THE THERMAL STATE OF THE EARTH.

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ABSTRACT. The available data related to the internal heat of the earth are reviewed. From these considerations it is concluded that: (1) The average radioactivities of surface rocks show a steady decrease with increase of density, though there is much scatter within a given type. (2) Heat generated at depths up to 300 km. or so has had sufficient time to approach a state where it is conducted out as rapidly as it is produced. (3) Nothing in the new results encourages the hope that fusion temperatures are a normal feature at any depth within the crust. (4) The thermal contraction available for mountain formation remains essentially unchanged except that a more uniform rate is suggested.

A MOST important group of geophysical data concerns the rate of heat outflow from the earth, its age, the radioactivities of rocks, the existence of folded mountains, and the existence of volcanoes. The first attempts to combine them into a single theory were made by Holmes[1] and me[2] in 1915-16, and I still think that the general principles stated then are valid, but the accumulation of additional quantitative data has made revision of the numerical results necessary at intervals. At present the data with regard to some aspects of the problem are so abundant that it is difficult for a single worker to attend to the whole of them, but for others they are scanty. Some very recent work has much improved some of the latter.

The early work of the present Lord Rayleigh, then R. J. Strutt, revealed the presence of small amounts of radium in all rocks examined, which must be continually liberating heat. This radium is being continually regenerated from uranium, and provides an estimate of the quantity of the latter present. Each stage in the degeneration of uranium to lead^{206} involves such a production of heat. The intermediate products, of which radium is one, are breaking up as fast as they are reformed, so that the ratios of their amounts to the amount of the parent uranium remaining are practically constant; and since their radioactive properties are well known, it is possible, given the amount of any member of the series, to infer the rate of generation of heat by the whole series. Another element,

[1] Geol. Mag. 1915, 102-112.
[2] Phil. Mag. 32: 575-591, 1916.

thorium, is at the head of another radioactive series, and is abundant enough to give, with its descendants, about as much heat as the uranium series. The abundant element potassium is also feebly radioactive and makes an appreciable contribution. One of our tasks is to say what becomes of this heat.

Measurements of temperature in mines and boreholes show an increase downwards, which implies that heat is being conducted upwards and radiated away from the earth's surface. In comparison with the sun's radiation the amount is exceedingly small, and the earth's surface temperature can be regarded as maintained entirely by the latter. This requires repetition because it is still sometimes thought that internal heat has an appreciable effect on surface temperature. According to recent determinations the heat outflow is about 10^{-6} cal./cm.^{2} sec., while the sun supplies about 0.008 cal./cm.^{2} sec. on an average over the earth, 8000 times as much. With Stefan's law of cooling, even if the internal heat were cut off entirely, the surface temperature would only fall by about 0.01°. But these measurements of heat outflow are of the first importance with regard to the distribution of radioactivity with depth, and to the thermal state of the interior. The point originally made by Rayleigh is that a few tens of kilometres of average surface rock would supply the whole of the outflow. Bullard estimates the heat production from average granite as 0.74×10^{-12} cal./cm.^{3} sec., so that about 13 km. of granite would suffice.

We have, therefore, three alternatives: either the radioactive elements are practically confined to a thin surface layer; or their radioactivity is inhibited more than 13 km. or so down; or heat generated below that level is somehow prevented from getting out. The second and third seem quite inadmissible. Direct experiment has shown that pressures and temperatures such as are likely to occur within a much larger range have no significant effect on radioactivity. This excludes the second alternative. With regard to the third, we know enough about the conductivities of rocks to say that heat generated 10^7 years ago has had time to reach the surface if it was at any depth less than about 30 km.; 10^9 years ago if its depth was less than about 300 km. These are representative of the times available, and show that accumulation of heat, as such, is impossible at the depths required. There is only one suggestion of a transformation of heat into some other form of energy that might be retained, namely, that of Joly, that fusion might occur and the heat become latent. But then further transfer of heat would be by convection, a much more rapid process than conduction, and this suggestion makes matters worse. There seems to be no alternative to supposing that heat generated at depths up to 300 km. or so has had time to approach a state where it is conducted out as fast as it is produced. We therefore come back to the first alternative, that radioactivity as intense as that of granite must be confined to a layer not more than about 13 km. thick. If its intensity falls off with depth, a greater depth is permissible, but we cannot suppose a greater total amount. I therefore maintain the principle that Holmes and I used in our earlier papers, that our firmest datum is the equation of heat conduction; it is right if the matter down to 300 km. is solid; if there is a liquid layer above that level, the amount of radioactive matter permitted by the heat outflow may be diminished, but cannot be increased.

This point needs emphasis because many experimenters, finding traces of radioactivity in even the densest rocks tested, such as dunite and eclogite, have treated their specimens as typical of the rocks at great depths and reconstructed Rayleigh's paradox. Dunites, for instance, are about a third as radioactive as what has been treated above as an average granite. If such radioactivity extended down to the core (nearly halfway to the centre), we should again have much more heat than is being conducted out, and the difficulty is reinstated. Now it seems to be often supposed that rocks of given geological types have immutable compositions like chemical compounds. This is not true. Even with regard to the major constituents there is considerable latitude of composition within what geologists would call granites or basalts; in the minor constituents there is relatively much more. It should really be enough to say that the difficulty in this form contradicts the equation of heat conduction and *therefore* has arisen from an assumption of too much radioactivity at depths between 13 and 300 km. It is a physical question to say *why* it should be concentrated upwards to the extent indicated. But even if this question is not answered, the omission does not throw any reasonable doubt on the statement that it *is* so concentrated.

Actually we have now a very plausible explanation, due to Goldschmidt. The structures of silicates are mainly determined by the possible arrangements of the oxygen atoms. Metals can form part of silicate crystals if their atoms have the right size to fit into the interstices between the oxygen atoms. If they are too large, they cannot enter the crystals; if they are too small, they are, apparently, still unable to stay there. Now the atoms of U and Th are very large, and Goldschmidt shows that the interstices are too small to accommodate them. Hence when a silicate magma crystallizes these elements would be expected to stay in solution to the last. Since the crystals tend to settle to the bottom, the radioactive elements would be concentrated to the top. This argument, intrinsically plausible, is confirmed by the observation of Pigott that uranium and thorium in rocks do not occur in the interiors of crystals, but on the interfaces, where the crystal structure is defective, and on the cleavage planes of the micas.

This explanation does not tell us how fast radioactivity should be expected to decrease with depth. For that we must still refer to the earth itself. We have information from seismology about crustal structure, which should help us a little, but in the last resort we must still use the equation of heat conduction. The rate of decrease is not directly determinable, because we cannot sample the rocks 10 km. down; nor can we trust the earth to have sampled them fairly for us in transporting igneous rocks to the surface, because such rocks have been fused and resolidified, and in such circumstances there is every reason to suppose that the specimens accessible to us are precisely those that have had their radioactivity most increased in the process. The presumption is, in fact, that every rock type occurring at the surface is more radioactive than the average for rocks of that type, where they lie undisturbed at their average depth (if, indeed, they occur anywhere except near the surface). In seismology the model usually considered consists of an upper layer and an intermediate one, each of the order of 15 km. thick, the latter resting on a lower layer extending down to the metallic core. Geological considerations, combined with laboratory measurements of the compressibilities of known rocks, compared with the compressibilities found from the observed velocities of earthquake waves, suggest that the upper layer may be granite, the intermediate one glassy basalt (tachylyte), and the lower olivine (dunite). There are difficulties about the seismic evidence concerning the intermediate layer, but it seems very unlikely to be crystalline basalt (dolerite, diabase, gabbro); it might be some rock consisting mainly of plagioclase. The identification of the lower layer with olivine is checked in an interesting way by the moon, whose density would agree within 0.01 with that of the standard specimen of olivine used by L. H. Adams and R. E. Gibson, if the compression due to its own weight were removed. For many purposes the actual identifications are not very important, because the elastic moduli are so closely correlated with density that any alternative rock type consistent with the seismic evidence must also have nearly the same density.

Similarly the average radioactivities of surface rocks show a steady decrease with increase of density, though there is much scatter within any single type. This may be supposed to hold also for deep rocks, though we must beware of assuming that the actual values are the same. But where we find granite at the surface over extensive regions, it seems fair to take it as representative of the top of the upper layer. It is doubtful whether surface basalts correspond to any of the layers; they may come from a transition zone at the base of the intermediate layer, and in any case have probably been much enriched in reaching their present positions. Surface dunites are also unsatisfactory. They do not appear to have come directly from the lower layer, but to be the result of the settling of olivine crystals from magmas of approximately basaltic composition. Such magmas would have been enriched; the least radioactive part, then, has been chosen, and it is exceedingly doubtful whether the resulting dunites are more or less radioactive than the material in its original position. In any case, their radioactivity cannot be taken as a standard for the lower layer. In fact, we really have no information to tell us how fast radioactivity should decrease with depth except what is provided by the equation of heat conduction. Two types of distribution have actually been applied in this equation. A formal solution given by Ingersoll and Zobel in 1913 assumed that it falls off exponentially, and this form was used by Holmes. I assumed in 1916 a uniform radioactivity in an upper layer, with none below it (the three-layer structure was not indicated by seismology until about ten years later). In "The Earth" (1929) I took a uniform upper layer, resting on an intermediate one with the radioactivity of average basalt and no radioactivity in the lower layer. This, however, is still rather unsatisfactory, for the above reasons; the layers are very unlikely to be uniform and transition zones are to be expected, so that the truth may be much nearer to the exponential type. Accordingly, I think that until some theoretically satisfactory way of determining the variation with depth is produced, we cannot do better than return to the exponential rule of Ingersoll and Zobel. In any case, we have only one equation and hence cannot determine more than one unknown.

The data chiefly needed for substitution in this equation are the rate of outflow of heat and the rate of generation of heat by average surface granite. The former is the product of two factors, the vertical gradient of temperature and the thermal conductivity of the rocks that it is measured in. Renewed attention has been given to these quantities during the last five years. In "The Earth," and in earlier work, the information was available only in such a form that workers in the subject could merely take averages and hope for the best. It was somewhat doubtful, moreover, whether averages had any particular meaning since separate determinations differed by factors of 4 or more. In 1936 I undertook a classification of the data for radioactivity of rocks[3] and found that though separate specimens of the same type differed greatly, they did fall into a simple form of distribution, expressed by Karl Pearson's Type III law. In this the chance of an observation in a range dx is given by

$$ydx = \frac{1}{p!}\frac{x^p}{a^{p+1}}e^{-x/a}\,dx$$

where a and p are parameters of the law. The expectation of x is $(p+1)a$ and is estimated directly from the mean of the observed values. Large values of p correspond to close concentrations of the chances about the expectation, small ones to large scatters. For granite the radium determinations gave $p=2.7\pm0.5$, thorium 2.3 ± 0.6; for basalt, Ra 6.8 ± 0.7, Th 3.7 ± 1.2; plateau and oceanic basalts, $p=30$ ($1/p=0.033\pm 0.006$). The granites from North America, Greenland, Iceland, the British Isles and Japan fell into a single distribution with mean radium content $1.59\pm0.12\times10^{-12}$ by weight, thorium $0.81\pm0.08\times10^{-5}$. It seems that these values are at present the best to take as representative of surface granite. The

[3] Gerlands Beiträge z. Geophysik 47: 149-170, 1936.

data include some old determinations, some of which are queried by recent experimenters, but until more modern methods have been applied to an equally wide range of localities, or possible systematic errors of earlier workers determined by direct comparison of new and old methods on similar materials, we must make the best of them. The data did not in themselves suggest any systematic differences between different investigators. The means are considerably less than those that I adopted from Holmes in "The Earth," namely, about 3.0×10^{-12} and 2.0×10^{-5}. This is chiefly because I have excluded numerous determinations for Finland and the Alps, which refer to very limited regions. R. D. Evans has also pointed out that some of them have a serious systematic error. (Bullard has quoted from me the values 2.38 ± 0.16 and 1.76 ± 0.22; but these were the preliminary ones given at the beginning of my 1936 paper before the need to treat regions separately had been discussed.) The potassium of granite may be taken as 3 per cent. Then the rate of generation of heat by the three series may be taken as 7.4×10^{-3} cal./sec per gram of radium, 0.72×10^{-8} cal./sec per gram of Th, and (following Bullard) 3×10^{-13} cal./sec per gram of potassium. The total per gram of granite is then 1.92×10^{-13} cal./sec, or, with a density of 2.63, $5.07 \pm 0.26 \times 10^{-13}$ cal./cm.^{3} sec. Bullard quotes from Immelman the value $2.36 \pm 0.16 \times 10^{-12}$ for the mean radium content of South African granites, and his result, 7.4×10^{-13} cal./cm.^{3} sec, is probably valid for them. These results are much less than the 13×10^{-13} cal./cm.^{3} sec given in "The Earth"; the difference is due partly to the exclusion of exceptional series and partly to the fact that Bullard has divided Holmes and Lawson's estimate of the heat generation by potassium by a factor of 10.[4]

The conductivities of rocks differ greatly, and so does the temperature gradient in the crust. But the disturbing feature was that, in spite of the fundamental importance of the heat outflow, which is the product of the conductivity and the temperature gradient, so little attention had been given to it that not a single case could be found where both factors had been determined for the same place. This made the study of the variation quite impossible, and the validity of the product of means as an estimate of the mean of the product was doubtful.

[4] Holmes and Lawson gave 3.9×10^{-12}; Bullard's reduction (not yet published) corresponds to a redetermination of the average life of Th by A. Bramley and A. K. Brewer, Phys. Rev. 40: 502-505, 1938.

The British Association therefore set up, in 1935, a committee "to direct determination of the thermal conductivities of rocks in mines and borings where the temperature has been, or it is likely to be, measured." Valuable work stimulated by this appointment has been done by E. C. Bullard,[5] A. E. Benfield,[6] L. J. Krige,[7] and E. M. Anderson.[8] Samples have been taken for different depths in each bore, and their conductivities found; and the temperature gradients have been compared not only with the mean conductivity but in detail. A boring, that is, may traverse strata of several different conductivities; but if there is no appreciable generation of heat within the levels traversed, the heat outflow should be the same at all levels, and hence the product of the conductivity and the gradient at the corresponding depth should be the same at all levels in each bore. This has been checked for bores in several parts of Britain and South Africa. In five different British bores studied by Benfield the heat outflow ranged from 0.68 to $1.53 : 10^{-6}$ cal./cm.^{2} sec, but there is reason to suppose that these are affected to some extent by residual cooling left from the last glacial period. Benfield, correcting for this, gets a range from 1.16 to 1.85×10^{-6} cal./cm.^{2} sec, mean $1.42 \pm 0.09 \times 10^{-6}$. The principle is presumably valid, but it is rather unsatisfactory that so large a correction should be necessary. For six bores in South Africa, Bullard gets values from 0.95 to 1.52×10^{-6}, with a mean of $1.16 \times 10^{-6} \pm 0.09$. The difference from the British determinations is quite small; the temperature gradients differ by a factor of 3 or more, but this is counter-balanced by the differences of conductivity, the British rocks being largely sandstones and shales and the South African ones quartzites. In fact, the low gradients long known to exist in South Africa, which make mining at great depths possible, are due to a normal heat outflow combined with so high a conductivity that the heat can be carried up at a comparatively low gradient. Similar work in other regions is needed, but the immediate suggestion is that the heat outflow is much steadier from place to place than either of its factors separately.

Apart from further corrections, chiefly for original heat,

[5] Proc. Roy. Soc. A. 173: 474-502, 1939.

[6] Proc. Roy. Soc. A. 173: 428-450, 1939.

[7] Proc. Roy. Soc. A. 173: 450-474, 1939.

[8] Proc. Roy. Soc. Edin. 60: 192-209, 1940.

the average heat outflow in Great Britain would be supplied by 28 ± 2 km. of the average granite of the region, and in South Africa by 15.7 ± 2 km. The uncertainties should perhaps be somewhat increased, for Britain, on account of the correction for the after-effect of the glacial period, and for South Africa because the thorium contents of the granites have been taken from analogy and not directly measured. Assuming that the rate of generation of heat has the form

$$P = A\, e^{-x/a}$$

where x is the depth and A and a are constants, A being the above mean for European and North American granites and using eq. (21) on p. 150 of "The Earth," with the same numerical values for other quantities, gives $a = 21$ km. The South African value would be much less; Bullard gives a maximum of 12 km. on a rather different basis. The ultimate steady temperature at great depths (that is, when all original heat has been lost, but not allowing for the decay of the radioactive elements) would be about 450° as against 490° as found in "The Earth," so that this quantity is little changed. The corresponding South African value would be much less.

The seismic determinations of the thicknesses are based on the propagation of transverse surface waves across Eurasia and the times of the near reflexions from deep-focus earthquakes near Japan. The former datum, which chiefly involves the upper layer, is representative of average conditions over Eurasia, but the latter is rather local and has been used by itself only because comparison of results about the thickness of the intermediate layer obtained by other methods has shown a number of unexplained discrepancies. As the results stand, however, and having regard to the uncertainties, the thermal result is fairly consistent with the seismic ones of 15 ± 3 km. and 18 ± 4 km. for the thicknesses of the upper and intermediate layers. The chief difficulty of interpretation is that we cannot take it for granted that the structure below Great Britain is the same as below Japan; we can only adopt this as a working hypothesis and hope that further work in both regions may provide a check on it. Thermal work in North America on the lines adopted in Great Britain and South Africa should also provide valuable information about the generality of the British result.

Nothing in the new results encourages the hope that fusion temperatures are a normal feature at any depth within the crust. The approximate reproduction of the ultimate steady temperature given in "The Earth" shows that the vertical distribution of temperatures shown on p. 154 needs little change as far as the present data are concerned. This is really not a defect. For though volcanoes exist, a generally fused layer would lead to absence of ocean tides and to a perfection of isostasy that certainly contradict the facts. It still appears that volcanoes are a special phenomenon and their explanation cannot be found in the average temperature distribution with depth.

The question of the thermal contraction available for mountain formation remains very much as it was. It has already been seen that most of the contraction probably comes from cooling below the oceans, not below the continents; and we have little direct evidence about the vertical distribution of radioactivity below the oceans. The chief recent result is the remarkable resemblance both in mean radioactivity and in scatter between the oceanic and plateau basalts. The coefficient of thermal expansion at high pressures is also not directly known. Thus we are still in the situation of knowing that thermal contraction should give a crustal shortening of the right order of magnitude to account for known mountains, but the theory still yields only an order of magnitude. One point that may be important is that the low heat flow in South Africa implies a lower ultimate steady temperature and therefore more cooling at great depths, so that deep rocks there should be stronger than in Europe and Asia and perhaps as strong as below the oceans. This may be connected with the scarcity of great folded mountain systems in Africa.

It is now doubtful whether the cooling represented by the error-function term in the temperature gives the most important part of the contraction available for mountain formation. The initial temperature would contain a term representing the variation of melting point with pressure. In most treatments of the problem the curvature of the earth is neglected, and this term remains constant for all time. But when the finite size of the earth is taken into account, it is found that this term must lead to cooling since solidification by about the same amount at all depths, and the resulting contraction, is of the same order of magnitude as is given by the term usually considered.[9] Further, it gives contraction at a uniform rate, which agrees better with the distribution of mountain formation with time. Previous solutions gave a decreasing rate, which, even with allowance for our more detailed knowledge of later disturbances, was hard to accept. But it is still harder to evaluate this term accurately because it requires knowledge of the coefficient of thermal expansion at still greater depths.

This note is intended to call attention to some of the problems needing solution, and will probably suggest others to other workers. The results obtained in "The Earth" need, apparently, less change than might have been expected from recent revisions of some of the data, the various changes largely cancelling in the quantities with most geological application. But even this solution is highly provisional and separate studies of other regions with regard to the heat outflow and the radioactivities of the rocks are necessary to test the generality of the inferences.

[9] M. N. R. A. S. Geophys. Suppl. 3: 6-9, 1932.

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