had any reason to suppose such distributions might have occurred in the case in dispute. This he has failed to do—he has evaded the point. (3) Prof. Pearson descends to vague generalities except in regard to Dr. Oliver Lodge, who may be left to defend himself.

With the last paragraph of the letter, however, I heartily concur. There is nothing the S.P.R. would welcome more than intelligent and independent criticism. Only the critic would have to study the evidence first, and the Professor apparently has the "scientific amenity" to see that by doing so he would cut his own throat; for he would, ipso facto, become a psychical researcher!

Edward T. Dixon.

Cambridge, December 29, 1894.

ON THE AGE OF THE EARTH.

It has been thought advisable to publish the following documents. On October 12 I put my views before Prof. Fitzgerald and Dr. Larmor. The first paper is a copy of my letter to Dr. Larmor. It has now been edited a little, as originally it was rather hurriedly written. Some long mathematical notes, added on November 1, to prove the legitimacy of my approximate method of calculation, are now omitted, as Mr. Heaviside has given exact solutions, and has found that there is practically no difference between mine and the exact numerical answers. That Mr. Heaviside should have been able, in his letters to me during eleven days, to work out so many problems, all seemingly beyond the highest mathematical analysis, is surely a triumph for his new methods of working. Only for Prof. Fitzgerald’s encouragement and sympathy is it very probable that this document would never have been published.

I have sometimes been asked by friends interested in geology to criticize Lord Kelvin’s calculations of the probable age of the earth. I have usually said that it is hopeless to expect that Lord Kelvin should have made an error in calculation. Besides, in every class in mathematical physics in the whole world since 1862 the problem has been put before students, and as the subject is of enormous interest, if there had been any error it certainly would have been discovered before now.

I dislike very much to consider any quantitative problem set by a geologist. In nearly every case the given conditions are much too vague for the matter to be in any sense satisfactory, and a geologist does not seem to mind a few millions of years in matters relating to theory. I never till about three weeks ago seriously considered the problem of the cooling of the earth except as a mere mathematical problem, as to which definite conditions were given. But the best authorities in geology and palentology are satisfied with evidences in their sciences of a much greater age than the one hundred million years stated by Lord Kelvin; and if they are right, there must be something wrong in Lord Kelvin’s conditions. On the other hand, his calculation is just now being used to discredit the direct evidence of geologists and biologists, and it is on this account that I have considered it my duty to question Lord Kelvin’s conditions.

The original object of Lord Kelvin’s investigation is usually forgotten. He sought to prove, and proved, that the earth is losing energy at a calculable rate. He said that the loss might be the loss of potential or chemical energy instead of sensible heat, or as well as heat, although he thought that a large proportion of potential or chemical energy was improbable; and it is only on the assumption that the earth is a cooling body losing energy originally only of the sensible-heat form, that his calculation of the age of the earth is based. Not only so, but also his earth is a homogeneous mass of rock such as we have on the surface, with the same conductivity and other heat properties. He starts with the knowledge that there is an average increase of temperature downwards in the earth of one Fahrenheit degree for every 50 feet. Assuming that the earth, a solid, was once at the uniform temperature of 70000° F., that its surface was suddenly brought to and kept at the temperature 0°, and taking k/c (k being conductivity and c capacity for heat of unit volume in 1000 foot units) as 400, he finds that 10° years have sufficient to cause the temperature-gradient at the surface to be what it is now. He stated that the conditions were sufficiently represented by an infinite uniform mass of matter at 70000° F. with an infinite plane face kept at 0°.

At first I preferred to consider the globe of 4000 miles radius of constant surface-emissivity to be cooling as if in an enclosure, kept at constant temperature. I made the emissivity infinite, and obtained Lord Kelvin’s answer for temperature-gradient near the surface. When the emissivity is taken of a finite value, the time taken to produce the present temperature-gradient is less than Lord Kelvin’s answer.

It is interesting to notice that if we take our enclosure to be at a zero of temperature which we can choose as we please, we have a method of using Fourier’s expression in certain cases in which the emissivity is not constant. By no method of working does it seem probable that we shall greatly alter Lord Kelvin’s answer.

Modification of Lord Kelvin’s Conditions.

But, when we depart from homogeneity, when we assume that the interior of the earth may be of better conducting material than the surface rock in which the temperature-gradient is alone measured, we find a very different state of things from that considered by Lord Kelvin. The cooling from a constant temperature of an infinite mass bounded by a cold plane face, a slice of which near the surface is of material different from the rest of the infinite block, is a problem difficult to attack mathematically. But if the slice is thin, or if much time has elapsed, the following artifice leads to a solution.

Imagine an infinite homogeneous block, originally at temperature $V_0$, whose surface is kept at 0. If $x_1$ is sufficiently small and $t$ great, we may neglect the exponential term, and (V being temperature and $t$ time, and $x$ the distance from the cold face)

$$\frac{dv}{dx} at x_1 = V_1 + \sqrt{\frac{k}{\pi c_1}}; \int_{x_1} at x_2 = V_2 + \sqrt{\frac{k}{\pi c_2}}.$$

The rate of flow of heat across unit area at $x_1 = k(V_1 - V_2) + \sqrt{\frac{k}{\pi c_2}}$. I take $k$ as conductivity, and $c$ as conductivity divided by capacity for heat of unit volume.

Now take another such homogeneous infinite block of different material, and use the letters with $\pi$ instead of 1. Let the time be the same in both. Let the surface slice from $x_1$ to 0 in the first, and from $x_2$ to 0 in the second be considered. We can, by taking proper values of $V_1$ and $V_2$ and $x_1$ and $x_2$, make the rates of flow of heat equal and the temperatures equal at $x_1$ and $x_2$:

$$k_1(v_1 - v_2) = k_2(v_1 - v_2) + \sqrt{\frac{k_2}{\pi c_2}}.$$

Hence $V_2 = V_1 + \sqrt{\frac{k_1}{\pi c_1}}$. Thus if $n k_2 = k_1$, we take $n x_2 = x_1$.

Now we can take the slice $x_2$ to 0 from the second block and let it take the place of the slice $x_1$ to 0 on the first block. The artificial block so produced will go on cooling, its outer face being kept at 0. But we shall have at the point of junction a sudden multiplication of $dv/dx$. In fact, $dv/dx$ will be what it used to be towards the interior, but will be $n$ times as great towards the surface. It is of no consequence what the value of $x_2$ is, if times are great and slices thin, the only important thing is that $x_2$ shall be $n$ times $x_1$. The application of the result is obvious.
Let the interior of the earth be a uniform sphere, uniformly heated to 7000° F. Take its r as m times what Lord Kelvin took it, then an increase of temperature downwards from the surface of 1 F. degree for every 50 m would be produced in 1 foot. Take k as n times what Lord Kelvin takes it. Now if we imagine a skin removed and replaced by one of 1/nth of the thickness and 1/mth of the conductivity, that is, take it of Lord Kelvin's conductivity of rock, the surface slope will be 1 in 50, what it is now, and Lord Kelvin's time will be multiplied by 2 and the proportion of the surface temperature.

Considering the great differences in conductivity of such bodies as we know, it is quite conceivable in our knowledge and ignorance of the interior of the earth that n²/m may be considerable even now, and probably was very considerable in past times. Roughly we may say that Lord Kelvin's age of the earth, 10⁸ years, ought to be multiplied by two or six times the ratio of the internal conductivity to the conductivity of the skin.

I am not in a position to criticise the arguments from tide phenomena which Lord Kelvin or Mr. Darwin would now put forward on the subject of much internal fluidity of the earth. The argument from precession has been given, and of course much internal fluidity would practically mean infinite conductivity for our purpose. But there is no doubt of a certain amount of fluidity inside even now, and taking it that the inside of the earth is a honeycomb mass of great rigidity, partly solid and partly fluid, we have reason to believe in very much greater quasi-conductivity inside than of true conductivity in the surface rocks, and if there is even only ten times the conductivity inside, it would practically mean that Lord Kelvin's age of the earth must be multiplied by 50.

If we imagine the earth perfectly conducting inside with a thin covering, say 60 miles thick, of rock, such as we know it on the surface, we must leave Lord Kelvin's infinite mass and study the sphere. Indeed, if we take it that we have now an infinite mass at 7000° F. of infinite conductivity, cooling through rock of from 60 to 70 miles thick with a constant gradient of 1 for every 50 feet, we can imagine that this state of things has existed for an infinite time, and any original distribution of temperature in the rock would settle down to such a state.

Taking, then, an internal sphere of infinite conductivity (and working in C.G.S. Centigrade units), its specific heat 0.2, and the conductivity of the rock 0.002, I find that if at the beginning of time there was an increase of temperature in 45 feet, and now there is an increase of 1 Centigrade degree in 90 feet, the lapse of time is 28,930 million years, or 290 times Lord Kelvin's age, and the core has cooled from 8000 to 4000 degrees. Of, again, in the last 10⁷ years the gradient has only diminished by 1/400th of its present value, and the core has only changed from 4010 to 4000 degrees.

I do not know that this speculation is worth much, except to illustrate in another way the augmented answer when we have higher conductivity inside. It would evidently lengthen the time if I assumed that the temperature-gradient was not uniform in the shell, but the exact mathematical calculation is so troublesome that I have not attempted it.


JOHN PERRY.

1 Observe that, even if we assume that there is the same conductivity inside and outside, inasmuch as the density is greater, r is greater, say 3 times as great, and even without the assistance of increased conductivity inside, we should find Lord Kelvin's age. I admit that all such calculations as this take its a to the value of c is too vague to be of much importance.

2 From the numerical temperature increases 0 and k, and if k is the thickness of the crust and R the radius of the internal sphere, if s is its specific heat and t its density and k its conductivity, we have:

\[ t = t_0 - \frac{k \cdot s}{3} \log \left( 1 + \frac{k}{s} \right) \]

3 If 'of be taken as the conductivity of rock, the times are only a third of what I have given.

In connection with this matter I notice that in Lord Kelvin's very short paper, entitled "The Doctrine of Uniformity" in Geology briefly

October 22, 1894.

The reasoning in my paper was applied either to infinite blocks of cooling material or to a sphere with an internal core which has infinite conductivity. At the time of writing I did not see my way to the consideration of the whole sphere with a core of finite conductivity and a shell of rock as a covering, but the case is easily to work when the shell is only a few miles in thickness, as will be seen below.

**Problem.**—A sphere of radius R = 6.38 x 10⁶ centim. of conductivity k = 0.47 (or 79 times that of surface rock) and initial temperature T₀ = 0.1645 (or 40 times the surface temperature) put upon it a shell of rock of thickness 4 x 10⁶ centim. (about 25 miles). The whole mass was once at a temperature \( V = 4000\) C., and suddenly the outside of the shell was put to 0° C. and kept at that. Find the time of cooling until the temperature-gradient in the shell has become 1 Centigrade degree in 2743 centim. (or 1° F. in 50 feet).

Now, if we are allowed to assume that the shell very rapidly acquired and retained a uniform temperature-gradient throughout its thickness, and it is easy to show that this assumption is allowable (or if not, then the discrepancy is in favor of a greater age) of (1) the problem is exactly the same as this:—The above-mentioned sphere has no shell of rock round it, but emits heat to an enclosure of 0° C., the constant emissivity of its surface being \( E = 1.475 \times 10^{-4}\); find the time in which the surface-temperature \( v\) becomes 146° C.

This problem is solved by Fourier, who gives for the temperature at the distance \( x\) from the centre

\[ v = 2\pi E x^2\ e^{-k - x^2/4}\]

where in the successive terms the values of \( e\) are taken to be the successive roots of the equation

\[ \tan e = 1 - E / \pi^2 \]

In the present case \( E / \pi^2 = 20\), and \( e₁, e₂, e₃, &c.\) are nearly \( 2, 3, 3\), &c. I have, however, taken the actual values of \( e₁, e₂, &c.\) as two exponential terms, only, being of importance, and I find that, if \( t = 96 \times 10⁷\) years, \( e = 142.7 + 576\ = 148.4\), and the last term

so that the age of cooling to the present temperature-gradient is more than 96 x 10⁷ years.

Refined, read before the Royal Society of Edinburgh in 1895, he finds:—"But the heat which we know, by observation, to be now conducted out of the Earth yearly is so great that if this action had been going on constantly ever since the formation of the earth this heat lost by the Earth would have been about as much as would be heat, by 100° Cent., by a cubic hectar of the ordinary surface rocks, or 100 times the heat lost by the Earth. (The numbers are mine.) In his address on "Geological Dynamics," Part II., published in 1895 (p. 106, vol. ii., "Popular Lectures and Addresses"). He calculates the total amount of energy which may "have been possessed by the Earth mass, partly gravitational and partly chemical, as 'being about 600 times as much heat as would raise the temperature of an equal area of surface-rock from 40° to 70° Cent." (The althecra are mine.) I do not think that these two statements have ever been before put in juxtaposition. Comparing them, we may say that, according to Lord Kelvin's own figures, if the present action had been going on with any approach to uniformity for 10⁷ years the total amount of heat lost by the Earth would have been the zephyr part of what Lord Kelvin gives as an estimate, an overestimate he calls it (but he says 'trat it is not possible to make one much less vague), of the whole amount of heat at present in the Earth. I mention this because some mathematical physicists believe that Lord Kelvin based his age of the Earth upon a calculation of this total 1-3. He only used it in opposition to the extreme doctrine of uniformity for the past 2000 million years (a doctrine which is not now believed in by any geologist), but it lends no support to his calculated age of the Earth.

All through this paper I give 10⁷ years as Lord Kelvin's age of the Earth. His own words (Trans. R. S. Edin., 1865 (2)) are:—"We must, therefore, allow very wide limits to such an estimate as I have attempted to make; we may with much probability say that one conclusion cannot have taken place less than 400000000 years ago, or we should have more uniform heat than we actually have (he means a more rapid increase of temperature downwards), or more than 400000000 years ago, or we should have no uniformity of heat at the Earth's surface. As to the various experiments he finds (v) that the present temperature-gradient of 1 Fahr. degree for 50 feet gives a life of 96 x 10⁷ years.

Because if \( s\) is the surface-temperature of the sphere and \( \theta\) the thickness of the shell of rock, \( s^2\) was the surface-temperature in the shell and \( s^2\) multiplied by conductivity of rock is equal to \( E\).
If we take $k$ as 195 times that of the surface-rock, and $k/c$ as 35 times that of the surface-rock, and if the shell has a depth of 3272 $\times 10^6$ centimetres (about 20 miles), the time of cooling until the temperature-gradient is 1 Cent. degree in 2743 centim. is more than 127 $\times 10^6$ years.

I kept no copy of the letter which I sent to Prof. Tait with the foregoing document. In it I explained my difficulty in getting Lord Kelvin to reconsider the internal heat question, and I asked for his advice.

Extract from Letter of Prof. Tait, November 22, 1894.

... my entire failure to catch the object of your paper. For I seem to gather that you don't object to Lord Kelvin's mathematics. Why, then, drag in mathematics at all, since it is absolutely obvious that the better conductor the interior in comparison with the skin, the longer ago must it have been when the whole was at 7000 F. : the state of the skin being as at present? I don't suppose Lord Kelvin would care to be troubled with a demonstration of that. As to the validity, or more properly the plausibility of his or your assumptions, I don't suppose anyone will ever be in a position to judge. He took the simple and apparently possible case of a uniform conductivity all through—having no doubt possible what. What if he had assumed, as he was quite entitled to do, that the conductivity diminishes very fast with rise of temperature? But I need not say any more, as I seem to have entirely missed your point.

Letter to Prof. Tait, November 26, 1894.

DEAR PROF. TAIT,—I should have been on the whole better satisfied if you had opposed my conclusions. You say I am right, and you ask my object. Surely Lord Kelvin's case is lost, as soon as one shows that there are possible conditions as to the interior rate that at present, much of its mass being kept cold and in the meteor form, and the rate may have greatly varied from time to time. This is not only possible but probable, and it is for you and Lord Kelvin to prove a negative, not the Tidal Retardation argument! Even if your rate of retardation is correct, the real basis of your calculation is your assumption that a solid earth cannot alter its shape (diminishing its equatorial radius by a few miles) even in 1000 million years, under the forces constantly tending to alter its shape, and yet we see the gradual closing up of passages in a mine, and

Then

\[ e = 1 = \frac{R}{hb}. \]

\[ \tan e = \frac{b}{A} \sin e. \]

\[ 2743 \times \sec e. \]

\[ 2743 \times \sin e. \]

\[ -k e^2 \sec e. \]

It enables to be calculated. It would doubt no possible, but it hardly be worth while, to find the values of $e$ and $A$ which would give a maximum value for $t$. In one of the above cases I took $e$ nearly $\pi$, and in the other $\pi$.

I am quite unable to attack the problem of the cooling of a sphere from an arbitrary initial condition, in which the diffusivity for heat is an arbitrary function of $r$.

There is some difficulty in $k/c$ which would give a greater age to the Earth than any other, but, again, it would hardly be worth while to spend much time on the problem. My purpose has not been to fix a higher limit to the age of the Earth, but to show that such a higher limit must be greater than some hundred of times one hundred million years.

Some of my friends have blamed me severely for not publishing the above document sooner. I was Lord Kelvin's pupil, and am still his affectionate pupil. My B.A. Lecture on Spinning Tops was stolen from him, as I duly acknowledged when it was published. I have been unkind to me, and there have been times when he must have found this difficult. One thing has not yet happened: I have not yet received the thirty pieces of silver.

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we know that wrinkling and faults and other changes of shape are always going on in the solid earth under the action of long-continued forces. I know that solid rock is not like cobbler's wax, but 100 years is a very long time, and the forces are great! I had thought these two arguments to be mere suppositions of the internal heat one which I took to be the only important one, like a diamond whose pure sparkle was brought into relief by two rubies. If I were alone in my opinion, I should still have the courage, I think, to write as I do; but as I have already told you, I did not venture to write and speak to Lord Kelvin, or write to you until I found that so many of my friends agreed with me—Fitzgerald, O. Reynolds, Larmor, Henrici, Lodge, Heaviside, and many others. Fitzgerald is the only man to mention my notion about the sun's heat, but he quite agrees with me. I have not put before him my notion the Tidal Retardation argument.

DEAR PROF. PERRY.—I should like to have your answers to two questions:—

(1) What grounds have you for supposing the inner materials of the earth to be better conductors than the skin?

(2) Do you fancy that any of the advanced geologists would thank you for 100 years instead of 10? Their least demand is for 1000 years—part of the secondary period!

Yours truly,

P. G. TAIT.

November 29, 1894.

DEAR PROF. TAIT,—It is for Lord Kelvin to prove that there is not greater conductivity inside. Nevertheless I will state my grounds:—

(a). In page 6 of the paper sent you I say "I am not in a position to criticise the argument from tide phenomena which Lord Kelvin or Mr. Darwin would now put forward on the subject of much internal fluidity of the earth. The argument from precession has been given up. Of course, much internal fluidity would practically mean infinite conductivity for the purpose. But there is no doubt of a certain amount of fluidity inside, even now, and taking it that the inside of the earth is a honeycomb mass of great rigidity, partly solid and partly fluid, we have reason to believe in very much greater quasi-conductivity inside than of true conductivity in the surface rocks." Even if we assume perfect solidity, and even neglecting our knowledge of much iron—surely there can be no doubt of the conductivity of rock increasing with the temperature from the analogies with electric conduction, one would say, without any experimenting, that as a metal diminishes in conductivity with increase of temperature, so a salt, a mixture of salts, a rock, may be expected to increase in conductivity with increase of temperature. I presume that Everett's book is recognised as giving the most exact information on these subjects. He nowhere suggests that rock diminishes in conductivity with temperature. Every case he gives shows an increase. I have made out the following table from the only quotations which Everett gives from Dr. Robert Weber: only five cases, but probably representative.

<table>
<thead>
<tr>
<th>Material</th>
<th>In conductivity</th>
<th>In specific heat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Micaeous gneiss</td>
<td>48.2</td>
<td>23.6</td>
</tr>
<tr>
<td>Mica schist</td>
<td>13.04</td>
<td>24.4</td>
</tr>
<tr>
<td>Emrie</td>
<td>18.56</td>
<td>35.7</td>
</tr>
<tr>
<td>Gneiss</td>
<td>21.4</td>
<td>61.5</td>
</tr>
<tr>
<td>Micaeous schist</td>
<td>94.5</td>
<td>35.4</td>
</tr>
</tbody>
</table>

Average... 43.1  36.1

Average, leaving out Emrie... 75  36

Even if the conductivity and specific heat did not alter, inasmuch as the internal density is greater, the volumetric capacity is greater; and if it is at three times as great, we have three times Lord Kelvin's age. In fact, the rule given at page 4 of my paper is the same as this:—If the conductivity inside is
body would be of the same opinion—in spite of what you say in your first letter. I know that Lord Kelvin himself did not seem to think me right when—after I had sent him the documents—I talked to him at Cambridge.

I remain, yours truly,

JOHN PERRY.

Copy of a Letter from Lord Kelvin.

The University, Glasgow, December 13, 1894.

DEAR PERRY,—Many thanks for sending me the printed copy of your letter to Larmor and the other papers, which I found waiting my arrival here on Saturday evening. I have been much interested in them and in the whole question that you raise, as to the effect of greater conductivity and greater thermal capacity in the interior. Your \( n^2 \sim t \) theorem is clearly right, and not limited to the case of the upper stratum being infinitely thin. Twenty or thirty kilometres may be as good as infinitely thin for our purposes. But your solution on the supposition of an upper stratum of constant thickness, having smaller conductivity and smaller thermal capacity than the strata below it, is very far from being applicable to the true case in which the qualities depend on the temperature. This is a subject for mathematical investigation which is exceedingly interesting in itself, quite irrespective of its application to the natural problem of underground heat.

For the natural problem, we must try and find how Robert Young's results can be adapted as trustworthy, and I have written to Everett to ask him if he can send me the separate copy of Weber's paper, which it seems was sent to him some time before 1885; but in any case it will be worth while to make further experiments on the subject, and I see quite a simple way, which I think I must try, to find what deviation from uniformity of conductivity there is in slate, or granite, or marble between ordinary temperatures and a red heat.

For all we know at present, however, I feel that we cannot assume as in any way probable the enormous differences of conductivity and thermal capacity at different depths which you take for your calculations. If you look at Section 11 of "Secular Cooling" ("Math. and Phys. Papers," vol. iii. p. 390), you will see that I refer to the question of thermal conductivities and specific heats at high temperatures. I thought my range from 200 millions to 400 millions was probably wide enough, but it is quite probable that I should have put the superior limit a good deal higher, perhaps 4000 instead of 400. The subject is extremely interesting; in fact, I would rather know the date of the Consistentorii Status than of the Norman Conquest; but it can bring no comfort in respect to demand for more work in Palaeontological Geology. Helmholts, Newcomb, and others are inexcusable in refusing sunlight for more than a score or a very few scores of million years of past time (see "Popular Lectures and Addresses," vol. i. p. 397).

So far as underground heat alone is concerned you are quite right that my estimate was too high, and that of Cottrell ("P. L. and A," vol. ii. p. 87) that that is all Geikie wants; but I should be exceedingly frightened to meet him now with only 20 million in my mouth.

And, lastly, don't despise secular diminution of the earth's moment of momentum. The thing is too obvious to every one who understands dynamics.

Yours always truly,

KELVIN.