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 acumen" 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, it is very probable that this document would never have been published.

I have sometimes been asked by friends interested in geology to criticise Lord Kelvin's calculation 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 conditions given 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 time. Therefore 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 palæontology 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 7000° 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 year foot units) as 400, he finds that 10^8 years have sufficed 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 7000° F. with an infinite plane face kept at 0.

At first I preferred to consider a *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_1, 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)

$$\frac{dv}{dx} \text{ at } x_1 = V_1 \div \sqrt{\pi\kappa_1 t}; \quad v_1 \text{ at } x_1 = V_1 x_1 \div \sqrt{\pi\kappa_1 t}.$$

Rate of flow of heat across unit area at $x_1 = k_1 V_1 \div \sqrt{\pi\kappa_1 t}$. I take k as conductivity, and κ 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 affix 2 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:

$$k_1 V_1/\sqrt{\kappa_1} = k_2 V_2/\sqrt{\kappa_2} \text{ and } V_1 x_1/\sqrt{\kappa_1} = V_2 x_2/\sqrt{\kappa_2}$$

Hence $k_1 \div x_1 = k_2 \div x_2$. Thus if $nk_2 = k_1$, we take $nx_2 = x_1$.

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 outside face being kept at 0_1. 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 κ_2 is, if times are great and slices thin, the only important thing is that k_1 shall be n times k_2. The application of the result is obvious:—

Let the interior of the earth be a uniform sphere, uniformly heated to 7000° F. Take its κ as m times what Lord Kelvin took it, then an increase of temperature downwards from the surface of 1 F. degree for every 50 n feet would be produced in $10^8 n^2/m$ years. Take its k as n times what Lord Kelvin takes. Now if we imagine a skin removed and replaced by one of $1/n$th of the thickness and $1/n$th 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 increased in the proportion n^2/m.

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 n^2/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, 10^8 years, ought to be multiplied by two to 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 up. 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 56.[1]

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[2] (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 1° Centigrade 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. Or, again, in the last 10^8 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.[3]

JOHN PERRY.

31 Brunswick Square, London, W.C., October 14.

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 a sphere with a core of finite conductivity and a shell of rock as a covering, but the case is really easy to work when the shell is only a few miles in thickness, as will be seen below.

PROBLEM.—A sphere of radius R = $6·38 \times 10^8$ centim. of conductivity $k = 0·47$ (or 79 times that of surface-rock) and $k/c = 0·16464$ (or 14 times that of surface-rock), has upon it a shell of rock of thickness 4×10^5 centim. (about $2\frac{1}{2}$ 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 discrepance is in favour of a greater age for the earth), 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[4] being $E = 1·475 \times 10^{-8}$; 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 r from the centre

$$v = \frac{2\text{VER}}{k} \Sigma \frac{\sin er/\text{R}}{er/\text{R}} \frac{\epsilon^{-ke^2t/c\text{R}^2}}{e \operatorname{cosec} e - \cos e},$$

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

$$e/\tan e = 1 - \text{ER}/k.$$

In the present case $\text{ER}/k = 20$, and e_1, e_2, e_3, &c., are nearly π, 2π, 3π, &c. I have, however, taken the actual values of e_1 and e_2—two exponential terms, only, being of importance, and I find that, if $t = 96 \times 10^8$ years,

$$v' = \underset{\text{1st term}}{142·7} + \underset{\text{2nd term}}{5·65} = 148·4;$$

so that the age of cooling to the present temperature-gradient is more than 96×10^8 years.

[1] Observe that, even if we assume that there is the same conductivity inside and outside, inasmuch as the density is greater, c is greater, say 3 times as great, and even without the assistance of increased conductivity inside, we have 3 times Lord Kelvin's age. I admit that all such speculation as to the value of c is too vague to be of much importance.

[2] If θ_0 and θ were the internal temperatures at the times t_0 and t, if b is the thickness of the crust and R the radius of the internal sphere, if s is its specific heat and ρ its density and k its conductivity,

$$t - t_0 = \frac{\text{R}bs\rho}{3k} \log \frac{\theta_0}{\theta}.$$

[3] If ·006 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 Refuted," read before the Royal Society of Edinburgh in 1865, 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 with any approach to uniformity for 20,000 million years, the amount of heat lost out of the Earth would have been about as much as would heat, by 100° Cent., a quantity of ordinary surface-rock 100 times the Earth's *bulk*." (The italics are mine.) In his address on "Geological Dynamics," Part II., published in 1869 (p. 126, vol. ii. "Popular Lectures and Addresses"), he calculates the total amount of energy which *may* once have been possessed by the Earth mass, partly gravitational and partly chemical, as 'being about 700 times as much heat as would raise the temperature of an equal *mass* of surface-rock from 0° to 100° Cent.' (The italics are mine.) I do not think that these two statements have ever before been 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 10^8 years the amount of heat lost by the Earth would have been the 1/7800th part of the whole energy which the whole Earth may once have possessed, or 1/2230th part of what Lord Kelvin gives as an estimate, an over-estimate he calls it (but he says that 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 loss. He only used it in opposition to the extreme doctrine of uniformity for the past 20,000 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 10^8 years as Lord Kelvin's age of the Earth. His own words (*Trans. R.S. Edin.*, 1862 (j) are:—"We must, therefore, allow very wide limits in such an estimate as I have attempted to make; but I think we may with much probability say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have [he means a more rapid increase of temperature downwards], nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature." Taking the average diffusivity for heat of the Edinburgh experiments, he finds (r) that the present temperature-gradient of 1 Fahr. degree for every 50 feet gives a life of 10^8 years.

[4] Because if v' is the surface-temperature of the sphere and b the thickness of the shell of rock, v'/b was the surface-gradient in the shell and v'/b multiplied by conductivity of rock is equal to Ev'.

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 $3 \cdot 272 \times 10^6$ centimetres (about 20 miles), the time of cooling until the temperature-gradient is 1 Cent. degree in 2743 centim. is *more* than 127×10^8 years."[1]

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 re-consider 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 uniform conductivity all through—having no data whatever. 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 internal state of the earth which will give many times the age which is your and his limit. . . . What troubles me is that I cannot see one bit that you have reason on your side, and yet I have been so accustomed to look up to you and Lord Kelvin, that I think I must be more or less of an idiot to doubt when you and he were so "cocksure." The argument from the sun's heat seems to me quite weak. Even a geologist without mathematics can see that the time given by Lord Kelvin will be increased if we assume that in past times the sun radiated energy at a smaller rate than at present, much of its mass being possibly 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.

Then 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 action of forces constantly tending to alter its shape, and yet we see the gradual closing up of passages in a mine, and 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 10^9 years is a very long time, and the forces are great!

I had thought these two arguments to be mere supporters 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 whom I have mentioned my notion about the sun's heat, but he quite agrees with me. I have not put before him my notion about the Tidal Retardation argument. . . .

November 27.

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 10^{10} years instead of 10^8? Their least demand is for 10^{12}:—for *part* of the mere 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:—

1 (α). 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 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."

1 (β). 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 now 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.

Percentage increase for a rise of 100° *Centigrade.*

	In conductivity.	In specific heat.
Micaceous gneiss ...	48·0	23·6
Mica schist	136·4	24·4
Eurite	1856	35·7
Gneiss	21·4	61·5
Micaceous schist ...	94·5	35·4
Average ...	431	36·1
Average, leaving out Eurite	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 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

[1] The general expression for any case is this:—A sphere of radius R of conductivity nk and capacity per unit volume mc/n surrounded by a shell of thickness b, conductivity k, and capacity for heat per unit volume c; take $E = k/b$: when is v', the temperature at the surface, equal to $b/2743$?

$$\frac{e}{\tan e} = 1 - R/bn.$$

Then

$$\frac{b}{2743} = \frac{2VR}{bn} \Sigma \frac{\sin e}{e} \frac{\sin e}{e - \frac{1}{2}\sin 2e} \epsilon^{-ke^2 t/cR^2}$$

enables t to be calculated. It would no doubt be possible, but it would hardly be worth while, to find the values of n and b which would give a maximum value for t. In one of the above cases I took e nearly π, and in the other $\pi/2$.

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 distribution of k/c which would give a greater age to the Earth than any other, but, again, it would hardly be worth whilo to spend much time on the problem. My purpose has not been to fix a higher limit to the age of the Earth; it has only been 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. He has been uniformly kind 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.

n times the conductivity outside; if the specific heat inside is s times the specific heat outside; if the density inside is d times the density outside; then Kelvin's age of the earth is increased $n\,s\,d$ times. . . It is not likely that Dr. Weber's rate of increase would be constant to such a temperature as 4000° C.; but the electric analogue allows us to imagine a greater and greater rate of increase at higher temperatures; therefore it is in Lord Kelvin's interest to take Weber's rate. Now at 4000° C. the conductivity would be [leaving out eurite, which seems abnormal and too much in my favour], thirty times as great as it is at the surface; the specific heat would be $14\frac{1}{2}$ times as great, and taking the density as three times, we have, even for a *perfectly solid* earth an age 1300 times the age given by Lord Kelvin.

2. In answer to your second question, Lord Kelvin completely destroyed the uniformitarian geologists, and not one now exists. It was an excellent thing to do. They are as extinct as the dodo or the great auk.

I have met many advanced geologists, and not one of them demands more than 1,000,000,000 years. Probably Sir Archibald Geikie is the most representative of the geologists who have studied this question, and he never (in recent years) seems to have desired even as much as 1,000,000,000 years. (See his address as President of the British Association.) The biologists have no independent scale of time; they go by geological time. According to Huxley, less than 1,000,000,000 years is enough as the age of life on the earth.

But surely the real question now is not so much what the geologists care about, as—Had Lord Kelvin a right to fix 10^8 years, or even 4×10^8 years, as the greatest possible age of the earth?

Yours truly,

JOHN PERRY.

December 6, 1894.

DEAR PROF. TAIT,—Prof. Fitzgerald has pointed out to me that the five rocks given by Everett are not to be found in his 1891 edition. I quoted from his 1886 edition. I therefore wrote to Everett, asking why he had left them out—was there a mistake? He writes to say: "I copied Weber's data from a copy of his paper which was, and may be still, in my possession, having been sent me through the post, probably by the author, or possibly by Dr. Stapff, the geologist of the St. Gothard Tunnel, with whom I had much correspondence in underground temperature. You seem to assume, in writing to Tait, that I picked out samples of Weber's results; but my recollection is that I gave everything without reservation.

"I did not reproduce his results in the 1891 edition, and I cannot remember all my reasons for dropping them. On comparing them with other people's, which I give, they appear to be much too small. There is such a mass of conduction results in my book, that I was on the look-out for something that might be omitted.

"I have just referred to the foreign translations of my book. The German edition, published in 1888, gives only a page of conductivities of solids, and includes among them one of R. Weber's, namely Glimmerschiefer ·000733 + ·000010 t. The Russian edition, brought out by editors who took tremendous pains in verifying and correcting references, gives my list of Weber's results exactly as it stands in my book, the sign of the temperature coefficient being positive in every case. I do not know of any direct evidence as to the variation of rock conductivity with temperature except R. Weber's, but there is something approaching to direct evidence in the comparison of George Forbes' results with Herschel and Dunn's (see my 1891 edition, pp. 126, 129). Forbes found at − 10° C. the conductivity of white marble to be ·00115, black marble ·00177. Dunn and Herschel found at the temperature of hot water, marbles, &c., ·0047 to ·0056 (see Forbes' remark, quoted at p. 129).

"You have built a very lofty edifice on the basis of Weber's results, and extrapolation is proverbially a risky process, but I consider you have established a strong presumption in favour of the increase of rock conductivity with temperature."

I did not know, when writing to you on November 26, that the Rev. M. H. Close, M.A., had (R. Dublin Soc., Feb. 1878) put forward in great detail the reasons which I gave you shortly, against the tidal retardation argument. I thought they were my own. I notice that this gentleman assumes that increased conductivity inside would help Lord Kelvin, and indeed I cannot help thinking that, without mathematics, almost anybody 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 $n^2 \div m$ 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 irrespectively of its application to the natural problem of underground heat.

For the natural problem, we must try and find how far Robert Weber's results can be accepted 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 1886; but in any case it will be worth while to make farther 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. 300), you will see that I refer to the question of thermal conductivities and specific heats at high temperatures. I thought my range from 20 millions to 400 millions was probably wide enough, but it is quite possible that I should have put the superior limit a good deal higher, perhaps 4000 instead of 400.

The subject is intensely interesting; in fact, I would rather know the date of the *Consistentior Status* than of the Norman Conquest; but it can bring no comfort in respect to demand for time in Palæontological Geology. Helmholtz, Newcomb, and another, are inexorable 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 100 millions, and please remark ("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.

JUPITER.

JUPITER being now near opposition, and having an apparent diameter of 47″·7, is displayed as a very brilliant object in the heavens, and his northerly declination of 23 degrees enables him to remain above the horizon for a period of $16\frac{1}{2}$ hours.

During the few ensuing months, the observation of his belts and spots will enlist a large amount of attention, for there is probably no other planetary object which exhibits a more diversified and variable aspect. One feature of the present observations will be important as enabling comparisons to be made as to the rates of motion of the various white and dark spots in this and preceding oppositions. No doubt many of the surface