

ART. XCIV. ON THE SECULAR COOLING OF THE EARTH.

[*Transactions of the Royal Society of Edinburgh*, Vol. XXIII.
Read April 28, 1862.]

1. For eighteen years it has pressed on my mind, that essential principles of Thermo-dynamics have been overlooked by those geologists who uncompromisingly oppose all paroxysmal hypotheses, and maintain not only that we have examples now before us, on the earth, of all the different actions by which its crust has been modified in geological history, but that these actions have never, or have not on the whole, been more violent in past time than they are at present.

2. It is quite certain the solar system cannot have gone on even as at present, for a few hundred thousand or a few million years, without the irrevocable loss (by dissipation, not by *annihilation*) of a very considerable proportion of the entire energy initially in store for sun heat, and for Plutonic action. It is quite certain that the whole store of energy in the solar system has been greater in all past time, than at present; but it is conceivable that the rate at which it has been drawn upon and dissipated, whether by solar radiation, or by volcanic action in the earth or other dark bodies of the system, may have been nearly equable, or may even have been less rapid, in certain periods of the past. But it is far more probable that the secular rate of dissipation has been in some direct proportion to the total amount of energy in store, at any time after the commencement of the present order of things, and has been therefore very slowly diminishing from age to age.

3. I have endeavoured to prove this for the sun's heat, in an

article recently published in *Macmillan's Magazine**, where I have shown that most probably the sun was sensibly hotter a million years ago than he is now. Hence, geological speculations assuming somewhat greater extremes of heat, more violent storms and floods, more luxuriant vegetation, and hardier and coarser-grained plants and animals, in remote antiquity, are more probable than those of the extreme quietist, or "uniformitarian," school. A "middle path," not generally safest in scientific speculation, seems to be so in this case. It is probable that hypotheses of grand catastrophes destroying all life from the earth, and ruining its whole surface at once, are greatly in error; it is impossible that hypotheses assuming an equability of sun and storms for 1,000,000 years, can be wholly true.

4. Fourier's mathematical theory of the conduction of heat is a beautiful working out of a particular case belonging to the general doctrine of the "Dissipation of Energy†." A characteristic of the practical solutions it presents is, that in each case a distribution of temperature, becoming gradually equalised through an unlimited future, is expressed as a function of the time, which is infinitely divergent for all times longer past than a definite determinable epoch. The distribution of heat at such an epoch is essentially *initial*—that is to say, it cannot result from any previous condition of matter by natural processes. It is, then, well called an "*arbitrary* initial distribution of heat," in Fourier's great mathematical poem, because that which is rigorously expressed by the mathematical formula could only be realised by action of a power able to modify the laws of dead matter. In an article published about nineteen years ago in the *Cambridge Mathematical Journal*‡, I gave the mathematical criterion for an essentially initial distribution; and in an inaugural essay, *De Motu Caloris per Terræ Corpus*, read before the Faculty of the University of Glasgow in 1846, I suggested, as an application of these principles, that a perfectly complete geothermic survey

* "On the Age of the Sun's Heat," March, 1862: [also *Popular Lectures and Addresses*, Macmillan, 1889].

† *Proceedings Royal Soc. Edin.* Feb. 1852, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy." Also, "On the Restoration of Energy in an Unequally Heated Space," *Phil. Mag.*, 1853, first half year. [Articles LIX. and LXII. Vol. I. above.]

‡ February, 1844.—"Note on Certain Points in the Theory of Heat:" [Art. X. Vol. I. above.]

would give us data for determining an initial epoch in the problem of terrestrial conduction. At the meeting of the British Association in Glasgow in 1855, I urged that special geothermic surveys should be made for the purpose of estimating absolute dates in geology, and I pointed out some cases, especially that of the salt-spring borings at Kreuznach, in Rhenish Prussia, in which eruptions of basaltic rock seem to leave traces of their igneous origin in residual heat*. I hope this suggestion may yet be taken up, and may prove to some extent useful; but the disturbing influences affecting underground temperature, as Professor Phillips has well shown in a recent inaugural address to the Geological Society, are too great to allow us to expect any very precise or satisfactory results.

5. The chief object of the present communication is to estimate from the known general increase of temperature in the earth downwards, the date of the first establishment of that *consistentior status*, which, according to Leibnitz's theory, is the initial date of all geological history.

6. In all parts of the world in which the earth's crust has been examined, at sufficiently great depths to escape large influence of the irregular and of the annual variations of the superficial temperature, a gradually increasing temperature has been found in going deeper. The rate of augmentation (estimated at only $\frac{1}{110}$ th of a degree, Fahr., in some localities, and as much as $\frac{1}{15}$ th of a degree in others, per foot of descent) has not been observed in a sufficient number of places to establish any fair average estimate for the upper crust of the whole earth. But $\frac{1}{50}$ th is commonly accepted as a rough mean; or, in other words, it is assumed as a result of observation, that there is, on the whole, about 1° Fahr. of elevation of temperature per 50 British feet of descent.

7. The fact that the temperature increases with the depth implies a continual loss of heat from the interior, by conduction outwards through or into the upper crust. Hence, since the upper crust does not become hotter from year to year, there must be a secular loss of heat from the whole earth. It is possible that no cooling may result from this loss of heat, but only an exhaustion of potential energy, which in this case could scarcely

* See *British Association Report of 1855 (Glasgow) Meeting*, [Art. LXXXVII. Vol. II. above].

be other than chemical affinity between substances forming part of the earth's mass. But it is certain that either the earth is becoming on the whole cooler from age to age, or the heat conducted out is generated in the interior by temporary dynamical (that is, in this case, chemical) action*. To suppose, as Lyell, adopting the chemical hypothesis, has done†, that the substances, combining together, may be again separated electrolytically by thermo-electric currents, due to the heat generated by their combination, and thus the chemical action and its heat continued in an endless cycle, violates the principles of natural philosophy in exactly the same manner, and to the same degree, as to believe that a clock constructed with a self-winding movement may fulfil the expectations of its ingenious inventor by going for ever.

8. It must indeed be admitted that many geological writers of the Uniformitarian school, who in other respects have taken a profoundly philosophical view of their subject, have argued in a most fallacious manner against hypotheses of violent action in past ages. If they had contented themselves with showing that many existing appearances, although suggestive of extreme violence and sudden change, may have been brought about by long-continued action, or by paroxysms not more intense than some of which we have experience within the periods of human history, their position might have been unassailable; and certainly could not have been assailed except by a detailed discussion of their facts. It would be a very wonderful, but not an absolutely incredible result, that volcanic action has never been more violent on the whole than during the last two or three centuries; but it is as certain that there is now less volcanic energy in the whole earth than there was a thousand years ago, as it is that there is less gunpowder in a "Monitor" after she has been seen to discharge shot and shell, whether at a nearly equable rate or not, for five hours without receiving fresh supplies, than there was at the beginning of the action. Yet this truth has been ignored or

* Another kind of dynamical action, capable of generating heat in the interior of the earth, is the friction which would impede tidal oscillations if the earth were partially or wholly constituted of viscous matter. See a paper by Prof. G. H. Darwin, "On problems connected with the tides of a viscous spheroid," *Phil. Proc. Roy.* 1879, Part II. W. T. July, 1883.

† *Principles of Geology*, chap. xxxi. ed. 1853.

denied by many of the leading geologists of the present day*, because they believe that the facts within their province do not demonstrate greater violence in ancient changes of the earth's surface, or do demonstrate a nearly equable action in all periods.

9. The chemical hypothesis to account for underground heat might be regarded as not improbable, if it was only in isolated localities that the temperature was found to increase with the depth; and, indeed, it can scarcely be doubted that chemical action exercises an appreciable influence (possibly negative, however) on the action of volcanoes; but that there is slow uniform "combustion," *eremacausis*, or chemical combination of any kind going on, at some great unknown depth under the surface everywhere, and creeping inwards gradually as the chemical affinities in layer after layer are successively saturated, seems extremely improbable, although it cannot be pronounced to be absolutely impossible, or contrary to all analogies in nature. The less hypothetical view, however, that the earth is merely a warm chemically inert body cooling, is clearly to be preferred in the present state of science.

10. Poisson's celebrated hypothesis, that the present underground heat is due to a passage, at some former period, of the solar system through hotter stellar regions, cannot provide the circumstances required for a paleontology continuous through that epoch of external heat. For from a mean of values of the conductivity, in terms of the thermal capacity of unit volume, of the earth's crust, in three different localities near Edinburgh, which I have deduced from the observations on underground temperature instituted by Principal Forbes there, I find that if the supposed transit through a hotter region of space took place between 1250 and 5000 years ago, the temperature of that supposed region must have been from 25° to 50° Fahr. above the present mean temperature of the earth's surface, to account for the present general rate of underground increase of temperature, taken as 1° Fahr. in 50 feet downwards. Human history negatives this supposition. Again, geologists and astronomers will, I presume, admit that the earth cannot, 20,000 years ago, have been

* It must be borne in mind that this was written in 1862. The opposite statement concerning the beliefs of geologists would probably be now [1889] nearer the truth. W. T.

in a region of space 100° Fahr. warmer than its present surface. But if the transition from a hot region to a cool region supposed by Poisson took place more than 20,000 years ago, the excess of temperature must have been more than 100° Fahr., and must therefore have destroyed animal and vegetable life. Hence, the farther back and the hotter we can suppose Poisson's hot region, the better for the geologists who require the longest periods; but the best for their view is Leibnitz's theory, which simply supposes the earth to have been at one time an incandescent liquid, without explaining how it got into that state. If we suppose the temperature of melting rock to be about 10,000 Fahr. (an extremely high estimate), the consolidation may have taken place 200,000,000 years ago. Or, if we suppose the temperature of melting rock to be 7000° Fahr. (which is more nearly what it is generally assumed to be), we may suppose the consolidation to have taken place 98,000,000 years ago.

11. These estimates are founded on the Fourier solution demonstrated below. The greatest variation we have to make on them, to take into account the differences in the ratios of conductivities to specific heats of the three Edinburgh rocks, is to reduce them to nearly half, or to increase them by rather more than half. A reduction of the Greenwich underground observations recently communicated to me by Professor Everett of Windsor, Nova Scotia [now, 1889, of Queen's College, Belfast], gives for the Greenwich rocks a quality intermediate between those of the Edinburgh rocks. But we are very ignorant as to the effects of high temperatures in altering the conductivities and specific heats of rocks, and as to their latent heat of fusion. 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, nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of emergence of the *consistentior status* was probably between those dates.

12. The mathematical theory on which these estimates are founded is very simple, being in fact merely an application of

one of Fourier's elementary solutions to the problem of finding at any time the rate of variation of temperature from point to point, and the actual temperature at any point, in a solid extending to infinity in all directions, on the supposition that at an initial epoch the temperature has had two different constant values on the two sides of a certain infinite plane. The solution for the two required elements is as follows:—

$$\frac{dv}{dx} = \frac{V}{\sqrt{\pi\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}},$$

$$v = v_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{\kappa t}}} dz \epsilon^{-z^2},$$

where κ denotes the conductivity of the solid, measured in terms of the thermal capacity of the unit of bulk;

- V , half the difference of the two initial temperatures;
- v_0 , their arithmetical mean;
- t , the time;
- x , the distance of any point from the middle plane;
- v , the temperature of the point x at time t ;

and, consequently (according to the notation of the differential calculus), dv/dx the rate of variation of the temperature per unit of length perpendicular to the isothermal planes.

13. To demonstrate this solution, it is sufficient to verify—

- (1) That the expression for v satisfies Fourier's equation for the linear conduction of heat, viz.;

$$\frac{dv}{dt} = \kappa \frac{d^2v}{dx^2};$$

- (2) That when $t = 0$, the expression for v becomes $v_0 + V$ for all positive, and $v_0 - V$ for all negative, values of x ;

and (3), That the expression for dv/dx is the differential coefficient of the expression for v with reference to x .

The propositions (1) and (3) are proved directly by differentiation. To prove (2), we have, when $t = 0$, and x positive,

$$v = v_0 + \frac{2V}{\pi} \int_0^{\infty} dz \epsilon^{-z^2},$$

or according to the known value, $\frac{1}{2}\sqrt{\pi}$, of the definite integral $\int_0^{\infty} dz \epsilon^{-z^2}$,

$$v = v_0 + V;$$

and for all values of t , the second term has equal positive and negative values for equal positive and negative values of x , so that when $t=0$ and x negative,

$$v = v_0 - V.$$

The admirable analysis by which Fourier arrived at solutions including this, forms a most interesting and important mathematical study. It is to be found in his *Théorie Analytique de la Chaleur*. Paris, 1822.

14. The accompanying diagram (page 303) represents, by two curves, the preceding expressions for dv/dx , and v respectively.

15. The solution thus expressed and illustrated applies, for a certain time, without sensible error, to the case of a solid sphere, primitively heated to a uniform temperature, and suddenly exposed to any superficial action, which for ever after keeps the surface at some other constant temperature. If, for instance, the case considered is that of a globe, 8000 miles in diameter, of solid rock, the solution will apply with scarcely sensible error for more than 1000 millions of years. For, if the rock be of a certain average quality as to conductivity and specific heat, the value of κ , as I have shown in a previous communication to the Royal Society*, will be 400, to unit of length a British foot and unit of time a year; and the equation expressing the solution becomes

$$\frac{dv}{dx} = \frac{V}{35.4} \cdot \frac{1}{\sqrt{t}} \cdot e^{-x^2/1600t};$$

and if we give t the value 1,000,000,000, or anything less, the exponential factor becomes less than e^{-50} (which being equal to about 1/270, may be regarded as insensible), when x exceeds 3,000,000 feet, or 568 miles. That is to say, during the first 1000 million years the variation of temperature does not become sensible at depths exceeding 568 miles, and is therefore confined to so thin a crust, that the influence of curvature may be neglected.

16. If, now, we suppose the time to be 100 million years from the commencement of the variation, the equation becomes

$$\frac{dv}{dx} = \frac{1}{3.54 \times 10^6} V e^{-x^2/1600 \times 10^6}.$$

* "On the Reduction of Observations of Underground Temperature." *Trans. Roy. Soc. Edin.*, March, 1860 [Art. xciii. above].

INCREASE OF TEMPERATURE DOWNWARDS IN THE EARTH.

$ON = x.$

$a = 2\sqrt{kt}.$

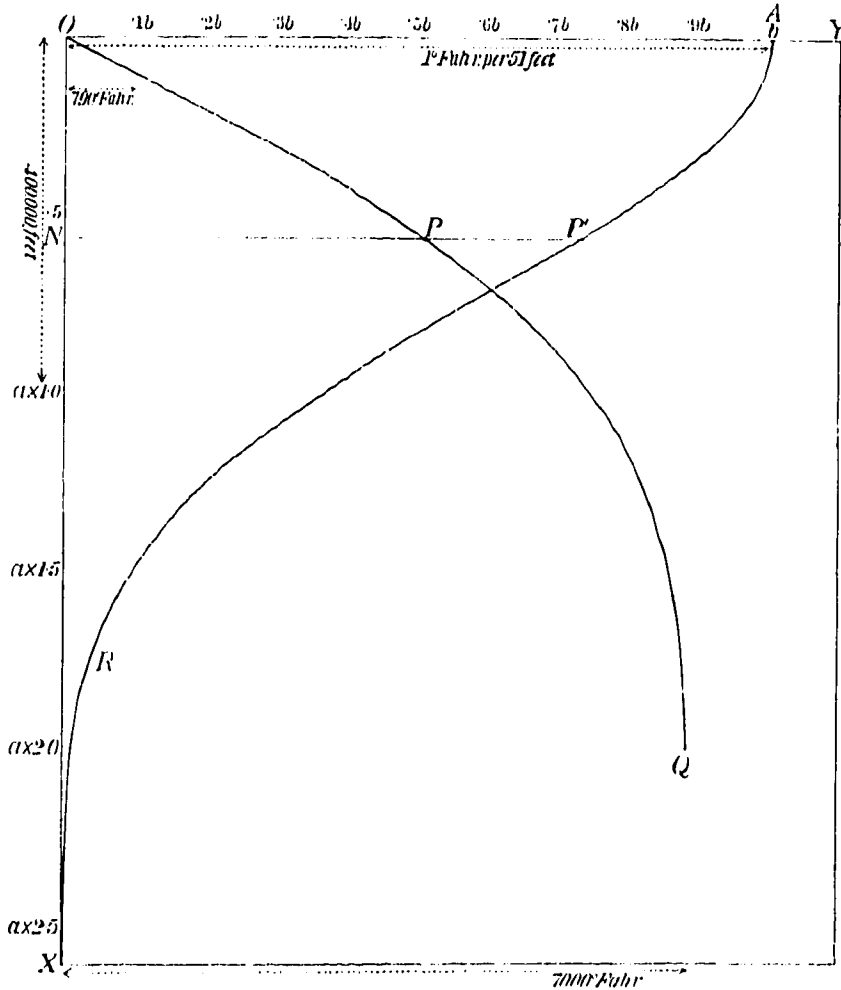
$NP' = b\epsilon^{-x^2/a^2} = y'.$

$\frac{dv}{dx} = \frac{V}{a} \cdot \frac{NP}{b \cdot \frac{1}{2}\sqrt{\pi}}.$

$NP = \text{area } ONP'A \div a = \frac{1}{a} \int_0^x y' dx^2.$

$v - v_0 = V \cdot \frac{NP}{b \cdot \frac{1}{2}\sqrt{\pi}}.$

The curve OPQ shows excess of temperature above that of the surface.
 The curve $AP'R$ shows rate of augmentation of temperature downwards.



* A table of the values of this integral, sometimes now called the "Error Function," is to be found in Table III. of De Morgan's article on "The Theory of Probabilities," *Encyclopaedia Metropolitana*, Edition 1845, Vol. II. W. T. March 27, 1889.

The diagram, therefore, shows the variation of temperature which would now exist in the earth, if its whole mass being first solid and at one temperature 100 million years ago, the temperature of its surface had been everywhere suddenly lowered by V degrees, and kept permanently at this lower temperature: the scales used being as follows:—

(1) For depth below the surface,—scale along OX , length a , represents 400,000 feet.

(2) For rate of increase of temperature per foot of depth,—scale of ordinates parallel to OY , length b , represents $\frac{1}{354000}$ of V per foot. If, for example, $V = 7000^\circ$ Fahr., this scale will be such that b represents $\frac{1}{50}$ of a degree per foot.

(3) For excess of temperature,—scale of ordinates parallel to OY , length b , represents $V/\frac{1}{2}\sqrt{\pi}$, or 7900° , if $V = 7000^\circ$ Fahr.

Thus the rate of increase of temperature from the surface downwards would be sensibly $\frac{1}{50}$ of a degree per foot for the first 100,000 feet or so. Below that depth the rate of increase per foot would begin to diminish sensibly. At 400,000 feet it would have diminished to about $\frac{1}{141}$ of a degree per foot. At 800,000 feet it would have diminished to less than $\frac{1}{50}$ of its initial value,—that is to say, to less than $\frac{1}{3550}$ of a degree per foot; and so on, rapidly diminishing, as shown in the curve. Such is, on the whole, the most probable representation of the earth's present temperature, at depths of from 100 feet, where the annual variations cease to be sensible, to 100 miles; below which the whole mass, or all, except a nucleus cool from the beginning, is (whether liquid or solid) probably at, or very nearly at, the proper melting temperature for the pressure at each depth.

17. The theory indicated above throws light on the question so often discussed as to whether terrestrial heat can have influenced climate through long geological periods, and allows us to answer it very decidedly in the negative. There would be an increment of temperature at the rate of 2° Fahr. per foot downwards near the surface, 10,000 years after the beginning of the cooling, in the case we have supposed. The radiation from earth and atmosphere into space (of which we have yet no satisfactory absolute measurement) would almost certainly be so rapid in the earth's actual circumstances, as not to allow a rate of increase of 2° Fahr. per

foot underground to augment the temperature of the surface by much more than about 1°; and hence I infer that the general climate cannot be sensibly affected by conducted heat, at any time more than 10,000 years after the commencement of superficial solidification. No doubt, however, in particular places there might be an elevation of temperature by thermal springs, or by eruptions of melted lava, and everywhere vegetation would, for the first three or four million years, if it existed so soon after the epoch of consolidation, be influenced by the sensibly higher temperature met with by roots extending a foot or more below the surface.

18. Whatever the amount of such effects is at any one time, it would go on diminishing according to the inverse proportion of the square roots of the times from the initial epoch. Thus, if at 10,000 years we have 2° per foot of increment below ground,

At	40,000 years	we should have	1°	per foot.
"	160,000	"	"	"
"	4,000,000	"	"	"
"	100,000,000	"	"	"

It is, therefore, probable that for the last 96,000,000 years the rate of increase of temperature underground has gradually diminished from about $\frac{1}{10}$ th to about $\frac{1}{100}$ th of a degree Fahrenheit per foot, and that the thickness of the crust through which any stated degree of cooling has been experienced has in that period gradually increased up to its present thickness from $\frac{1}{10}$ th of that thickness. Is not this, on the whole, in harmony with geological evidence, rightly interpreted? Do not the vast masses of basalt, the general appearances of mountain ranges, the violent distortions and fractures of strata, *the great prevalence of metamorphic action* (which must have taken place at depths of not many miles if so much), all agree in demonstrating that the rate of increase of temperature downwards must have been much more rapid, and in rendering it probable that volcanic energy, earthquake shocks, and every kind of so-called Plutonic action, have been, on the whole, more abundantly and violently operative in geological antiquity than in the present age?

19. But it may be objected to this application of mathematical theory—(1), That the earth was once all melted, or at least melted all round its surface, and cannot possibly, or rather cannot with any probability, be supposed to have been ever a uni-

formly heated solid, 7000° Fahr. warmer than our present surface temperature, as assumed in the mathematical problem; and (2), No natural action could possibly produce at one instant, and maintain for ever after, a seven thousand degrees' lowering of the surface temperature. Taking the second objection first, I answer it by saying, what I think cannot be denied, that a large mass of melted rock, exposed freely to our air and sky, will, after it once becomes crusted over, present in a few hours, or a few days, or at the most a few weeks, a surface so cool that it can be walked over with impunity. Hence, after 10,000 years, or, indeed, I may say after a single year, its condition will be sensibly the same as if the actual lowering of temperature experienced by the surface had been produced in an instant and maintained constant ever after. I answer the first objection by saying, that if experimenters will find the latent heat of fusion, and the variations of conductivity and specific heat of the earth's crust up to its melting point, it will be easy to modify the solution given above, so as to make it applicable to the case of a liquid globe gradually solidifying from without inwards, in consequence of heat conducted through the solid crust to a cold external medium. In the meantime, we can see that this modification will not make any considerable change in the resulting temperature of any point in the crust, unless the latent heat parted with on solidification proves, contrary to what we may expect from analogy, to be considerable in comparison with the heat that an equal mass of the solid yields in cooling from the temperature of solidification to the superficial temperature. But, what is more to the purpose, it is to be remarked that the objection, plausible as it appears, is altogether fallacious, and that the problem solved above corresponds much more closely, in all probability, with the actual history of the earth, than does the modified problem suggested by the objection. The earth, although once all melted, or melted all round its surface, did, in all probability, really become a solid at its melting temperature all through, or all through the outer layer, which had been melted; and not until the solidification was thus complete, or nearly so, did the surface begin to cool. That this is the true view can scarcely be doubted, when the following arguments are considered.

20. In the first place, we shall assume that at one time the earth consisted of a solid nucleus, covered all round with a very

deep ocean of melted rocks, and left to cool by radiation into space. This is the condition that would supervene, on a cold body much smaller than the present earth meeting a great number of cool bodies still smaller than itself, and is therefore in accordance with what we may regard as a probable hypothesis regarding the earth's antecedents. It includes, as a particular case, the commoner supposition, that the earth was once melted throughout, a condition which might result from the collision of two nearly equal masses. But the evidence which has convinced most geologists that the earth had a fiery beginning, goes but a very small depth below the surface, and affords us absolutely no means of distinguishing between the actual phenomena, and those which would have resulted from either an entire globe of liquid rock, or a cool solid nucleus covered with liquid to any depth exceeding 50 or 100 miles. Hence, irrespectively of any hypothesis as to antecedents from which the earth's initial fiery condition may have followed by natural causes, and simply assuming, as rendered probable by geological evidence, that there was at one time melted rock all over the surface, we need not assume the depth of this lava ocean to have been more than 50 or 100 miles; although we need not exclude the supposition of any greater depth, or of an entire globe of liquid.

21. In the process of refrigeration, the fluid must (as I have remarked regarding the sun, in the recent article in *Macmillan's Magazine** already referred to, and regarding the earth's atmosphere, in a communication to the Literary and Philosophical Society of Manchester†) be brought by convection, to fulfil a definite law of distribution of temperature which I have called "convective equilibrium of temperature." That is to say, the temperatures at different parts in the interior must, in any great fluid mass which is kept well stirred, differ according to the different pressures by the difference of temperatures which any one portion of the liquid would present, if given at the temperature and pressure of any one part, and then subjected to variation of pressure, while prevented from losing or gaining heat. The

* "On the Age of the Sun's Heat," March, 1862: also *Popular Lectures and Addresses*, Vol. I. Macmillan, 1889.

† "On the Convective Equilibrium of Temperature in the Atmosphere," read Jan. 21, 1862: published in the *Memoirs*, Vol. II. of 3rd Series; [Art. xcii. above, Appendix E.]

reason for this is the extreme slowness of true thermal conduction; and the consequently preponderating influence of great currents throughout a continuous fluid mass, in determining the distribution of temperature through the whole.

22. The thermo-dynamic law connecting temperature and pressure in a fluid mass, not allowed to lose or gain heat, investigated theoretically, and experimentally verified in the cases of air and water, by Dr Joule and myself*, shows, therefore, that the temperature in the liquid will increase from the surface downwards, if, as is most probably the case, the liquid contracts in cooling. On the other hand, if the liquid, like water near its freezing point, expanded in cooling, the temperature, according to the convective and thermo-dynamic laws just stated (§§ 21, 22), would actually be lower at great depths than near the surface, even although the liquid is cooling from the surface; but there would be a very thin superficial layer of lighter and cooler liquid, losing heat by true conduction, until solidification at the surface would commence.

23. Again, according to the thermo-dynamic law of freezing, investigated by my brother, Professor James Thomson†, and verified by myself experimentally for water‡, the temperature of solidification will, at great depths, because of the great pressure there, be higher there than at the surface if the fluid contracts, or lower than at the surface if it expands, in becoming solid.

24. How the temperature of solidification, for any pressure, may be related to the corresponding temperature of fluid con-

* Joule, "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," *Philosophical Magazine*, May, 1845; or *Joule's Scientific Papers*, Vol. 1, London, 1884. Thomson, "On a Method for Discovering Experimentally the Mechanical Work spent, and the Heat produced by the Compression of a Gaseous Fluid," *Trans. Roy. Soc. Edin.*, Vol. xx. Part II., read April 21, 1851; *Philosophical Magazine*, December, 1852 [Art. XLVIII. Part IV. §§ 61-80, Vol. 1. above]. Joule and Thomson, "On the Thermal Effects of Fluids in Motion," *Trans. Roy. Soc.*, read June 16, 1853, and June 15, 1854 [Art. XLIX. Vol. 1. above]. Thomson, "On the Alterations of Temperature accompanying Changes of Pressure in Fluids," *Proc. Roy. Soc.*, Vol. VIII., read June 15, 1857 [Art. XCII. App. A., above].

† "Theoretical Considerations Regarding the Effect of Pressure on the Freezing Point of Water," *Trans. R. S. E.*, Jan. 1849. [Appendix to Art. XLII. Vol. 1. above.]

‡ *Proceedings R. S. E.*, Jan. 1850; *Philosophical Magazine*, 1850, Vol. XXXVII. &c. [Art. XLII. Vol. 1. above].

vective equilibrium, it is impossible to say, without knowledge which we do not yet possess, regarding the expansion with heat, and the specific heat of the fluid, and the change of volume and the latent heat developed in the transition from fluid to solid.

25. For instance, supposing, as is most probably true, both that the liquid contracts in cooling towards its freezing-point, and that it contracts in freezing; we cannot tell, without definite numerical data regarding those elements, whether the elevation of the temperature of solidification, or of the actual temperature of a portion of the fluid given just above its freezing point, produced by a given application of pressure, is the greater. If the former is greater than the latter, solidification would commence at the bottom, or at the centre if there is no solid nucleus to begin with, and would proceed outwards; and there could be no complete permanent incrustation all round the surface till the whole globe is solid, with, possibly, the exception of irregular, comparatively small spaces of liquid.

26. If, on the contrary, the elevation of temperature, produced by an application of pressure to a given portion of the fluid, is greater than the elevation of the freezing temperature produced by the same amount of pressure, the superficial layer of the fluid would be the first to reach its freezing point, and the first actually to freeze.

27. But if, according to the second supposition of § 22 above, the liquid expanded in cooling near its freezing-point, the solid would probably likewise be of less specific gravity than the liquid at its freezing-point. Hence the surface would crust over permanently with a crust of solid, constantly increasing inwards by the freezing of the interior fluid in consequence of heat conducted out through the crust. The condition most commonly assumed by geologists would thus be produced.

28. But Bischof's experiments, upon the validity of which, so far as I am aware, no doubt has ever been thrown, show that melted granite, slate, and trachyte, all contract by something about 20 per cent. in freezing. We ought, indeed, to have more experiments on this most important point, both to verify Bischof's results on rocks, and to learn how the case is with iron and other un-oxidised metals. In the meantime we must assume it as probable

that the melted substance of the earth did really contract by a very considerable amount in becoming solid.

29. Hence, if according to any relations whatever among the complicated physical circumstances concerned, freezing did really commence at the surface, either all round or in any part, before the whole globe had become solid, the solidified superficial layer must have broken up and sunk to the bottom, or to the centre, before it could have attained a sufficient thickness to rest stably on the lighter liquid below. It is quite clear, indeed, that if at any time the earth were in the condition of a thin solid shell of, let us suppose 50 feet or 100 feet thick of granite, enclosing a continuous melted mass of 20 per cent. less specific gravity in its upper parts, where the pressure is small, this condition cannot have lasted many minutes. The rigidity of a solid shell of superficial extent, so vast in comparison with its thickness, must be as nothing, and the slightest disturbance must cause some part to bend down, crack, and allow the liquid to run out over the whole solid. The crust itself must in consequence become shattered into fragments, which must all sink to the bottom, or to meet in the centre and form a nucleus there if there is none to begin with.

30. It is, however, scarcely possible, that any such continuous crust can ever have formed all over the melted surface at one time, and afterwards have fallen in. The mode of solidification conjectured in § 25 above, seems on the whole the most consistent with what we know of the physical properties of the matter concerned. So far as regards the result, it agrees, I believe, with the view adopted as the most probable by Mr Hopkins*. But whether from the condition being rather that described in § 26 above, which seems also possible, for the whole or for some parts of the heterogeneous substance of the earth, or from the viscidness as of mortar, which necessarily supervenes in a melted fluid, composed of ingredients becoming, as the whole cools, separated by crystallising at different temperatures, before the solidification is perfect, and which we actually see in lava from modern volcanoes; it is probable that when the whole globe, or some very thick superficial layer of it, still liquid or viscid, has cooled down to near its temperature of perfect solidification, incrustation at the surface must commence.

* See his Report on "Earthquakes and Volcanic Action," *British Association Report for 1847*.

31. It is probable that crust may thus form over wide extents of surface, and may be temporarily buoyed up by the vesicular character it may have retained from the ebullition of the liquid in some places; or, at all events, it may be held up by the viscosity of the liquid, until it has acquired some considerable thickness sufficient to allow gravity to manifest its claim, and sink the heavier solid below the lighter liquid. This process must go on until the sunk portions of crust build up from the bottom a sufficiently close ribbed solid skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava pools or lakes.

32. In the honey-combed solid and liquid mass thus formed, there must be a continual tendency for the liquid, in consequence of its less specific gravity, to work its way up; whether by masses of solid falling from the roofs of vesicles or tunnels, and causing earthquake shocks, or by the roof breaking quite through when very thin, so as to cause two such hollows to unite, or the liquid of any of them to flow out freely over the outer surface of the earth; or by gradual subsidence of the solid, owing to the thermodynamic melting, which portions of it, under intense stress, must experience, according to views recently published by my brother, Professor James Thomson*. The results which must follow from this tendency seem sufficiently great and various to account for all that we see at present, and all that we learn from geological investigation, of earthquakes, of upheavals and subsidences of solid, and of eruptions of melted rock.

33. These conclusions, drawn solely from a consideration of the necessary order of cooling and consolidation, according to Bischof's result as to the relative specific gravities of solid and of melted rock, are in perfect accordance with what I have recently demonstrated † regarding the present condition of the earth's interior,—that it is not, as commonly supposed, all liquid within a thin solid crust of from 30 to 100 miles thick, but that it is on the whole more rigid certainly than a continuous solid globe of glass of the same diameter, and probably than one of steel.

* "On Crystallization and Liquefaction as influenced by Stresses tending to Change of Form in Crystals," *Proceedings of the Royal Society*, Vol. xi., read Dec. 5, 1861.

† In a paper "On the Rigidity of the Earth," communicated to the Royal Society a few days ago; April, 1862 [Art. xcv. below].