unity, and if it yields tidally they should have equal values. The very close agreement between them is probably somewhat due to chance. From this point of view it seems reasonable to combine all the observations, resulting from 66 years of observation, for both sorts of tides together.

Then writing X and Y for the numerical factors by which the equilibrium values of the two components of either tide are to be multiplied in order to give the actual results, I find

$$X = \cdot 676 \pm \cdot 076, \quad Y = \cdot 029 \pm \cdot 065.$$

Tidal yielding of the earth's mass. Rigidity about equal to or greater than that of steel.

These results really seem to present evidence of a tidal yielding of the earth's mass, showing that it has an effective rigidity about equal to that of steel *.

But this result is open to some doubt for the following reason:

Taking only the Indian results (48 years in all), which are much more consistent than the English ones, I find

$$X = \cdot 931 \pm \cdot 056, \quad Y = \cdot 155 \pm \cdot 068.$$

We thus see that the more consistent observations seem to bring out the tides more nearly to their theoretical equilibrium-values with no elastic yielding of the solid.

It is to be observed however that the Indian results being confined within a narrow range of latitude give (especially when we consider the absence of minute accuracy in the evaluation of $\mathfrak{C}$ in § 848 (c)) a less searching test for the elastic yielding, than a combination of results from all latitudes.

On the whole we may fairly conclude that, whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and that the effective rigidity is at least as great as that of steel.

* It is remarkable that elastic yielding of the upper strata of the earth, in the case where the sea does not cover the whole surface, may lead to an apparent augmentation of oceanic tides at some places, situated on the coasts of continents. This subject is investigated in the Report for 1882 of the Committee of the British Association on "The Lunar Disturbance of Gravity." It is there, however, erroneously implied that this kind of elastic yielding would cause an apparent augmentation of tide at all stations of observation.

APPENDIX TO CHAPTER VII.

The following Appendices are reprints of papers published at various times. Excepting where it is expressly so stated, or where it is obvious from the context, they speak as from the date of publication. The marginal notes however to the appendices which appeared in the first edition speak as at the date of issue of that edition, viz. 1867; *in the new appendices the marginal notes are now added for the first time.*

(C.)—Equations of Equilibrium of an Elastic Solid deduced from the Principle of Energy*.

(*a*) Let a solid composed of matter fulfilling no condition of isotropy in any part, and not homogeneous from part to part, be given of any shape, unstrained, and let every point of its surface be altered in position to a given distance in a given direction. It is required to find the displacement of every point of its substance, in equilibrium. Let x, y, z be the co-ordinates of any particle, P, of the substance in its undisturbed position, and $x+\alpha$, $y+\beta$, $z+\gamma$ its co-ordinates when displaced in the manner specified: that is to say, let α, β, γ be the components of the required displacement. Then, if for brevity we put

Strain of any magnitude specified by six elements.

$$\left.\begin{aligned}
A &= \left(\frac{d\alpha}{dx}+1\right)^2 + \left(\frac{d\beta}{dx}\right)^2 + \left(\frac{d\gamma}{dx}\right)^2\\
B &= \left(\frac{d\alpha}{dy}\right)^2 + \left(\frac{d\beta}{dy}+1\right)^2 + \left(\frac{d\gamma}{dy}\right)^2\\
C &= \left(\frac{d\alpha}{dz}\right)^2 + \left(\frac{d\beta}{dz}\right)^2 + \left(\frac{d\gamma}{dz}+1\right)^2\\
a &= \frac{d\alpha}{dy}\frac{d\alpha}{dz} + \left(\frac{d\beta}{dy}+1\right)\frac{d\beta}{dz} + \frac{d\gamma}{dy}\left(\frac{d\gamma}{dz}+1\right)\\
b &= \frac{d\alpha}{dz}\left(\frac{d\alpha}{dx}+1\right) + \frac{d\beta}{dz}\frac{d\beta}{dx} + \left(\frac{d\gamma}{dz}+1\right)\frac{d\gamma}{dx}\\
c &= \left(\frac{d\alpha}{dx}+1\right)\frac{d\alpha}{dy} + \frac{d\beta}{dx}\left(\frac{d\beta}{dy}+1\right) + \frac{d\gamma}{dx}\frac{d\gamma}{dy}
\end{aligned}\right\}\ \ldots\ldots\ldots(1);$$

these six quantities A, B, C, a, b, c are proved [§ 190 (*e*) and § 181 (5)] to thoroughly determine the strain experienced by the

* Appendix to a paper by Sir W. Thomson on "Dynamical problems regarding Elastic Spheroidal Shells and Spheroids of incompressible liquid." *Phil. Trans.* 1863, Vol. 153, p. 610.

Strain specified by six elements.

substance infinitely near the particle P (irrespectively of any rotation it may experience), in the following manner:

(*b.*) Let ξ, η, ζ be the undisturbed co-ordinates of a particle infinitely near P, relatively to axes through P parallel to those of x, y, z respectively; and let $\xi_,$, $\eta_,$, $\zeta_,$ be the co-ordinates relative still to axes through P, when the solid is in its strained condition. Then

$$\xi_,^2+\eta_,^2+\zeta_,^2 = A\xi^2+B\eta^2+C\zeta^2+2a\eta\zeta+2b\zeta\xi+2c\xi\eta \;\ldots\ldots(2);$$

and therefore all particles which in the strained state lie on a spherical surface

$$\xi_,^2+\eta_,^2+\zeta_,^2 = r_,^2,$$

are in the unstrained state, on the ellipsoidal surface,

$$A\xi^2+B\eta^2+C\zeta^2+2a\eta\zeta+2b\zeta\xi+2c\xi\eta = r_,^2.$$

This (§§ 155—165) completely defines the homogeneous strain of the matter in the neighbourhood of P.

Anticipatory application of the Carnot and Clausius thermodynamic law:

(*c.*) Hence, the thermodynamic principles by which, in a paper on the "Thermo-elastic Properties of Matter*," Green's dynamical theory of elastic solids was demonstrated as part of the modern dynamical theory of heat, show that if $w dxdydz$ denote the work required to alter an infinitely small undisturbed volume, $dxdydz$, of the solid, into its disturbed condition, when its temperature is kept constant, we must have

its combination with Joule's law expressed analytically for elastic solid:

$$w = f(A,\ B,\ C,\ a,\ b,\ c) \;\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

where f denotes a positive function of the six elements, which vanishes when $A-1$, $B-1$, $C-1$, a, b, c each vanish. And if W denote the whole work required to produce the change actually experienced by the whole solid, we have

Potential energy of deformation;

$$W = \iiint w dxdydz \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4)$$

where the triple integral is extended through the space occupied by the undisturbed solid.

a minimum for stable equilibrium.

(*d.*) The position assumed by every particle in the interior of the solid will be such as to make this a minimum subject to the condition that every particle of the surface takes the position given to it; this being the elementary condition of stable equilibrium. Hence, by the method of variations

$$\delta W = \iiint \delta w dxdydz = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots(5).$$

* *Quarterly Journ. of Math.*, April, 1855, or *Mathematical and Physical Papers* by Sir W. Thomson, 1882, Art. XLVIII. Part VII.

Potential energy of deformation; a minimum for stable equilibrium.

But, exhibiting only terms depending on $\delta\alpha$, we have

$$\begin{aligned}\delta w = &\left\{2\frac{dw}{dA}\left(\frac{d\alpha}{dx}+1\right)+\frac{dw}{db}\frac{d\alpha}{dz}+\frac{dw}{dc}\frac{d\alpha}{dy}\right\}\frac{d\delta\alpha}{dx}\\ &+\left\{2\frac{dw}{dB}\frac{d\alpha}{dy}+\frac{dw}{da}\frac{d\alpha}{dz}+\frac{dw}{dc}\left(\frac{d\alpha}{dx}+1\right)\right\}\frac{d\delta\alpha}{dy}\\ &+\left\{2\frac{dw}{dC}\frac{d\alpha}{dz}+\frac{dw}{da}\frac{d\alpha}{dy}+\frac{dw}{db}\left(\frac{d\alpha}{dx}+1\right)\right\}\frac{d\delta\alpha}{dz}\\ &+\text{etc.}\end{aligned}$$

Hence, integrating by parts, and observing that $\delta\alpha$, $\delta\beta$, $\delta\gamma$ vanish at the limiting surface, we have

$$\delta W = -\iiint dxdydz\left\{\left(\frac{dP}{dx}+\frac{dQ}{dy}+\frac{dR}{dz}\right)\delta\alpha+\text{etc.}\right\}\;\ldots\ldots(6)$$

where for brevity P, Q, R denote the multipliers of $\frac{d\delta\alpha}{dx}$, $\frac{d\delta\alpha}{dy}$, $\frac{d\delta\alpha}{dz}$ respectively, in the preceding expression. In order that δW may vanish, the multipliers of $\delta\alpha$, $\delta\beta$, $\delta\gamma$, in the expression now found for it, must each vanish, and hence we have, as the equations of equilibrium

Equations of internal equilibrium of an elastic solid experiencing no bodily force.

$$\left.\begin{aligned}&\frac{d}{dx}\left\{2\frac{dw}{dA}\left(\frac{d\alpha}{dx}+1\right)+\frac{dw}{db}\frac{d\alpha}{dz}+\frac{dw}{dc}\frac{d\alpha}{dy}\right\}\\ &+\frac{d}{dy}\left\{2\frac{dw}{dB}\frac{d\alpha}{dy}+\frac{dw}{da}\frac{d\alpha}{dz}+\frac{dw}{dc}\left(\frac{d\alpha}{dx}+1\right)\right\}\\ &+\frac{d}{dz}\left\{2\frac{dw}{dC}\frac{d\alpha}{dz}+\frac{dw}{da}\frac{d\alpha}{dy}+\frac{dw}{db}\left(\frac{d\alpha}{dx}+1\right)\right\}=0\\ &\qquad\text{etc.}\quad\text{etc.}\end{aligned}\right\}\;\ldots\ldots(7),$$

of which the second and third, not exhibited, may be written down merely by attending to the symmetry.

Their solution proved possible and unique when surface displacement is given, unless there can be unstable equilibrium:

(*e.*) From the property of w that it is necessarily positive when there is any strain, it follows that there must be some distribution of strain through the interior which shall make $\iiint w dxdydz$ *the least possible*, subject to the prescribed surface condition; and therefore that the solution of equations (7) subject to this condition, is possible. If, whatever be the nature of the solid as to difference of elasticity in different directions, in any part, and as to heterogeneity from part to part, and whatever be the extent of the change of form and dimensions to which it is subjected, there cannot be any internal configuration of unstable equilibrium, nor consequently any but one of stable equilibrium, with the prescribed surface displacement, and no disturbing force on the interior; then, besides being always positive, w must be such a function of A, B, etc., that there can be only one solution of the equations. This is obviously the case when the unstrained solid is homogeneous.

hence necessarily unique for a homogeneous solid.

Extension of the analysis to include bodily force, and data of surface easy.

(*f.*) It is easy to include, in a general investigation similar to the preceding, the effects of any force on the interior substance, such as we have considered particularly for a spherical shell, of homogeneous isotropic matter, in §§ 730...737 above. It is also easy to adapt the general investigation to superficial data of *force*, instead of displacement.

Transition to case of infinitely small strains.

Green's theory:

(*g.*) Whatever be the general form of the function f for any part of the substance, since it is always positive it cannot change in sign when $A-1, B-1, C-1, a, b, c$, have their signs changed; and therefore for infinitely small values of these quantities it must be a homogeneous quadratic function of them with constant coefficients. (And it may be useful to observe that for all values of the variables A, B, etc., it must therefore be expressible in the same form, with varying coefficients, each of which is always finite, for all values of the variables.) Thus, for infinitely small strains we have Green's theory of elastic solids, founded on a homogeneous quadratic function of the components of strain, expressing the work required to produce it. Thus, putting

$$A-1=2e,\quad B-1=2f,\quad C-1=2g \;\ldots\ldots\ldots\ldots(8)$$

and denoting by $\frac{1}{2}(e,e)$, $\frac{1}{2}(f,f)$,...(e,f),...(e,a),... the coefficients, we have, as above (§ 673),

$$w=\tfrac{1}{2}\left.\begin{aligned}\{(e,e)\,e^2+(f,f)f^2+(g,g)\,g^2+(a,a)\,a^2+(b,b)\,b^2+(c,c)\,c^2\}\\ +(e,f)\,ef+(e,g)\,eg+(e,a)\,ea+(e,b)\,eb+(e,c)\,ec\\ +(f,g)fg+(f,a)fa+(f,b)fb+(f,c)fc\\ +(g,a)\,ga+(g,b)\,gb+(g,c)\,gc\\ +(a,b)\,ab+(a,c)\,ac\\ +(b,c)\,bc\end{aligned}\right\}(9).$$

(*h.*) When the strains are infinitely small the products $\dfrac{dw}{dA}\dfrac{da}{dx}$, $\dfrac{dw}{db}\dfrac{da}{dz}$, etc., are each infinitely small, of the second order. We

therefore omit them; and then attending to (8), we reduce (7) to

Case of infinitely small strains:—

dynamic equations of internal equilibrium:

$$\left.\begin{aligned}\frac{d}{dx}\frac{dw}{de}+\frac{d}{dy}\frac{dw}{dc}+\frac{d}{dz}\frac{dw}{db}=0\\ \frac{d}{dx}\frac{dw}{dc}+\frac{d}{dy}\frac{dw}{df}+\frac{d}{dz}\frac{dw}{da}=0\\ \frac{d}{dx}\frac{dw}{db}+\frac{d}{dy}\frac{dw}{da}+\frac{d}{dz}\frac{dw}{dg}=0\end{aligned}\right\}\ldots\ldots\ldots\ldots(10),$$

which are the equations of interior equilibrium. Attending to (9) we see that $\dfrac{dw}{de}\ldots\dfrac{dw}{da}\ldots$ are linear functions of e, f, g, a, b, c the components of strain. Writing out one of them as an example we have

$$\frac{dw}{de}=(e,e)\,e+(e,f)f+(e,g)\,g+(e,a)\,a+(e,b)\,b+(e,c)\,c\ldots(11).$$

And, α, β, γ denoting, as before, the component displacements of any interior particle, P, from its undisturbed position (x, y, z) we have, by (8) and (1)

and relative kinematic equations.

$$\left.\begin{aligned}e=\frac{d\alpha}{dx},\quad f=\frac{d\beta}{dy},\quad g=\frac{d\gamma}{dz}\\ a=\frac{d\beta}{dz}+\frac{d\gamma}{dy},\quad b=\frac{d\gamma}{dx}+\frac{d\alpha}{dz},\quad c=\frac{d\alpha}{dy}+\frac{d\beta}{dx}\end{aligned}\right\}\ldots\ldots(12).$$

It is to be observed that the coefficients (e,e), (e,f), etc., will be in general functions of (x, y, z), but will be each constant when the unstrained solid is homogeneous.

Solution proved unique whether the body is homogeneous or heterogeneous, when surface displacement is given:

(*i.*) It is now easy to prove directly, for the case of infinitely small strains, that the solution of the equations of interior equilibrium, whether for a heterogeneous or a homogeneous solid, subject to the prescribed surface condition, is unique. For, let α, β, γ be components of displacement fulfilling the equations, and let α', β', γ' denote any other functions of x, y, z, having the same surface values as α, β, γ, and let $e', f', \ldots, w'$ denote functions depending on them in the same way as $e, f, \ldots, w$ depend on α, β, γ. Thus by Taylor's theorem,

$$w'-w=\frac{dw}{de}(e'-e)+\frac{dw}{df}(f'-f)+\frac{dw}{dg}(g'-g)+\frac{dw}{da}(a'-a)+\frac{dw}{db}(b'-b)+\frac{dw}{dc}(c'-c)+H,$$

where H denotes the same homogeneous quadratic function of

Case of infinitely small strains:—

$e' - e$, etc., that w is of e, etc. If for $e' - e$, etc., we substitute their values by (12), this becomes

$$w' - w = \frac{dw}{de}\frac{d(\alpha'-\alpha)}{dx} + \frac{dw}{db}\frac{d(\alpha'-\alpha)}{dz} + \frac{dw}{dc}\frac{d(\alpha'-\alpha)}{dy} + \text{etc.} + H.$$

Multiplying by $dxdydz$, integrating by parts, observing that $\alpha' - \alpha$, $\beta' - \beta$, $\gamma' - \gamma$ vanish at the bounding surface, and taking account (10), we find simply

$$\iiint (w' - w)\, dxdydz = \iiint H dxdydz \ldots\ldots\ldots\ldots\ldots (13).$$

But H is essentially positive. Therefore every other interior condition than that specified by α, β, γ, provided only it has the same bounding surface, requires a greater amount of work than w to produce it: and the excess is equal to the work that would be required to produce, from a state of no displacement, such a displacement as superimposed on α, β, γ, would produce the other. And inasmuch as α, β, γ, fulfil only the conditions of satisfying (11) and having the given surface values, it follows that no other than one solution can fulfil these conditions.

solution not necessarily unique, when the surface data are of force.

(*j.*) But (as has been pointed out to us by Stokes) when the surface data are of force, not of displacement, or when force acts from without, on the interior substance of the body, the solution is not in general unique, and there may be configurations of unstable equilibrium even with infinitely small displacement. For instance, let part of the body be composed of a steel-bar magnet; and let a magnet be held outside in the same line, and with a pole of the same name in its end nearest to one end of the inner magnet. The equilibrium will be unstable, and there will be positions of stable equilibrium with the inner bar slightly inclined to the line of the outer bar, unless the rigidity of the rest of the body exceed a certain limit.

Condition that substance may be isotropic, without limitation to infinitely small strains:

(*k.*) Recurring to the general problem, in which the strains are not supposed infinitely small; we see that if the solid is isotropic in every part, the function of A, B, C, a, b, c which expresses w, must be merely a function of the roots of the equation [§ 181 (11)]

$$(A-\zeta^2)(B-\zeta^2)(C-\zeta^2) - a^2(A-\zeta^2) - b^2(B-\zeta^2) - c^2(C-\zeta^2) + 2abc = 0 \ldots (14)$$

which (that is the positive values of ζ) are the ratios of elongation along the principal axes of the strain-ellipsoid. It is unnecessary here to enter on the analytical expression of this condition. For in the case of $A-1$, $B-1$, $C-1$, a, b, c each infinitely small, it obviously requires that

$$\left.\begin{array}{l}(e,e)=(f,f)=(g,g);\ (f,g)=(g,e)=(e,f);\ (a,a)=(b,b)=(c,c);\\ (e,a)=(f,b)=(g,c)=0;\ (b,c)=(c,a)=(a,b)=0;\ \text{and}\\ (e,b)=(e,c)=(f,c)=(f,a)=(g,a)=(g,b)=0.\end{array}\right\}\ldots(15).$$

Thus the 21 coefficients are reduced to three—

(e, e) which we may denote by the single letter $\mathfrak{A}$,
(f, g) " " " " $\mathfrak{B}$,
(a, a) " " " " n.

expressed in equations among the moduli of elasticity for case of infinitely small strains.

It is clear that this is necessary and sufficient for insuring *cubic isotropy;* that is to say, perfect equality of elastic properties with reference to the three rectangular directions OX, OY, OZ. But for *spherical isotropy*, or complete isotropy with reference to all directions through the substance, it is further necessary that

$$\mathfrak{A} - \mathfrak{B} = 2n \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (16);$$

as is easily proved analytically by turning two of the axes of co-ordinates in their own plane through 45°; or geometrically by examining the nature of the strain represented by any one of the elements a, b, c (a simple shear) and comparing it with the resultant of c, and $f = -e$ (which is also a simple shear). It is convenient now to put

$$\mathfrak{A} + \mathfrak{B} = 2m;\ \text{so that}\ \mathfrak{A} = m + n,\ \mathfrak{B} = m - n \ldots\ldots\ldots (17);$$

and thus the expression for the potential energy per unit of volume becomes

Potential energy of infinitely small strain in isotropic solid.

$$2w = m(e+f+g)^2 + n(e^2+f^2+g^2-2fg-2ge-2ef+a^2+b^2+c^2) \ldots (18).$$

Using this in (9), and substituting for e, f, g, a, b, c their values by (12), we find immediately the equations of internal equilibrium, which are the same as (6) of § 698.

(*l.*) To find the mutual force exerted across any surface within the solid, as expressed by (1) of § 662, we have clearly, by considering the work done respectively by P, Q, R, S, T, U (§ 662) on any infinitely small change of figure or dimensions in the solid,

Components of stress required for infinitely small strain.

$$P = \frac{dw}{de},\quad Q = \frac{dw}{df},\quad R = \frac{dw}{dg},\quad S = \frac{dw}{da},\quad T = \frac{dw}{db},\quad U = \frac{dw}{dc} \ldots (19).$$

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Hence, for an isotropic solid, (18) gives the expressions which we have used above, (12) of § 673.

Moduli of resistance to compression and of rigidity.

(m.) To interpret the coefficients m and n in connexion with elementary ideas as to the elasticity of the solid; first let $a=b=c=0$, and $e=f=g=\frac{1}{3}\delta$: in other words, let the substance experience a uniform dilatation, in all directions, producing an expansion of volume from 1 to $1+\delta$. In this case (18) becomes

$$w=\tfrac{1}{2}(m-\tfrac{1}{3}n)\delta^2;$$

and we have

$$\frac{dw}{d\delta}=(m-\tfrac{1}{3}n)\delta.$$

Hence $(m-\frac{1}{3}n)\delta$ is the normal force per unit area of its surface required to keep any portion of the solid expanded to the amount specified by δ. Thus $m-\frac{1}{3}n$ measures the elastic force called out by, or the elastic resistance against, change of volume: and viewed as a *modulus of elasticity*, it may be called the bulk-modulus. [Compare §§ 692, 693, 694, 688, 682, and 680.] What is commonly called the "compressibility" is measured by $1/(m-\frac{1}{3}n)$.

And let next $e=f=g=b=c=0$; which gives

$$w=\tfrac{1}{2}na^2; \text{ and, by (19), } S=na.$$

This shows that the tangential force per unit area required to produce an infinitely small shear (§ 171), amounting to a, is na. Hence n measures the innate power of the body to resist change of shape, and return to its original shape when force has been applied to change it: that is to say, it measures *the rigidity* of the substance.

(D).—On the Secular Cooling of the Earth*.

Appendix D. Dissipation of energy disregarded by many followers of Hutton.

(a.) 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.

* *Transactions of the Royal Society of Edinburgh*, 1862 (W. Thomson).

(b.) 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. Dissipation of energy from the solar system.

(c.) I have endeavoured to prove this for the sun's heat, in an article recently published in *Macmillan's Magazine* (March 1862)*, 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. Terrestrial climate influenced by the probably hotter sun of a few million years ago.

(d.) 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

* Reprinted as Appendix E, below.

† *Proceedings of Royal Soc. Edin.*, Feb. 1852. "On a universal Tendency in Nature to the Dissipation of Mechanical Energy," *Mathematical and Physical Papers*, by Sir W. Thomson, 1882, Art. LIX. Also, "On the Restoration of Energy in an unequally Heated Space," *Phil. Mag.*, 1853, first half year, *Mathematical and Physical Papers*, by Sir W. Thomson, 1882, Art. LXII.

distribution of temperature, becoming gradually equalized 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 realized 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 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 Creuznach, 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 ‡.

Mathematicians' use of word "arbitrary" metaphysically significant.

Criterion of an essentially "initial" distribution of heat in a solid:

now applied to estimate date of earth's consolidation, from data of present underground temperature.

Value of local geothermic surveys, for estimation of absolute dates in geology.

(*e*.) 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.

* Feb. 1844.—"Note on Certain Points in the Theory of Heat," *Mathematical and Physical Papers*, by Sir W. Thomson, 1882, Vol. I. Art. x.

† See British Association Report of 1855 (Glasgow) Meeting.

‡ Much work in the direction suggested above has been already carried out by the Committee of the British Association, on Underground Temperature.

(*f*.) 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 other, 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.

Increase of temperature downwards in earth's crust; but very imperfectly observed hitherto.

(*g*.) 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 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.

Secular loss of heat out of the earth demonstrated:

but not so any present or past secular cooling, however probable.

Fallacy of a thermo-electric perpetual motion.

* 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 Mr G. H. Darwin, "On problems connected with the tides of a viscous spheroid." *Phil. Trans.* Part II. 1879.

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

Exception to the soundness of arguments adduced in the promulgation and prosecution of the Huttonian reform.

(*h.*) 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 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.

Secular diminution of whole amount of volcanic energy quite certain:

but not in 1862 admitted by some of the chief geologists.

(*i.*) 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

Chemical hypothesis to account for ordinary underground heat not impossible, but very improbable.

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

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.

(*j.*) 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 palæontology 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, 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 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 further 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.

Poisson's hypothesis to account for ordinary underground heat proved impossible without destruction of life.

Poisson's hypothesis disproved as any acceptable mitigation of Leibnitz's theory.

(*k.*) These estimates are founded on the Fourier solution demonstrated below. The greatest variation we have to make in them, to take into account the differences in the ratios of con-

Probable limits of uncertainty as to thermal con-

ductivities and capacities of surface rocks.

ductivities 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, 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.

Extreme admissible limits of date of earth's consolidation.

(*l.*) 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:—

Mathematical expression for interior temperature near the surface of a hot solid commencing to cool:

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

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

where κ denotes the conductivity of the solid, measured in terms of the thermal capacity of the unity 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 and 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.

(*m.*) 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,

proof.

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

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.

(*n.*) The accompanying diagram (page 477) represents, by two curves, the preceding expressions for dv/dx and v respectively.

(*o.*) 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 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 found in a previous communication to the Royal

Expression for interior temperature near surface of a hot body commencing to cool:

proved to be practically approximate for the earth for 100 million years.

Society,* will be 400, for unit of length a British foot and unit of time a year; and the equation expressing the solution becomes

$$\frac{dv}{dx} = \frac{1}{35 \cdot 4} \frac{V}{\sqrt{t}} \epsilon^{-x^2/1600t};$$

and if we give t the value 1,000,000,000, or anything less, the exponential factor becomes less than $\epsilon^{-5 \cdot 6}$ (which being equal to about $\frac{1}{270}$, 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.

(*p*.) If, now, we suppose the time to be 100 million years from the commencement of the variation, the equation becomes

$$\frac{dv}{dx} = \frac{1}{3 \cdot 54 \times 10^5} V \epsilon^{-x^2/1600 \times 10^8}.$$

Distribution of temperature 100 million years after commencement of cooling of a great enough mass of average rock.

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, $V = 7000°$ Fahr. this scale will be such that b represents $\frac{1}{50 \cdot 6}$ of a degree Fahr. 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 $V = 7000°$ Fahr.

Thus the rate of increase of temperature from the surface downwards would be sensibly $\frac{1}{51}$ 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

* "On the Periodical Variations of Underground Temperature." *Trans. Roy. Soc. Edin.*, March 1860.

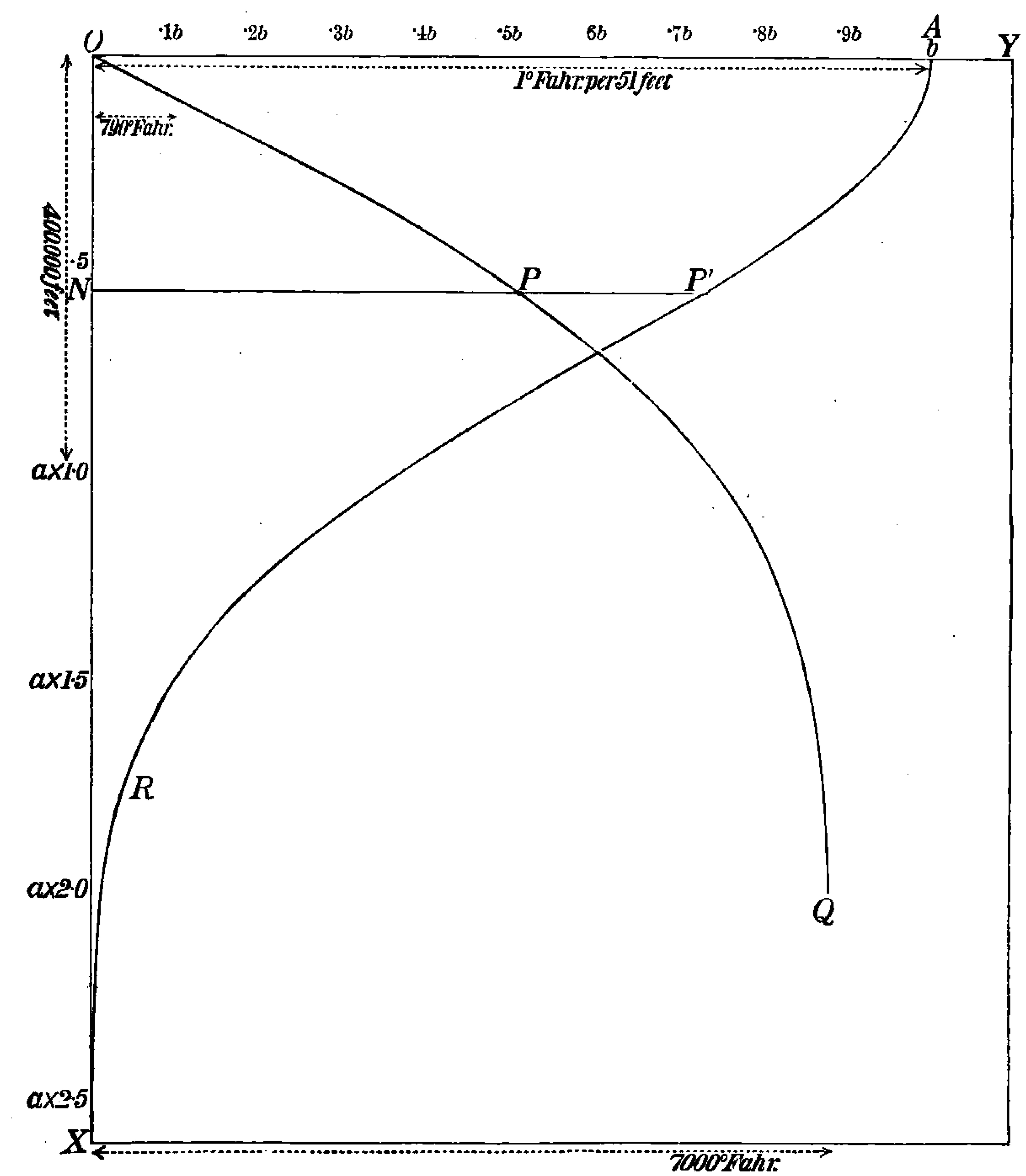
INCREASE OF TEMPERATURE DOWNWARDS IN THE EARTH.

Distribution of temperature 100 million years after commencement of cooling of a great enough mass of average rock:

$ON = x.$ $\quad a = 2\sqrt{\kappa t}.$

$NP' = b\epsilon^{-x^2/a^2} = y'.$ $\quad \frac{dv}{dx} = \frac{V}{a} \cdot \frac{NP}{b\frac{1}{2}\sqrt{\pi}}.$

$NP = area\ ONP'A \div a = \frac{1}{a}\int_0^x y' dx.$ $\quad v - v_0 = V \cdot \frac{NP}{b \cdot \frac{1}{2}\sqrt{\pi}}.$



graphically represented

OPQ curve showing excess of temperature above that of the surface.

AP'R curve showing rate of augmentation of temperature downwards.

800,000 feet it would have diminished to less than $\frac{1}{50}$ of its initial value,—that is to say, to less than $\frac{1}{2550}$ 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.

Terrestrial climate not sensibly influenced by underground heat.

(*q*.) 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.

Rates of increase of temperature inwards in a great enough mass of average rock, at various times after commencement of cooling from a primitive temperature of 7000° Fahr.

(*r*.) 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	„	„	$\frac{1}{2}$°	„
„	4,000,000	„	„	$\frac{1}{10}$°	„
„	100,000,000	„	„	$\frac{1}{50}$°	„

It is therefore probable that for the last 96,000,000 years the rate of increase of temperature under ground has gradually diminished from about $\frac{1}{10}$th to about $\frac{1}{50}$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 $\frac{1}{5}$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?

Objections to terrestrial application raised and removed.

(*s*.) 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 uniformly 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

Objections to terrestrial application raised and removed.

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.

Present underground temperature probably due to heat generated in the original building of the earth.

(*t*.) 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

Primitive heating may have been throughout, or merely through a superficial layer of no greater depth than $\frac{1}{80}$ of the radius.

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.

(*u*.) In the process of refrigeration, the fluid must [as I have remarked regarding the sun, in a recent article in *Macmillan's Magazine* (March, 1862)*, 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 part, and then subjected to variation of pressure, but prevented from losing or gaining heat. The 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.

"Convective equilibrium of temperature" defined:

must have been approximately fulfilled until solidification commenced.

(*v*.) 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

* See Appendix E, below.

† *Proceedings*, Jan. 1862. "On the Convective Equilibrium of Temperature in the Atmosphere."

‡ Joule, "On the Changes of Temperature produced by the Rarefaction and Condensation of Air," *Phil. Mag.* 1845. Thomson, "On a Method for Determining Experimentally the Heat evolved by the Compression of Air;" Dynamical Theory of Heat, Part IV., *Trans. R. S. E.*, Session 1850-51; and reprinted *Phil. Mag.* Joule and Thomson, "On the Thermal Effects of Fluids in Motion," *Trans. R. S. Lond.*, June 1853 and June 1854. Joule and Thomson, "On the Alterations of Temperature accompanying Changes of Pressure in Fluids," *Proceedings R. S. Lond.*, June 1857. These articles, except the first by Joule, are all now republished in Vol. I. Arts. XLVIII. and XLIX. of *Mathematical and Physical Papers*, by Sir W. Thomson.

Alternative cases as to distribution of temperature before solidification.

freezing-point, expands in cooling, the temperature, according to the convective and thermo-dynamic laws just stated (§§ *u*, *v*), 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.

Effect of pressure on the temperature of solidification.

(*w*.) 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, be higher there than at the surface if the fluid contracts, or lower than at the surface if it expands, in becoming solid.

(*x*.) How the temperature of solidification, for any pressure, may be related to the corresponding temperature of fluid convective 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.

Question whether solidification commenced at surface or centre or bottom.

(*y*.) 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.

(*z*.) If, on the contrary, the elevation of temperature, produced

* "Theoretical Considerations regarding the Effect of Pressure in lowering the Freezing-point of Water," *Trans. R. S. E.*, Jan. 1849. Republished by permission of the author, in Vol. I. (pp. 156—164) of *Mathematical and Physical Papers*, by Sir W. Thomson, 1882.

† *Proceedings R. S. E.*, Session 1849-50. *Mathematical and Physical Papers*, by Sir W. Thomson, 1882, p. 165.

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.

(*aa*.) But if, according to the second supposition of § *v*, 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.

Importance of experimental investigation of contraction or expansion of melted rocks in solidification.

(*bb*.) But Bischof's experiments, upon the validity of which, as 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 unoxydised metals. In the meantime we must consider it as probable that the melted substance of the earth did really contract by a very considerable amount in becoming solid.

Bischof's experiments proving contraction make it probable that the surface was never allowed to cool till solidification was very nearly complete through the interior.

(*cc*.) 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 would cause some part to bend down, crack, and allow the liquid to run out over the whole solid. The crust itself would in consequence become shattered into fragments, which must all sink to the bottom, or meet in the centre and form a nucleus there if there is none to begin with.

(*dd.*) 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 § *y*, 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 § *z*, which seems also possible, for the whole or for some parts of the heterogeneous substance of the earth, or from the viscidity as of mortar, which necessarily supervenes in a melted fluid, composed of ingredients becoming, as the whole cools, separated by crystallizing 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.

(*ee.*) 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 viscidity 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.

Probable cause of volcano and earthquakes.

(*ff.*) 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

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

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 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.

(*gg.*) 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 §§ 832...848, 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.

(E.) On the Age of the Sun's Heat†.

The second great law of Thermodynamics involves a certain principle of *irreversible action in nature*. It is thus shown that, although mechanical energy is *indestructible*, there is a universal tendency to its dissipation, which produces gradual augmentation and diffusion of heat, cessation of motion, and exhaustion of potential energy through the material universe‡. The result would inevitably be a state of universal rest and death, if the universe were finite and left to obey existing laws. But it is impossible to conceive a limit to the extent of matter in the universe; and therefore science points rather to an endless progress, through an endless space, of action involving the trans-

Dissipation of Energy.

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

† From *Macmillan's Magazine*, March 1862.

‡ See *Proceedings R.S.E.* Feb. 1852, or *Phil. Mag.* 1853, first half year, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy." *Math. and Phys. Papers*, by Sir W. Thomson, 1882, Art. LIX.

formation of potential energy into palpable motion and thence into heat, than to a single finite mechanism, running down like a clock, and stopping for ever. It is also impossible to conceive either the beginning or the continuance of life, without an overruling creative power; and, therefore, no conclusions of dynamical science regarding the future condition of the earth, can be held to give dispiriting views as to the destiny of the race of intelligent beings by which it is at present inhabited.

The object proposed in the present article is an application of these general principles to the discovery of probable limits to the periods of time, past and future, during which the sun can be reckoned on as a source of heat and light. The subject will be discussed under three heads:—

I. The secular cooling of the sun.

II. The present temperature of the sun.

III. The origin and total amount of the sun's heat.

PART I.

ON THE SECULAR COOLING OF THE SUN.

Rate of cooling of sun unknown.

How much the sun is actually cooled from year to year, if at all, we have no means of ascertaining, or scarcely even of estimating in the roughest manner. In the first place we do not know that he is losing heat at all. For it is quite certain that *some heat* is generated in his atmosphere by the influx of meteoric matter; and it is possible that the *amount* of heat so generated from year to year is sufficient to compensate the loss by radiation. It is, however, also possible that the sun is now an incandescent liquid mass, radiating away heat, either primitively created in his substance, or, what seems far more probable, generated by the falling in of meteors in past times, with no sensible compensation by a continuance of meteoric action.

Heat generated by fall of meteors into the sun

It has been shown* that, if the former supposition were true, the meteors by which the sun's heat would have been produced during the last 2,000 or 3,000 years must have been during all

* "On the Mechanical Energies of the Solar System." *Transactions of the Royal Society of Edinburgh*, 1854, and *Phil. Mag.* 1854, second half-year. *Math. and Phys. Papers*, by Sir W. Thomson (Art. LXVI. of Vol. II. now in the press).

that time much within the earth's distance from the sun, and must therefore have approached the central body in very gradual spirals; because, if enough of matter to produce the supposed thermal effect fell in from space outside the earth's orbit, the length of the year would have been very sensibly shortened by the additions to the sun's mass which must have been made. The quantity of matter annually falling in must, on that supposition, have amounted to $\frac{1}{47}$ of the earth's mass, or to $\frac{1}{17,000,000}$ of the sun's; and therefore it would be necessary to suppose the zodiacal light to amount to at least $\frac{1}{5000}$ of the sun's mass, to account in the same way for a future supply of 3,000 years' sun-heat. When these conclusions were first published it was pointed out that "disturbances in the motions of visible planets" should be looked for, as affording us means for estimating the possible amount of matter in the zodiacal light; and it was conjectured that it could not be nearly enough to give a supply of 300,000 years' heat at the present rate. These anticipations have been to some extent fulfilled in Le Verrier's great researches on the motion of the planet Mercury, which have recently given evidence of a sensible influence attributable to matter circulating as a great number of small planets within his orbit round the sun. But the amount of matter thus indicated is very small; and, therefore, if the meteoric influx taking place at present is enough to produce any appreciable portion of the heat radiated away, it must be supposed to be from matter circulating round the sun, within very short distances of his surface. The density of this meteoric cloud would have to be supposed so great that comets could scarcely have escaped, as comets actually have escaped, showing no discoverable effects of resistance, after passing his surface within a distance equal to $\frac{1}{8}$ of his radius. All things considered, there seems little probability in the hypothesis that solar radiation is compensated, to any appreciable degree, by heat generated by meteors falling in, at present; and, as it can be shown that no chemical theory is tenable*, it must be concluded as most probable that the sun is at present merely an incandescent liquid mass cooling.

insufficient to give heat supply.

because the matter in zodiacal light and intra-mercurial planets is certainly small.

The sun an incandescent cooling mass.

How much he cools from year to year, becomes therefore a

* "Mechanical Energies," &c. referred to above.

question of very serious import, but it is one which we are at present quite unable to answer. It is true we have data on which we might plausibly found a probable estimate, and from which we might deduce, with at first sight seemingly well founded confidence, limits, not very wide, within which the present true rate of the sun's cooling must lie. For we know, from the independent but concordant investigations of Herschel and Pouillet, that the sun radiates every year from his whole surface about 6×10^{30} (six million million million million million) times as much heat as is sufficient to raise the temperature of 1 lb. of water by 1° Cent. We also have excellent reason for believing that the sun's substance is very much like the earth's. Stokes's principles of solar and stellar chemistry have been for many years explained in the University of Glasgow, and it has been taught as a first result that sodium does certainly exist in the sun's atmosphere, and in the atmospheres of many of the stars, but that it is not discoverable in others. The recent application of these principles in the splendid researches of Bunsen and Kirchhof (who made an independent discovery of Stokes's theory) has demonstrated with equal certainty that there are iron and manganese, and several of our other known metals, in the sun. The specific heat of each of these substances is less than the specific heat of water, which indeed exceeds that of every other known terrestrial body, solid or liquid. It might, therefore, at first sight seem probable that the mean specific heat* of the sun's whole substance is less, and very certain that it cannot be much greater, than that of water. If it were equal to the specific heat of water we should only have to divide the preceding number (6×10^{30}), derived from Herschel's and Pouillet's observations, by the number of pounds ($4{\cdot}23 \times 10^{30}$) in the sun's mass, to find 1°·4 Cent. for the present annual rate of

Pouillet's and Herschel's estimates of solar radiation.

Largeness of specific heat of sun

* The "specific heat" of a homogeneous body is the quantity of heat that a unit of its substance must acquire or must part with, to rise or to fall by 1° in temperature. The mean specific heat of a heterogeneous mass, or of a mass of homogeneous substance, under different pressures in different parts, is the quantity of heat which the whole body takes or gives in rising or in falling 1° in temperature, divided by the number of units in its mass. The expression, "mean specific heat" of the sun, in the text, signifies the total amount of heat actually radiated away from the sun, divided by his mass, during any time in which the average temperature of his mass sinks by 1°, whatever physical or chemical changes any part of his substance may experience.

cooling. It might therefore seem probable that the sun cools more, and almost certain that he does not cool less, than a centigrade degree and four-tenths annually. But, if this estimate were well founded, it would be equally just to assume that the sun's expansibility* with heat does not differ greatly from that of some average terrestrial body. If, for instance, it were the same as that of solid glass, which is about $\frac{1}{40000}$ of bulk, or $\frac{1}{120000}$ of diameter, per 1° Cent. (and for most terrestrial liquids, especially at high temperatures, the expansibility is much more), and if the specific heat were the same as that of liquid water, there would be in 860 years a contraction of one per cent. on the sun's diameter, which could scarcely have escaped detection by astronomical observation. There is, however, a far stronger reason than this for believing that no such amount of contraction can have taken place, and therefore for suspecting that the physical circumstances of the sun's mass render the condition of the substances of which it is composed, as to expansibility and specific heat, very different from that of the same substances when experimented on in our terrestrial laboratories. Mutual gravitation between the different parts of the sun's contracting mass must do an amount of work, which cannot be calculated with certainty, only because the law of the sun's interior density is not known. The amount of work performed during a contraction of one-tenth per cent. of the diameter, if the density remained uniform through the interior, would, as Helmholtz showed, be equal to 20,000 times the mechanical equivalent of the amount of heat which Pouillet estimated to be radiated from the sun in a year. But in reality the sun's density must increase very much towards his centre, and probably in varying proportions, as the temperature becomes lower and the whole mass contracts. We cannot, therefore, say whether the work actually done by mutual gravitation during a contraction of one-tenth per cent. of the diameter, would be

and smallness of expansibility rendered probable by absence of sensible contraction in solar diameter.

Work done in contraction of solar diameter by $\frac{1}{1000}$ may give heat supply for perhaps 20,000 years.

* The "expansibility in volume," or the "cubical expansibility," of a body, is an expression technically used to denote the proportion which the increase or diminution of its bulk, accompanying a rise or fall of 1° in its temperature, bears to its whole bulk at some stated temperature. The expression, "the sun's expansibility," used in the text, may be taken as signifying the ratio which the actual contraction, during a lowering of his mean temperature by 1° Cent., bears to his present volume.

more or less than the equivalent of 20,000 years' heat; but we may regard it as most probably not many times more or less than this amount. Now, it is in the highest degree improbable that mechanical energy can in any case increase in a body contracting in virtue of cooling. It is certain that it really does diminish very notably in every case hitherto experimented on. It must be supposed, therefore, that the sun always radiates away in heat something more than the Joule-equivalent of the work done on his contracting mass, by mutual gravitation of its parts. Hence, in contracting by one-tenth per cent. in his diameter, or three-tenths per cent. in his bulk, the sun must give out something either more, or not greatly less, than 20,000 years' heat; and thus, even without historical evidence as to the constancy of his diameter, it seems safe to conclude that no such contraction as that calculated above one per cent. in 860 years can have taken place in reality. It seems, on the contrary, probable that, at the present rate of radiation, a contraction of one-tenth per cent. in the sun's diameter could not take place in much less than 20,000 years, and scarcely possible that it could take place in less than 8,600 years. If, then, the mean specific heat of the sun's mass, in its actual condition, is not more than ten times that of water, the expansibility in volume must be less than $\frac{1}{4000}$ per 100° Cent., (that is to say, less than $\frac{1}{10}$ of that of solid glass,) which seems improbable. But although from this consideration we are led to regard it as probable that the sun's specific heat is considerably more than ten times that of water (and, therefore, that his mass cools considerably less than 100° in 700 years, a conclusion which, indeed, we could scarcely avoid on simply geological grounds), the physical principles we now rest on fail to give us any reason for supposing that the sun's specific heat is more than 10,000 times that of water, because we cannot say that his expansibility in volume is probably more than $\frac{1}{400}$ per 1° Cent. And there is, on other grounds, very strong reason for believing that the specific heat is really much less than 10,000. For it is almost certain that the sun's mean temperature* is even now as high as 14,000°

* [Rosetti (*Phil. Mag.* 1879, 2nd half year) estimates the effective radiational temperature of the sun as "not much less than ten thousand degrees Centigrade:" (9965° is the number expressing the results of his measurements). On the other hand, C. W. Siemens estimates it at as low as 3000° Cent. The mean tem-

Cent.; and the greatest quantity of heat that we can explain, with any probability, to have been by natural causes ever acquired by the sun (as we shall see in the third part of this article), could not have raised his mass at any time to this temperature, unless his specific heat were less than 10,000 times that of water.

Sun's specific hea probably between 10 and 10,000 times that of water; and fall of temperature 100° Cent. in from 700 to 700,000 years.

We may therefore consider it as rendered highly probable that the sun's specific heat is more than ten times, and less than 10,000 times, that of liquid water. From this it would follow with certainty that his temperature sinks 100° Cent. in some time from 700 years to 700,000 years.

PART II.

ON THE SUN'S PRESENT TEMPERATURE.

At his surface the sun's temperature cannot, as we have many reasons for believing, be incomparably higher than temperatures attainable artificially in our terrestrial laboratories.

Sun's superficial temperature comparable with what may be artificially produced.

Among other reasons it may be mentioned that the sun radiates heat, from every square foot of his surface, at only about 7,000 horse power*. Coal, burning at a rate of a little less than a pound per two seconds, would generate the same amount; and it is estimated (Rankine, 'Prime Movers,' p. 285, Ed. 1859) that, in the furnaces of locomotive engines, coal burns at from one pound in thirty seconds to one pound in ninety seconds, per square foot of grate-bars. Hence heat is radiated from the sun at a rate not more than from fifteen to forty-five times as high as that at which heat is generated on the grate-bars of a locomotive furnace, per equal areas.

perature of the whole sun's mass must (Part II. below) be much higher than the "surface temperature," or "effective radiational temperature."—W. T. Nov. 9, 1882.]

* One horse power in mechanics is a technical expression (following Watt's estimate), used to denote a rate of working in which energy is evolved at the rate of 33,000 foot pounds per minute. This, according to Joule's determination of the dynamical value of heat, would, if spent wholly in heat, be sufficient to raise the temperature of 23¾ lbs. of water by 1° Cent. per minute.

[Note of Nov. 11, 1882. This is sixty-seven times the rate per unit of radiant surface at which energy is emitted from the incandescent filament of the Swan electric lamp when at the temperature which gives about 240 candles per horse power.]

Interior temperature probably far higher.

The interior temperature of the sun is probably far higher than that at his surface, because direct conduction can play no sensible part in the transference of heat between the inner and outer portions of his mass, and there must in virtue of the prodigious convective currents due to cooling of the outermost portions by radiation into space, be an approximate *convective* equilibrium of heat throughout the whole, if the whole is fluid. That is to say, the temperatures, at different distances from the centre, must be approximately those which any portion of the substance, if carried from the centre to the surface, would acquire by expansion without loss or gain of heat.

Law of temperature probably roughly that of convective equilibrium.

PART III.

ON THE ORIGIN AND TOTAL AMOUNT OF THE SUN'S HEAT.

The sun being, for reasons referred to above, assumed to be an incandescent liquid now losing heat, the question naturally occurs, How did this heat originate? It is certain that it cannot have existed in the sun through an infinity of past time, since, as long as it has so existed, it must have been suffering dissipation, and the finiteness of the sun precludes the supposition of an infinite primitive store of heat in his body.

The sun must, therefore, either have been created an active source of heat at some time of not immeasurable antiquity, by an over-ruling decree; or the heat which he has already radiated away, and that which he still possesses, must have been acquired by a natural process, following permanently established laws. Without pronouncing the former supposition to be essentially incredible, we may safely say that it is in the highest degree improbable, if we can show the latter to be not contradictory to known physical laws. And we do show this and more, by merely pointing to certain actions, going on before us at present, which, if sufficiently abundant at some past time, must have given the sun heat enough to account for all we know of his past radiation and present temperature.

Solar heat must arise from conversion of kinetic and potential energy.

It is not necessary at present to enter at length on details regarding the meteoric theory, which appears to have been first proposed in a definite form by Mayer, and afterwards independently by Waterston; or regarding the modified hypothesis of meteoric vortices, which the writer of the present article showed to be necessary, in order that the length of the year, as known for the last 2,000 years, may not have been sensibly disturbed by the accessions which the sun's mass must have had during that period, if the heat radiated away has been always compensated by heat generated by meteoric influx.

For the reasons mentioned in the first part of the present article, we may now believe that all theories of complete, or nearly complete, contemporaneous meteoric compensation, must be rejected; but we may still hold that—

"*Meteoric action .... is .... not only proved to exist as a cause of solar heat, but it is the only one of all conceivable causes which we know to exist from independent evidence**."

The form of meteoric theory which now seems most probable, and which was first discussed on true thermodynamic principles by Helmholtz†, consists in supposing the sun and his heat to have originated in a coalition of smaller bodies, falling together by mutual gravitation, and generating, as they must do according to the great law demonstrated by Joule, an exact equivalent of heat for the motion lost in collision.

That some form of the meteoric theory is certainly the true and complete explanation of solar heat can scarcely be doubted, when the following reasons are considered:

Chemical action insufficient, but meteoric theory may easily explain heat for 20 million years.

(1) No other natural explanation, except by chemical action, can be conceived.

(2) The chemical theory is quite insufficient, because the most energetic chemical action we know, taking place between substances amounting to the whole sun's mass, would only generate about 3,000 years' heat‡.

(3) There is no difficulty in accounting for 20,000,000 years' heat by the meteoric theory.

* "Mechanical Energies of the Solar System," referred to above.

† Popular lecture delivered on the 7th February, 1854, at Königsberg, on the occasion of the Kant commemoration.

‡ "Mechanical Energies of the Solar System."

It would extend this article to too great a length, and would require something of mathematical calculation, to explain fully the principles on which this last estimate is founded. It is enough to say that bodies, all much smaller than the sun, falling together from a state of relative rest, at mutual distances all large in comparison with their diameters, and forming a globe of uniform density equal in mass and diameter to the sun, would generate an amount of heat which, accurately calculated according to Joule's principles and experimental results, is found to be just 20,000,000 times Pouillet's estimate of the annual amount of solar radiation. The sun's density must, in all probability, increase very much towards his centre, and therefore a considerably greater amount of heat than that must be supposed to have been generated if his whole mass was formed by the coalition of comparatively small bodies. On the other hand, we do not know how much heat may have been dissipated by resistance and minor impacts before the final conglomeration; but there is reason to believe that even the most rapid conglomeration that we can conceive to have probably taken place could only leave the finished globe with about half the entire heat due to the amount of potential energy of mutual gravitation exhausted. We may, therefore, accept, as a lowest estimate for the sun's initial heat, 10,000,000 times a year's supply at present rate, but 50,000,000 or 100,000,000 as possible, in consequence of the sun's greater density in his central parts.

Only about half the heat due to energy of matter concentrating in sun available for explaining solar temperature.

The considerations adduced above, in this paper, regarding the sun's possible specific heat, rate of cooling, and superficial temperature, render it probable that he must have been very sensibly warmer one million years ago than now; and, consequently, that if he has existed as a luminary for ten or twenty million years, he must have radiated away considerably more than ten or twenty million times the present yearly amount of loss.

The sun has probably not lighted the earth for 100 million years.

It seems, therefore, on the whole most probable that the sun has not illuminated the earth for 100,000,000 years, and almost certain that he has not done so for 500,000,000 years. As for the future, we may say, with equal certainty, that inhabitants of the earth cannot continue to enjoy the light and heat essential to their life, for many million years longer, unless sources now unknown to us are prepared in the great storehouse of creation.

(F.)—On the Size of Atoms*.

The idea of an atom has been so constantly associated with incredible assumptions of infinite strength, absolute rigidity, mystical actions at a distance, and indivisibility, that chemists and many other reasonable naturalists of modern times, losing all patience with it, have dismissed it to the realms of metaphysics, and made it smaller than "anything we can conceive." But if atoms are inconceivably small, why are not all chemical actions infinitely swift? Chemistry is powerless to deal with this question, and many others of paramount importance, if barred by the hardness of its fundamental assumptions, from contemplating the atom as a real portion of matter occupying a finite space, and forming a not immeasurably small constituent of any palpable body.

More than thirty years ago naturalists were scared by a wild proposition of Cauchy's, that the familiar prismatic colours proved the "sphere of sensible molecular action" in transparent liquids and solids to be comparable with the wave-length of light. The thirty years which have intervened have only confirmed that proposition. They have produced a large number of capable judges; and it is only incapacity to judge in dynamical questions that can admit a doubt of the substantial correctness of Cauchy's conclusion. But the "sphere of molecular action" conveys no very clear idea to the non-mathematical mind. The idea which it conveys to the mathematical mind is, in my opinion, irredeemably false. For I have no faith whatever in attractions and repulsions acting at a distance between centres of force according to various laws. What Cauchy's mathematics really proves is this: that in palpably homogeneous bodies such as glass or water, contiguous portions are not similar when their dimensions are moderately small fractions of the wave-length. Thus in water contiguous cubes, each of one one-thousandth of a centimetre breadth are sensibly similar. But contiguous cubes of one ten-millionth of a centimetre must be very sensibly different. So in a solid mass of brickwork, two adjacent lengths of 20,000 centimetres each, may contain, one of them nine hundred and ninety-nine bricks and two half bricks, and the

Meaning of sphere of molecular action.

Meaning of homogeneity.

* *Nature*, March 1870.

other one thousand bricks: thus two contiguous cubes of 20,000 centimetres breadth may be considered as sensibly similar. But two adjacent lengths of forty centimetres each might contain one of them, one brick, and two half bricks, and the other two whole bricks; and contiguous cubes of forty centimetres would be very sensibly dissimilar. In short, optical dynamics leaves no alternative but to admit that the diameter of a molecule, or the distance from the centre of a molecule to the centre of a contiguous molecule in glass, water, or any other of our transparent liquids and solids, exceeds a ten-thousandth of the wave-length, or a two-hundred-millionth of a centimetre.

Contact electricity of metals.

By experiments on the contact electricity of metals made in the year 1862, and described in a letter to Dr Joule*, which was published in the proceedings of the Literary and Philosophical Society of Manchester [Jan. 1862], I found that plates of zinc and copper connected with one another by a fine wire attract one another, as would similar pieces of one metal connected with the two plates of a galvanic element, having about three-quarters of the electro-motive force of a Daniel's element.

Energy of electric attraction between plates of different metals in metallic contact.

Measurements published in the Proceedings of the Royal Society for 1860 showed that the attraction between parallel plates of one metal held at a distance apart small in comparison with their diameters, and kept connected with such a galvanic element, would experience an attraction amounting to two ten-thousand-millionths of a gramme weight per area of the opposed surfaces equal to the square of the distance between them. Let a plate of zinc and a plate of copper, each a centimetre square and a hundred-thousandth of a centimetre thick, be placed with a corner of each touching a metal globe of a hundred-thousandth of a centimetre diameter. Let the plates, kept thus in metallic communication with one another be at first wide apart, except at the corners touching the little globe, and let them then be gradually turned round till they are parallel and at a distance of a hundred-thousandth of a centimetre asunder. In this position they will attract one another with a force equal in all to two grammes weight. By abstract dynamics and the theory of energy, it is readily proved that the work done by the changing force of attraction during the motion by which we have supposed

* [Now published as Art. XXII. in a "Reprint of Papers on Electrostatics and Magnetism" by Sir William Thomson. New edition, 1883.]

this position to be reached, is equal to that of a constant force of two grammes weight acting through a space of a hundred-thousandth of a centimetre; that is to say, to two hundred-thousandths of a centimetre-gramme. Now let a second plate of zinc be brought by a similar process to the other side of the plate of copper; a second plate of copper to the remote side of this second plate of zinc, and so on till a pile is formed consisting of 50,001 plates of zinc and 50,000 plates of copper, separated by 100,000 spaces, each plate and each space one hundred-thousandth of a centimetre thick. The whole work done by electric attraction in the formation of this pile is two centimetre-grammes.

Work done in forming pile of zinc and copper plates.

The whole mass of metal is eight grammes. Hence the amount of work is a quarter of a centimetre-gramme per gramme of metal. Now 4,030 centimetre-grammes of work, according to Joule's dynamical equivalent of heat, is the amount required to warm a gramme of zinc or copper by one degree Centigrade. Hence the work done by the electric attraction could warm the substance by only $\frac{1}{16120}$ of a degree. But now let the thickness of each piece of metal and of each intervening space be a hundred-millionth of a centimetre instead of a hundred thousandth. The work would be increased a million-fold unless a hundred-millionth of a centimetre approaches the smallness of a molecule. The heat equivalent would therefore be enough to raise the temperature of the material by 62°. This is barely, if at all, admissible, according to our present knowledge, or, rather, want of knowledge, regarding the heat of combination of zinc and copper. But suppose the metal plates and intervening spaces to be made yet four times thinner, that is to say, the thickness of each to be a four hundred-millionth of a centimetre. The work and its heat equivalent will be increased sixteen-fold. It would therefore be 990 times as much as that required to warm the mass by 1° cent., which is very much more than can possibly be produced by zinc and copper in entering into molecular combination. Were there in reality anything like so much heat of combination as this, a mixture of zinc and copper powders would, if melted in any one spot, run together, generating more than heat enough to melt each throughout; just as a large quantity of gunpowder if ignited in any one spot burns throughout without fresh application of heat. Hence plates of zinc and copper of a

The heat of combination of zinc and copper shows that molecules probably are at least 10^{-8} cm. and certainly more than $\frac{1}{4}\times10^{-8}$ cm in diameter.

three hundred-millionth of a centimetre thick, placed close together alternately, form a near approximation to a chemical combination, if indeed such thin plates could be made without splitting atoms.

Work done in stretching fluid film against surface tension.

The theory of capillary attraction shows that when a bubble—a soap-bubble for instance—is blown larger and larger, work is done by the stretching of a film which resists extension as if it were an elastic membrane with a constant contractile force. This contractile force is to be reckoned as a certain number of units of force per unit of breadth. Observation of the ascent of water in capillary tubes shows that the contractile force of a thin film of water is about sixteen milligrammes weight per millimetre of breadth. Hence the work done in stretching a water film to any degree of thinness, reckoned in millimetre-milligrammes, is equal to sixteen times the number of square millimetres by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of its contractile force. In an article "On the Thermal effect of drawing out a Film of Liquid," published in the Proceedings of the Royal Society for April 1858, I have proved from the second law of thermodynamics that about half as much more energy, in the shape of heat, must be given to the film to prevent it from sinking in temperature while it is being drawn out. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature increases by twenty-four milligramme-millimetres for every square millimetre added to its area.

Intrinsic energy of a mass of water estimated from the heat required to prevent film from cooling as it extends.

Suppose then a film to be given with a thickness of a millimetre, and suppose its area to be augmented ten thousand and one fold: the work done per square millimetre of the original film, that is to say per milligramme of the mass would be 240,000 millimetre-milligrammes. The heat equivalent of this is more than half a degree centigrade of elevation of temperature of the substance. The thickness to which the film is reduced on this supposition is very approximately a ten-thousandth of millimetre. The commonest observation on the soap-bubble (which in contractile force differs no doubt very little from pure water) shows that there is no sensible diminution of contractile force by reduction of the thickness to the ten-thousandth of a millimetre; inasmuch as the thickness which

gives the first maximum brightness round the black spot seen where the bubble is thinnest, is only about an eight-thousandth of a millimetre.

The very moderate amount of work shown in the preceding estimates is quite consistent with this deduction. But suppose now the film to be farther stretched until its thickness is reduced to a twenty-millionth of a millimetre. The work spent in doing this is two-thousand times more than that which we have just calculated. The heat equivalent is 1,130 times the quantity required to raise the temperature of the liquid by one degree centigrade. This is far more than we can admit as a possible amount of work done in the extension of a liquid film. A smaller amount of work spent on the liquid would convert it into vapour at ordinary atmospheric pressure. The conclusion is unavoidable, that a water-film falls off greatly in its contractile force before it is reduced to a thickness of a twenty-millionth of a millimetre. It is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off in the contractile force as long as there are several molecules in the thickness. It is therefore probable that there are not several molecules in a thickness of a twenty-millionth of a millimetre of water.

Surface tension falls much before the film is reduced to $\frac{1}{2}\times10^{-8}$ cm., and there are probably few molecules in that thickness.

The kinetic theory of gases suggested a hundred years ago by Daniel Bernoulli has, during the last quarter of a century, been worked out by Herapath, Joule, Clausius, and Maxwell, to so great perfection that we now find in it satisfactory explanations of all non-chemical properties of gases. However difficult it may be to even imagine what kind of thing the molecule is, we may regard it as an established truth of science that a gas consists of moving molecules disturbed from rectilinear paths and constant velocities by collisions or mutual influences, so rare that the mean length of nearly rectilinear portions of the path of each molecule is many times greater than the average distance from the centre of each molecule to the centre of the molecule nearest it at any time. If, for a moment, we suppose the molecules to be hard elastic globes all of one size, influencing one another only through actual contact, we have for each molecule simply a zigzag path composed of rectilinear portions, with abrupt changes of direction. On this supposition Clausius proves, by a simple application of the calculus of pro-

Kinetic theory of gases.

Meaning of molecule, free path and collision.

Average length of free path estimated by Clausius.

Kinetic theory of gases. Meaning of molecule, free path and collision.

babilities, that the average length of the free path of a particle from collision to collision bears to the diameter of each globe, the ratio of the whole space in which the globes move, to eight times the sum of the volumes of the globes. It follows that the number of the globes in unit volume is equal to the square of this ratio divided by the volume of a sphere whose radius is equal to that average length of free path. But we cannot believe that the individual molecules of gases in general, or even of any one gas, are hard elastic globes. Any two of the moving particles or molecules must act upon one another somehow, so that when they pass very near one another they shall produce considerable deflexion of the path and change in the velocity of each. This mutual action (called force) is different at different distances, and must vary, according to variations of the distance so as to fulfil some definite law. If the particles were hard elastic globes acting upon one another only by contact, the law of force would be—zero force when the distance from centre to centre exceeds the sum of the radii, and infinite repulsion for any distance less than the sum of the radii. This hypothesis, with its "hard and fast" demarcation between no force and infinite force, seems to require mitigation. Without entering on the theory of vortex atoms at present, I may at least say that soft elastic solids, not necessarily globular, are more promising than infinitely hard elastic globes. And, happily, we are not left merely to our fancy as to what we are to accept as probable in respect to the law of force. If the particles were hard elastic globes the average time from collision to collision would be inversely as the average velocity of the particles. But Maxwell's experiments on the variation of the viscosities of gases with change of temperature prove that the mean time from collision to collision is independent of the velocity if we give the name collision to those mutual actions only which produce something more than a certain specified degree of deflection of the line of motion. This law could be fulfilled by soft elastic particles (globular or not globular); but, as we have seen, not by hard elastic globes. Such details, however, are beyond the scope of our present argument. What we want now are rough approximations to absolute values, whether of time or space or mass—not delicate differential results. From Joule, Maxwell, and Clausius we know that the average velocity of the molecules of

oxygen or nitrogen or common air, at ordinary atmospheric temperature and pressure, is about 50,000 centimetres per second, and the average time from collision to collision a five-thousand-millionth of a second. Hence the average length of path of each molecule between collisions is about $\frac{1}{100000}$ of a centimetre. Now, having left the idea of hard globes, according to which the dimensions of a molecule and the distinction between collision and no collision are perfectly sharp, something of circumlocution must take the place of these simple terms.

Kinetic theory of gases.

Average free path 10^{-5} cm.

Meaning of collision and diameter of molecule.

First, it is to be remarked that two molecules in collision will exercise a mutual repulsion in virtue of which the distance between their centres, after being diminished to a minimum, will begin to increase as the molecules leave one another. This minimum distance would be equal to the sum of the radii, if the molecules were infinitely hard elastic spheres; but in reality we must suppose it to be very different in different collisions. Considering only the case of equal molecules, we might, then, define the radius of a molecule as half the average shortest distance reached in a vast number of collisions. The definition I adopt for the present is not precisely this, but is chosen so as to make as simple as possible the statement I have to make of a combination of the results of Clausius and Maxwell. Having defined the radius of a gaseous molecule, I call the double of the radius the diameter; and the volume of a globe of the same radius or diameter I call the volume of the molecule.

Free path cannot be more than 5000 times diameter of molecule;

The experiments of Cagniard de la Tour, Faraday, Regnault, and Andrews, on the condensation of gases do not allow us to believe that any of the ordinary gases could be made forty thousand times denser than at ordinary atmosphere pressure and temperature, without reducing the whole volume to something less than the sum of the volume of the gaseous molecules, as now defined. Hence, according to the grand theorem of Clausius quoted above, the average length of path from collision to collision cannot be more than five thousand times the diameter of the gaseous molecule; and the number of molecules in unit of volume cannot exceed 25,000,000 divided by the volume of a globe whose radius is that average length of path. Taking now the preceding estimate, $\frac{1}{100000}$ of a centimetre, for the average length of path from collision to collision we conclude that the and diameter cannot be less than 2×10^{-9} cm.

diameter of the gaseous molecule cannot be less than $\frac{1}{500\,000,000}$ of a centimetre; nor the number of molecules in a cubic centimetre of the gas (at ordinary density) greater than 6×10^{21} (or six thousand million million million).

Average distance from centre to centre of molecules in solids and liquids between 7×10^{-9} and 2×10^{-9} cm.

The densities of known liquids and solids are from five hundred to sixteen thousand times that of atmospheric air at ordinary pressure and temperature; and, therefore, the number of molecules in a cubic centimetre may be from 3×10^{24} to 10^{26} (that is, from three million million million million to a hundred million million million million). From this (if we assume for a moment a cubic arrangement of molecules), the distance from centre to nearest centre in solids and liquids may be estimated at from $\frac{1}{140,000,000}$ to $\frac{1}{460,000,000}$ of a centimetre.

The four lines of argument which I have now indicated, lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they establish with what we cannot but regard as a very high degree of probability the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the hundred-millionth, and greater than the two thousand-millionth of a centimetre*.

Illustration of size of molecules.

To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a rain drop, or a globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be more coarse grained than a heap of small shot, but probably less coarse grained than a heap of cricket-balls.

* I find that M. Loschmidt had preceded me in the fourth of the preceding methods of estimating the size of atoms [Sitzungsberichte of the Vienna Acad., 12 Oct., 1865, p. 395]. He finds the diameter of a molecule of common air to be about a ten-millionth of a centimetre. M. Lippmann has also given a remarkably interesting and original investigation relating to the size of atoms *Comptes Rendus*, Oct. 16th, 1882, basing his argument on the variations of capillarity under electrification. He finds that the thickness of the double electric layer, according to Helmholtz's theory, is about a 35-millionth of a centimetre. W. T., Dec. 13, 1882.

(G.)—On Tidal Friction, by G. H. Darwin, F.R.S.

(*a.*) *The retardation of the earth's rotation, as deduced from the secular acceleration of the Moon's mean motion.*

Retardation of earth's rotation. Numerical estimates.

In my paper on the precession of a viscous spheroid [*Phil. Trans.* Pt. II., 1879, or *Scientific Papers*, Vol. II. p. 36], all the data are given which are requisite for making the calculations for Professor Adams' result in § 830, viz.: that if there is an unexplained part in the coefficient of the secular acceleration of the moon's mean motion amounting to 6″, and if this be due to tidal friction, then in a century the earth gets 22 seconds behind time, when compared with an ideal clock, going perfectly for a century, and perfectly rated at the beginning of the century. In the paper referred to however the earth is treated as homogeneous, and the tides are supposed to consist in a bodily deformation of the mass. The numerical results there given require some modification on this account.

If E, E', E'' be the heights of the semidiurnal, diurnal and fortnightly tides, expressed as fractions of the equilibrium tides of the same denominations; and if ϵ, ϵ', ϵ'' be the corresponding retardations of phase of these tides due to friction; it is shown on p. 476 [*Scientific Papers*, Vol. II. p. 68] and in equation (48), that in consequence of lunar and solar tides, at the end of a century, the earth, as a time-keeper, is behind the time indicated by the ideal perfect clock.

$$1900{\cdot}27\ E\sin 2\epsilon + 423{\cdot}49\ E'\sin\epsilon' \text{ seconds of time} \quad\ldots\ldots(a),$$

and that if the motion of the moon were unaffected by the tides, an observer, taking the earth as his clock, would note that at the end of the century the moon was in advance of her place in her orbit by

$$1043''{\cdot}28\ E\sin 2\epsilon + 232''{\cdot}50\ E'\sin\epsilon' \quad\ldots\ldots(b).$$

This is of course merely the expression of the same fact as (*a*), in a different form.

Lastly it is shown in equation (60) that from these causes in a century, the moon actually lags behind her place

$$630''{\cdot}7\ E\sin 2\epsilon + 108''{\cdot}6\ E'\sin\epsilon' - 7''{\cdot}042\ E''\sin 2\epsilon'' \quad\ldots\ldots(c).$$

In adapting these results to the hypothesis of oceanic tides on a heterogeneous earth, we observe in the first place that, if the

Retardation of earth's rotation.

fluid tides are inverted, that is to say if for example it is low water under the moon, then friction advances the fluid tides*, and therefore in that case the ϵ's are to be interpreted as advancements of phase; and secondly that the E's are to be multiplied by $\frac{2}{11}$, which is the ratio of the density of water to the mean density of the earth. Next the earth's moment of inertia (as we learn from col. vii. of the table in § 824) is about ·83 of its amount on the hypothesis of homogeneity, and therefore the results (a) and (b) have both to be multiplied by 1/·83 or 1·2; the result (c) remains unaffected except as to the factor $\frac{2}{11}$.

Thus subtracting (c) from (b) as amended, we find that to an observer, taking the earth as a true time-keeper, the moon is, at the end of the century, in advance of her place by

$$\tfrac{2}{11}\{(1{\cdot}2\times 1043''{\cdot}28-630''{\cdot}7)\,E\sin 2\epsilon + (1{\cdot}2\times 232''{\cdot}50-108''{\cdot}6)\,E'\sin\epsilon'+7''{\cdot}042\,E''\sin 2\epsilon''\},$$

which is equal to

$$\tfrac{2}{11}\{621''{\cdot}24\,E\sin 2\epsilon+170''{\cdot}40\,E'\sin\epsilon'+7''{\cdot}04\,E''\sin 2\epsilon''\}\ldots(d)$$

and from (a) as amended that the earth, as a time-keeper, is behind the time indicated by the ideal clock, perfectly rated at the beginning of the century, by

$$\tfrac{2}{11}\{2280{\cdot}32\,E\sin 2\epsilon+508{\cdot}19\,E'\sin\epsilon'\}\text{ seconds of time }\ldots\ldots\ldots(e).$$

Now if we suppose that the tides have their equilibrium height, so that the E's are each unity; and that ϵ' is one half of ϵ (which must roughly correspond to the state of the case), and that ϵ'' is insensible, and ϵ small, (d) becomes

$$\tfrac{4}{11}\{621''{\cdot}24+\tfrac{1}{4}\times 170''{\cdot}40\}\,\epsilon\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(f)$$

and (e) becomes

$$\tfrac{4}{11}\{2280{\cdot}32+\tfrac{1}{4}\times 508{\cdot}19\}\,\epsilon\text{ seconds of time }\ldots\ldots\ldots\ldots(g).$$

If (f) were equal to 1″, then (g) would clearly be

$$\frac{2280{\cdot}32+\frac{1}{4}\times 508{\cdot}19}{621{\cdot}24+\frac{1}{4}\times 170{\cdot}40}\text{ seconds of time }\ldots\ldots\ldots\ldots\ldots\ldots(h).$$

The second term, both in the numerator and denominator of (h), depends on the diurnal tide, which only exists when the ecliptic

* That this is true may be seen from considerations of energy. If it were approximately low water under the moon, the earth's rotation would be accelerated by tidal friction, if the tides of short period lagged; and this would violate the principles of energy.

is oblique. Now Adams' result was obtained on the hypothesis that the obliquity of the ecliptic was nil, therefore according to his assumption, 1″ in the coefficient of lunar acceleration means that the earth, as compared with a perfect clock rated at the beginning of the century, is behind time

Adams' result.

$$\frac{2280{\cdot}32}{621{\cdot}24}=3\tfrac{2}{3}\text{ seconds at the end of a century.}$$

Accordingly 6″ in the coefficient gives 22 secs. at the end of a century, which is his result given in § 830. If however we include the obliquity of the ecliptic and the diurnal tide, we find that 1″ in the coefficient means that the earth, as compared with the perfect clock, is behind time

$$\frac{2407{\cdot}37}{663{\cdot}80}=3{\cdot}6274\text{ seconds at the end of a century.}$$

Thus taking Hansen's 12″·56 with Delaunay's 6″·1, we have the earth behind 6·46 × 3·6274 = 23·4 sec., and taking Newcomb's 8″·4 with Delaunay's 6″·1, we have the earth behind 2·3 × 3·6274 = 8·3 sec.

Other results.

It is worthy of notice that this result would be only very slightly vitiated by the incorrectness of the hypothesis made above as to the values of the E's and ϵ's; for $E\sin 2\epsilon$ occurs in the important term both in the numerator and denominator of the result for the earth's defect as a time-keeper, and thus the hypothesis only enters in determining the part played by the diurnal tide. Hence the result is not sensibly affected by some inexactness in this hypothesis, nor by the fact that the oceans in reality only cover a portion of the earth's surface.

(b.) *The Determination of the Secular Effects of Tidal Friction by a Graphical Method.* (Portion of a paper published in the *Proc. Roy. Soc.* No. 197, 1879 [or *Scientific Papers*, Vol. II. p. 195], but with alterations and additions.)

General problem of tidal friction.

Suppose an attractive particle or satellite of mass m to be moving in a circular orbit, with an angular velocity Ω, round a planet of mass M, and suppose the planet to be rotating about an axis perpendicular to the plane of the orbit, with an angular velocity n; suppose, also, the mass of the planet to be partially or wholly imperfectly elastic or viscous, or that there are oceans on the surface of the planet; then the attraction of the satellite must produce a relative motion in the parts of the planet, and that motion must be subject to friction, or, in other words, there must be frictional tides of some sort or other. The system must accordingly be losing energy by friction, and its configuration must change in such a way that its whole energy diminishes.

Such a system does not differ much from those of actual planets and satellites, and, therefore, the results deduced in this hypothetical case must agree pretty closely with the actual course of evolution, provided that time enough has been and will be given for such changes.

Let C be the moment of inertia of the planet about its axis of rotation;

r the distance of the satellite from the centre of the planet;

h the resultant moment of momentum of the whole system;

e the whole energy, both kinetic and potential of the system.

It will be supposed that the figure of the planet and the distribution of its internal density are such that the attraction of the satellite causes no couple about any axis perpendicular to that of rotation.

Special units. I shall now adopt a special system of units of mass, length, and time such that the analytical results are reduced to their simplest forms.

Let the unit of mass be $Mm/(M+m)$.

Let the unit of length γ be such a distance, that the moment of inertia of the planet about its axis of rotation may be equal to the moment of inertia of the planet and satellite, treated as particles, about their centre of inertia, when distant γ apart from one another. This condition gives

$$M\left(\frac{m\gamma}{M+m}\right)^2 + m\left(\frac{M\gamma}{M+m}\right)^2 = C$$

whence
$$\gamma = \left\{\frac{C(M+m)}{Mm}\right\}^{\frac{1}{2}}.$$

Let the unit of time τ be the time in which the satellite revolves through $57^\circ{\cdot}3$ about the planet, when the satellite's radius vector is equal to γ. In this case $1/\tau$ is the satellite's orbital angular

velocity, and by the law of periodic times we have

$$\tau^{-2}\gamma^3 = \mu(M+m)$$

where μ is the attraction between unit masses at unit distance. Then by substitution for γ

$$\tau = \left\{\frac{C^3(M+m)}{\mu^2(Mm)^3}\right\}^{\frac{1}{2}}.$$

This system of units will be found to make the three following functions each equal to unity, viz. $\mu^{\frac{1}{2}}Mm(M+m)^{-\frac{1}{2}}$, μMm, and C. The units are in fact derived from the consideration that these functions are each to be unity.

Numerical values of the units for earth and moon. In the case of the earth and moon, if we take the moon's mass as $\frac{1}{82}$nd of the earth's, and the earth's moment of inertia as $\frac{1}{3}Ma^2$ [see § 824], it may easily be shown that the unit of mass is $\frac{1}{83}$ of the earth's mass, the unit of length is 5·26 earth's radii or 33,506 kilometres, and the unit of time is 2 hrs. 41 minutes. In these units the present angular velocity of the earth's diurnal rotation is expressed by ·7044, and the moon's present radius vector by 11·454.

Moment of momentum and energy of system. The two bodies being supposed to revolve in circles about their common centre of inertia with an angular velocity Ω, the moment of momentum of orbital motion is

$$M\left(\frac{mr}{M+m}\right)^2\Omega + m\left(\frac{Mr}{M+m}\right)^2\Omega = \frac{Mm}{M+m}r^2\Omega.$$

Then, by the law of periodic times, in a circular orbit,

$$\Omega^2 r^3 = \mu(M+m)$$

whence $$\Omega r^2 = \mu^{\frac{1}{2}}(M+m)^{\frac{1}{2}}r^{\frac{1}{2}}.$$

And the moment of momentum of orbital motion

$$= \mu^{\frac{1}{2}}Mm(M+m)^{-\frac{1}{2}}r^{\frac{1}{2}},$$

and in the special units this is equal to $r^{\frac{1}{2}}$.

The moment of momentum of the planet's rotation is Cn, and $C=1$, in the special units.

Therefore $$h = n + r^{\frac{1}{2}} \quad\text{.........................(1).}$$

Again, the kinetic energy of orbital motion is

$$\tfrac{1}{2}M\left(\frac{mr}{M+m}\right)^2\Omega^2 + \tfrac{1}{2}m\left(\frac{Mr}{M+m}\right)^2\Omega^2 = \tfrac{1}{2}\frac{Mm}{M+m}r^2\Omega^2 = \tfrac{1}{2}\frac{\mu Mm}{r}.$$

Moment of momentum and energy of system.

The kinetic energy of the planet's rotation is $\frac{1}{2}Cn^2$.

The potential energy of the system is $-\mu Mm/r$.

Adding the three energies together, and transforming into the special units, we have

$$2e = n^2 - \frac{1}{r} \quad \text{.........................(2).}$$

Since the moon's present radius vector is 11·454, it follows that the orbital momentum of the moon is 3·384. Adding to this the rotational momentum of the earth which is ·704, we obtain 4·088 for the total moment of momentum of the moon and earth. The ratio of the orbital to the rotational momentum is 4·80, so that the total moment of momentum of the system would, but for the obliquity of the ecliptic, be 5·80 times that of the earth's rotation. In § 276, where the obliquity is taken into consideration, the number is given as 5·38.

Now let $x = r^{\frac{1}{2}}$, $y = n$, $Y = 2e$.

It will be noticed that x, the moment of momentum of orbital motion, is equal to the square root of the satellite's distance from the planet.

Then the equations (1) and (2) become

$$h = y + x \quad \text{.................................(3).}$$

$$Y = y^2 - \frac{1}{x^2} = (h-x)^2 - \frac{1}{x^2} \quad \text{..................(4).}$$

(3) is the equation of conservation of moment of momentum, or shortly, the equation of momentum; (4) is the equation of energy.

Two configurations of maximum and minimum energy for given momentum, determined by quartic equation.

Now, consider a system started with given positive (or say clockwise*) moment of momentum h; we have all sorts of ways in which it may be started. If the two rotations be of opposite kinds, it is clear that we may start the system with any amount of energy however great, but the true maxima and minima of energy compatible with the given moment of momentum are given by $dY/dx = 0$,

or $$x - h + \frac{1}{x^3} = 0,$$

that is to say, $$x^4 - hx^3 + 1 = 0 \quad \text{........................(5).}$$

* This is contrary to the ordinary convention, but I leave this passage as it stood originally.

We shall presently see that this quartic has either two real roots and two imaginary, or all imaginary roots*.

This quartic may be derived from quite a different consideration, viz., by finding the condition under which the satellite may move round the planet, so that the planet shall always show the same face to the satellite, in fact, so that they move as parts of one rigid body.

In these configurations the satellite moves as though rigidly connected with the planet.

The condition is simply that the satellite's orbital angular velocity $\Omega = n$ the planet's angular velocity of rotation; or since $n = y$ and $r^{\frac{1}{2}} = \Omega^{-\frac{1}{3}} = x$, therefore $y = 1/x^3$.

By substituting this value of y in the equation of momentum (3), we get as before

$$x^4 - hx^3 + 1 = 0 \quad \text{.........................(5).}$$

In my paper on the "Precession of a Viscous Spheroid†," I obtained the quartic equation from this last point of view only, and considered analytically and numerically its bearings on the history of the earth.

Sir William Thomson, having read the paper, told me that he thought that much light might be thrown on the general physical meaning of the equation, by a comparison of the equation of conservation of moment of momentum with the energy of the system for various configurations, and he suggested the appropriateness of geometrical illustration for the purpose of this comparison. The method which is worked out below is the result of the suggestions given me by him in conversation.

The simplicity with which complicated mechanical interactions may be thus traced out geometrically to their results appears truly remarkable.

At present we have only obtained one result, viz.: that if with given moment of momentum it is possible to set the satellite and planet moving as a rigid body, then it is possible to do so in two ways, and one of these ways requires a maximum amount of energy and the other a minimum; from which it is clear that one must be a rapid rotation with the satellite near the planet, and the other a slow one with the satellite remote from the planet.

* I have elsewhere shown that when it has real roots, one is greater and the other less than $\frac{3}{4}h$. *Proc. Roy. Soc.* No. 202, 1880, [or *Scientific Papers*, Vol. II. p. 390. G. H. D.]

† *Trans. Roy. Soc.* Part I. 1879, [or *Scientific Papers*, Vol. II. p. 36. G. H. D.]

In these configurations the satellite moves as though rigidly connected with the planet.

Now, consider the three equations,

$$h = y + x \quad \text{.............................(6)},$$

$$Y = (h - x)^2 - \frac{1}{x^2} \quad \text{.............................(7)},$$

$$x^3 y = 1 \quad \text{.............................(8)}.$$

(6) is the equation of momentum; (7) that of energy; and (8) we may call the equation of rigidity, since it indicates that the two bodies move as though parts of one rigid body.

Graphical solution.

Now, if we wish to illustrate these equations geometrically, we may take as abscissa x, which is the moment of momentum of orbital motion; so that the axis of x may be called the axis of orbital momentum. Also, for equations (6) and (8) we may take as ordinate y, which is the moment of momentum of the planet's rotation; so that the axis of y may be called the axis of rotational momentum. For (7) we may take as ordinate Y, which is twice the energy of the system; so that the axis of Y may be called the axis of energy. Then, as it will be convenient to exhibit all three curves in the same figure, with a parallel axis of x, we must have the axis of energy identical with that of rotational momentum.

It will not be necessary to consider the case where the resultant moment of momentum h is negative, because this would only be equivalent to reversing all the rotations; thus h is to be taken as essentially positive.

Then the line of momentum, whose equation is (6), is a straight line inclined at 45° to either axis, having positive intercepts on both axes.

The curve of rigidity, whose equation is (8), is clearly of the same nature as a rectangular hyperbola, but having a much more rapid rate of approach to the axis of orbital momentum than to that of rotational momentum.

The intersections (if any) of the curve of rigidity with the line of momentum have abscissæ which are the two roots of the quartic $x^4 - hx^3 + 1 = 0$. The quartic has, therefore, two real roots or all imaginary roots. Then, since $x = \sqrt{r}$, the intersection which is more remote from the origin, indicates a configuration where the satellite is remote from the planet; the other gives the configuration where the satellite is closer

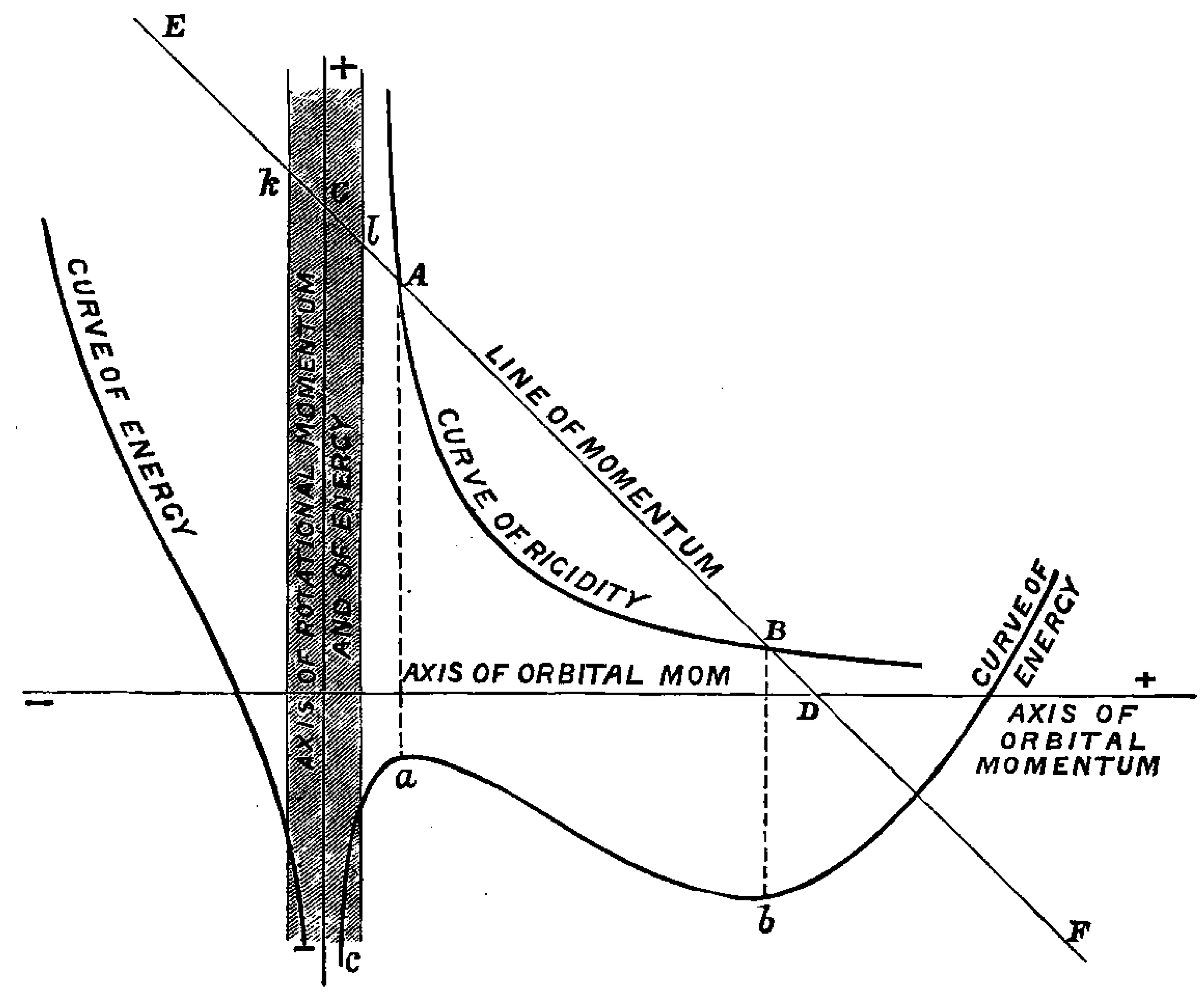
Graphical solution.

to the planet. We have already learnt that these two correspond respectively to minimum and maximum energy.

When x is very large, the equation to the curve of energy is $Y = (h - x)^2$, which is the equation to a parabola, with a vertical axis parallel to Y and distant h from the origin, so that the axis of the parabola passes through the intersection of the line of momentum with the axis of orbital momentum.

When x is very small the equation becomes $Y = -1/x^2$.

Fig. 1.



Hence, the axis of Y is asymptotic on both sides to the curve of energy.

Then, if the line of momentum intersects the curve of rigidity, the curve of energy has a maximum vertically underneath the point of intersection nearer the origin, and a minimum underneath the point more remote. But if there are no intersections, it has no maximum or minimum.

It is not easy to exhibit these curves well if they are drawn to scale, without making a figure larger than it would be

Graphical solution.

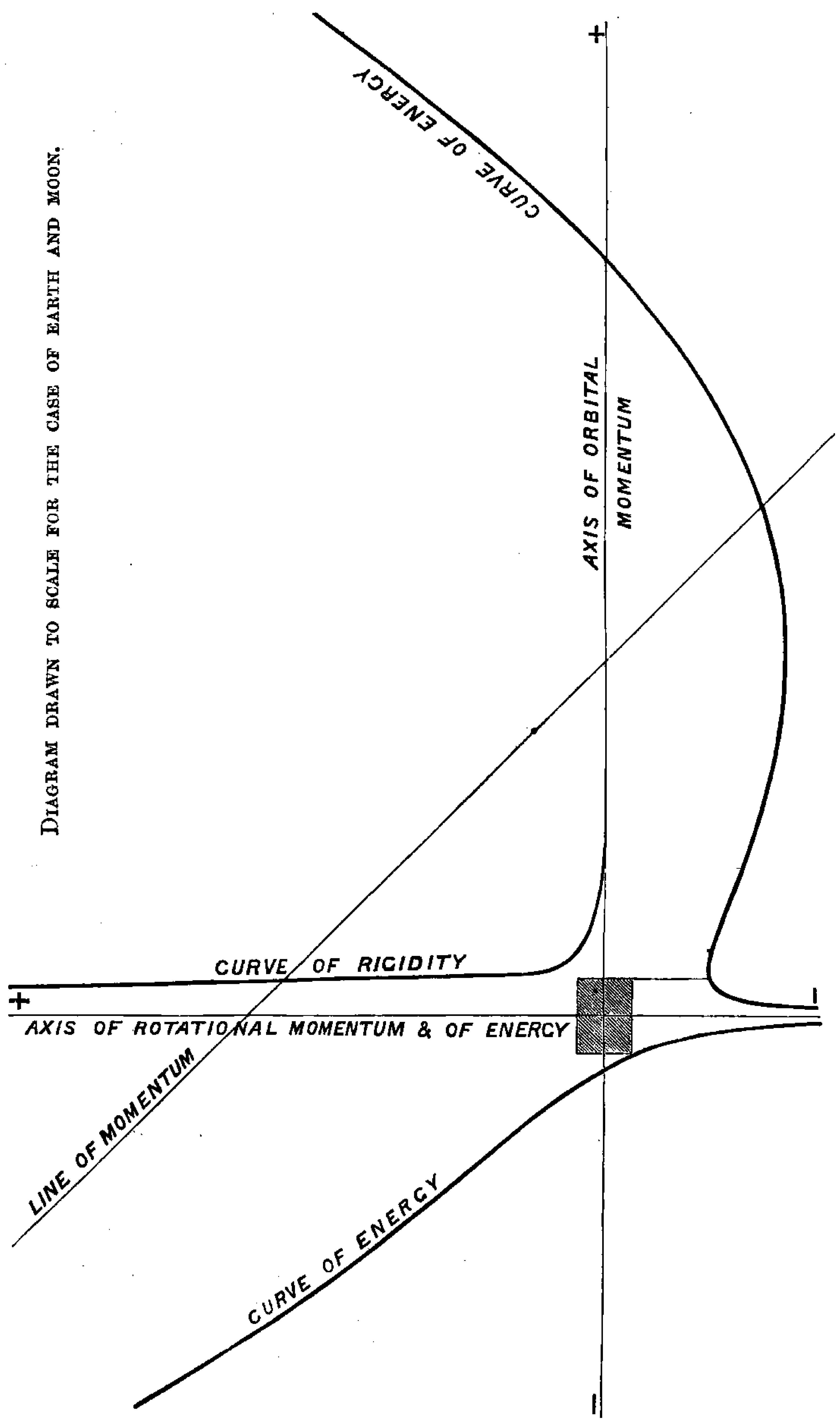


Fig. 2.

convenient to print, and accordingly fig. 1 gives them as drawn with the free hand. As the zero of energy is quite arbitrary, the origin for the energy curve is displaced downwards, and this prevents the two curves from crossing one another in a confusing manner. The same remark applies also to figs. 2 and 3.

Graphical solution.

Fig. 1 is erroneous principally in that the curve of rigidity ought to approach its horizontal asymptote much more rapidly, so that it would be difficult in a drawing to scale to distinguish the points of intersection B and D.

Fig. 2 exhibits the same curves, but drawn to scale, and designed to be applicable to the case of the earth and moon, that is to say, when $h = 4$ nearly.

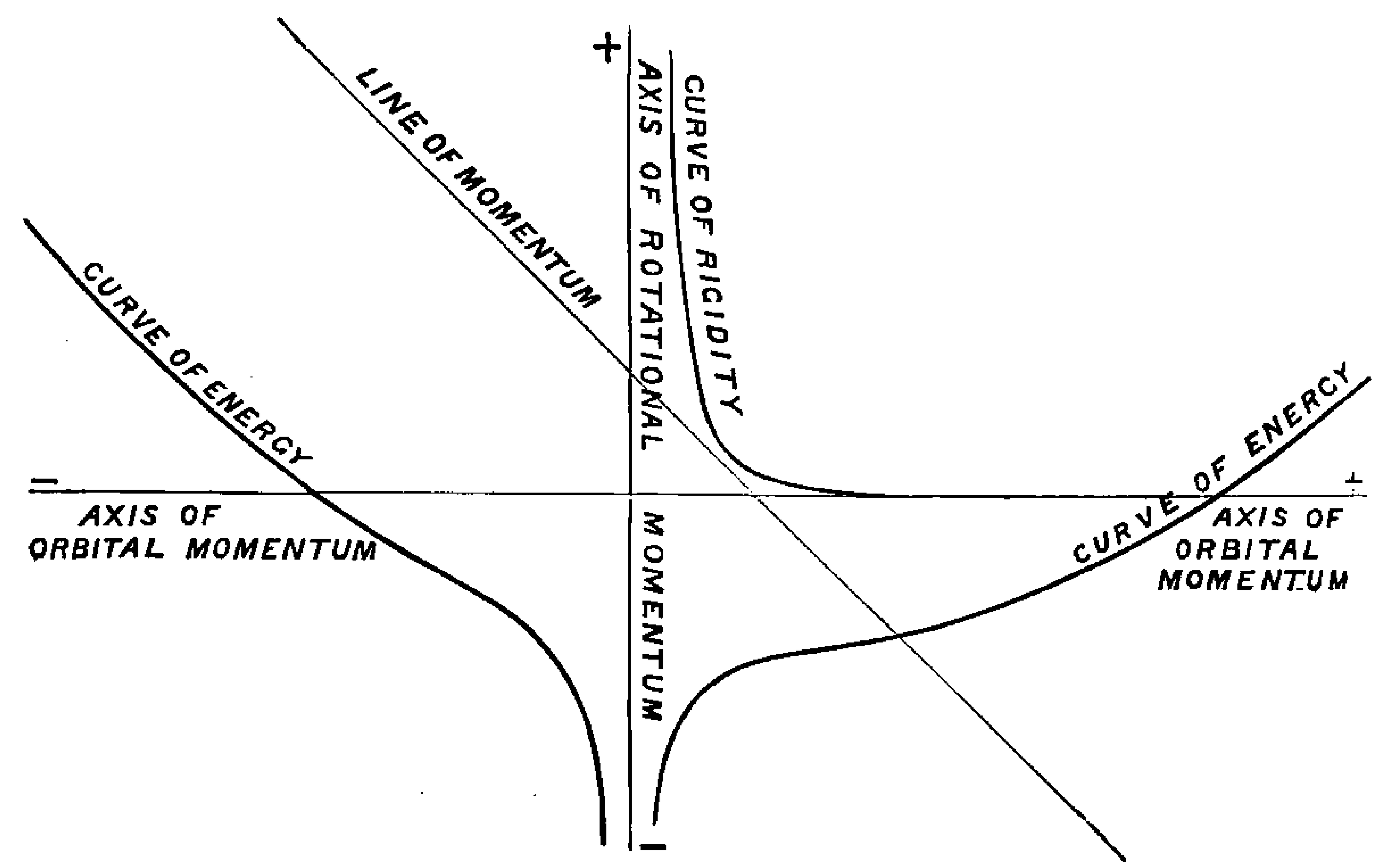


Fig. 3.

Fig. 3 shows the curves when $h = 1$, and when the line of momentum does not intersect the curve of rigidity; and here there is no maximum or minimum in the curve of energy.

These figures exhibit all the possible methods in which the bodies may move with given moment of momentum, and they differ in the fact that in figs. 1 and 2 the quartic (5) has real roots, but in the case of fig. 3 this is not so. Every point of the line of momentum gives by its abscissa and ordinate the square root of the satellite's distance and the rotation of

Graphical solution.

the planet, and the ordinate of the energy curve gives the energy corresponding to each distance of the satellite.

Parts of these figures have no physical meaning, for it is impossible for the satellite to move round the planet at a distance which is less than the sum of the radii of the planet and satellite. Accordingly in fig. 1 a strip is marked off and shaded on each side of the vertical axis, within which the figure has no physical meaning.

Since the moon's diameter is about 2,200 miles, and the earth's about 8,000, therefore the moon's distance cannot be less than 5,100 miles; and in fig. 2, which is intended to apply to the earth and moon and is drawn to scale, the base of the strip is only shaded, so as not to render the figure confused.

The point P in fig. 2 indicates the present configuration of the earth and moon.

The curve of rigidity $x^3y = 1$ is the same for all values of h, and by moving the line of momentum parallel to itself nearer or further from the origin, we may represent all possible moments of momentum of the whole system.

Critical value of moment of momentum.

The smallest amount of moment of momentum with which it is possible to set the system moving as a rigid body, with centrifugal force enough to balance the mutual attraction, is when the line of momentum touches the curve of rigidity. The condition for this is clearly that the equation $x^4 - hx^3 + 1 = 0$ should have equal roots. If it has equal roots, each root must be $\frac{3}{4}h$, and therefore

$$(\tfrac{3}{4}h)^4 - h(\tfrac{3}{4}h)^3 + 1 = 0,$$

whence $h^4 = 4^4/3^3$ or $h = 4/3^{\frac{3}{4}} = 1{\cdot}75$.

The actual value of h for the moon and earth is about 4, and hence if the moon-earth system were started with less than $\frac{7}{16}$ of its actual moment of momentum, it would not be possible for the two bodies to move so that the earth should always show the same face to the moon.

Again if we travel along the line of momentum there must be some point for which yx^3 is a maximum, and since $yx^3 = n/\Omega$ there must be some point for which the number of planetary rotations is greatest during one revolution of the satellite, or shortly there must be some configuration for which there is a maximum number of days in the month.

Now yx^3 is equal to $x^3(h-x)$, and this is a maximum when $x = \frac{3}{4}h$ and the maximum number of days in the month is $(\frac{3}{4}h)^3 (h - \frac{3}{4}h)$ or $3^3h^4/4^4$; if h is equal to 4, as is nearly the case for the earth and moon, this becomes 27.

Maximum number of days in the month.

Hence it follows that we now have very nearly the maximum number of days in the month. A more accurate investigation in my paper on the "Precession of a Viscous Spheroid," showed that taking account of solar tidal friction and of the obliquity to the ecliptic the maximum number of days is about 29, and that we have already passed through the phase of maximum.

We will now consider the physical meaning of the several parts of the figures.

It will be supposed that the resultant moment of momentum of the whole system corresponds to a clockwise rotation.

Now imagine two points with the same abscissa, one on the momentum line and the other on the energy curve, and suppose the one on the energy curve to guide that on the momentum line.

Then since we are supposing frictional tides to be raised on the planet, therefore the energy must degrade, and however the two points are set initially, the point on the energy curve must always slide down a slope carrying with it the other point.

Now looking at fig. 1 or 2, we see that there are four slopes in the energy curve, two running down to the planet, and two others which run down to the minimum. In fig. 3 on the other hand there are only two slopes, both of which run down to the planet.

Various modes of degradation according to initial circumstances.

In the first case there are four ways in which the system may degrade, according to the way it was started; in the second only two ways.

i. Then in fig. 1, for all points of the line of momentum from C through E to infinity, x is negative and y is positive; therefore this indicates an anti-clockwise revolution of the satellite, and a clockwise rotation of the planet, but the moment of momentum of planetary rotation is greater than that of the orbital motion. The corresponding part of the curve of energy slopes uniformly down, hence however the system be started, for this part of the line of momentum, the satellite must approach the planet, and will fall into it when its distance is given by the point k.

Various modes of degradation according to initial circumstances.

ii. For all points of the line of momentum from D through F to infinity, x is positive and y is negative; therefore the motion of the satellite is clockwise, and that of the planetary rotation anti-clockwise, but the moment of momentum of the orbital motion is greater than that of the planetary rotation. The corresponding part of the energy curve slopes down to the minimum b. Hence the satellite must approach the planet until it reaches a certain distance where the two will move round as a rigid body. It will be noticed that as the system passes through the configuration corresponding to D, the planetary rotation is zero, and from D to B the rotation of the planet becomes clockwise.

If the total moment of momentum had been as shown in fig. 3, then the satellite would have fallen into the planet, because the energy curve would have no minimum.

From i and ii we learn that if the planet and satellite are set in motion with opposite rotations, the satellite will fall into the planet, if the moment of momentum of orbital motion be less than or equal to or only greater by a certain critical amount (viz. $4/3^{\frac{3}{4}}$, in our special units), than the moment of momentum of planetary rotation, but if it be greater by more than a certain critical amount the satellite will approach the planet, the rotation of the planet will stop and reverse, and finally the system will come to equilibrium when the two bodies move round as a rigid body, with a long periodic time.

Case of Martian Satellite.

iii. We now come to the part of the figure between C and D. For the parts AC and BD of the line AB in fig. 1, the planetary rotation is slower than that of the satellite's revolution, or the month is shorter than day, as in one of the satellites of Mars. In fig. 3 these parts together embrace the whole. In all cases the satellite approaches the planet. In the case of fig. 3, the satellite must ultimately fall into the planet; in the case of figs. 1 and 2 the satellite will fall in if its distance from the planet is small, or move round along with the planet as a rigid body if its distance be large.

General case for satellites of solar system.

For the part of the line of momentum AB, the month is longer than the day, and this is the case of all known satellites except the nearer one of Mars. As this part of the line is non-existent in fig. 3, we see that the case of all existing satellites (except the Martian one) is comprised within this part of figs. 1 and 2. Now if a satellite be placed in the condition A, that is to say, moving rapidly round a planet, which always shows the same face to the satellite, the condition is clearly dynamically unstable, for the least disturbance will determine whether the system shall degrade down the slopes ac or ab, that is to say, whether it falls into or recedes from the planet. If the equilibrium breaks down by the satellite receding, the recession will go on until the system has reached the state corresponding to B.

Compare § 778″ (*g*).

The point P, in fig. 2, shows approximately the present state of the earth and moon, viz., when $x = 3{\cdot}2$, $y = {\cdot}8$*.

Suggested origin of the moon.

It is clear that, if the point l, which indicates that the satellite is just touching the planet, be identical with the point A, then the two bodies are in effect parts of a single body in an unstable configuration. If, therefore, the moon was originally part of the earth, we should expect to find A and l identical. The figure 2, which is drawn to represent the earth and moon, shows that there is so close an approach between the edge of the shaded band and the intersection of the line of momentum and curve of rigidity, that it would be scarcely possible to distinguish them on the figure. Hence, there seems a considerable probability that the two bodies once formed parts of a single one, which broke up in consequence of some kind of instability. This view is confirmed by the more detailed consideration of the case in the paper on the "Precession of a Viscous Spheroid," and subsequent papers, which have appeared in the Philosophical Transactions of the Royal Society.

Compare § 778″ (*i*).

Double-star system.

The remainder of the paper, of which this Appendix forms a part, is occupied with a similar graphical treatment of the problem involved in the case of a planet and satellite or a system of two stars, each raising frictional tides in the other, and revolving round one another orbitally. This problem involves the construction of a surface of energy.

* The proper values for the present configuration of the earth and moon are $x = 3{\cdot}4$, $y = {\cdot}7$. Figure (2) was drawn for the paper as originally presented to the Royal Society, and is now merely reproduced.

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The arabic numbers refer to sections. The numbers I. *and* II. *preceding them indicate the volume (parts of the original first volume) in which they will be found. The capital letters refer to the Appendixes. In the case of the Appendixes* A_0, A, B, *references are given to the pages of* I.; *the other Appendixes are at the ends of their respective volumes.*

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