. 442.

⁷ Knott's *Physics of Earthquakes*, p. 104.

⁸ *Physics of the Earth's Crust*, Appendix to 2nd ed., p. 34.

the second phase is propagated as a distortional wave, for, "among material bodies, a solid alone really possesses the rigidity sufficient for the production within it of transverse vibrations and for their maintenance during their propagation."¹ But I have suggested an alternative explanation of the two phases, depending upon the presence of water-gas in the magma, which, from volcanic eruptions, we know to be a constituent.²

The cause usually assigned for the supposed rigidity of the interior is the enormous pressure to which it is exposed. It is held that this would so increase the viscosity as to render it practically solid, but I am not aware that any experimental fact has shown this to be probable; for it is a different question from that of the melting temperature being raised by pressure. We know that in the case of a vapour there is a certain critical temperature, above which no pressure will reconvert it into a liquid. Is it not possible that in the case of a liquid there may be a critical temperature above which no pressure will convert the liquid into a solid? And this may be the case in the earth's interior.

III.—ON THE AFFINITIES OF THE TRIASSIC PLANT *YUCCITES VOGESIACUS*, Schimper & Mougeot.

By E. A. NEWELL ARBER, M.A., F.L.S., F.G.S., Trinity College, Cambridge; University Demonstrator in Palaeobotany.

IN a paper published in 1907, in the Transactions of the Linnean Society of London,³ I described and figured, under the name *Zamites grandis*, sp. nov., some excellent specimens of detached leaves recently collected by my friend Mr. L. J. Wills, B.A., F.G.S., from the Keuper rocks of Bromsgrove (Worcestershire).

These fossils were regarded as identical with the specimens from the Bunter of the Vosges, first described by Schimper & Mougeot⁴ under the name of *Yuccites vogesiacus*.

In discussing the affinities of this plant I pointed out⁵ that this question had been "a subject of much confusion in the past, and the cause of considerable difficulty at the present". The difficulty lay in the fact that these large, lanceolate, Monocotyledonous-looking leaves, with close, stout, and strictly parallel nerves, were almost invariably detached, and with one possible exception, which, however, was not very conclusive, there was no direct evidence, either from the British or the Continental examples, as to the manner in which they were borne. Consequently direct evidence as to their affinities was lacking.

On the other hand, these leaves presented a very close resemblance to the pinnae of a Zamitean frond except in size, in which respect they were very dissimilar. I concluded, however, that they were the detached pinnae of a Zamitean frond of large size.

¹ Poincaré, *The New Physics*: International Scientific Series, 1907, vol. xc, p. 173.

² Proc. Cambridge Phil. Soc.

³ Arber, Trans. Linn. Soc. London, 1907, ser. II, Bot., vol. vii, pt. vii, p. 109.

⁴ Schimper & Mougeot, Monogr. Plant. foss. Vosges, 1844, p. 42, pl. xxi, figs. 1 and 2.

⁵ Arber, *ibid.*, p. 115.