



# XXXIII. Some applications of physics and mathematics to geology

C. Chree M.A.

**To cite this article:** C. Chree M.A. (1891) XXXIII. Some applications of physics and mathematics to geology , Philosophical Magazine Series 5, 32:196, 233-252, DOI: [10.1080/14786449108620181](https://doi.org/10.1080/14786449108620181)

**To link to this article:** <http://dx.doi.org/10.1080/14786449108620181>



Published online: 08 May 2009.



Submit your article to this journal [↗](#)



View related articles [↗](#)

THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

---

[FIFTH SERIES.]

---

SEPTEMBER 1891.

---

XXXIII. *Some Applications of Physics and Mathematics to Geology.* By C. CHREE, M.A., Fellow of King's College, Cambridge\*.

PART I. *Some Physical and Mathematical Data.*

MANY of the terms employed in treating of the properties and conditions of matter have in common use a somewhat vague meaning. The meaning, so far as clearly outlined, is also only too often different from that which the physicist intends to convey. As regards terms such as *rigid*, *solid*, *plastic*, *viscous*, &c. it seems to me that even eminent geologists are apt to be misled by the popular usage, so that they fall into error respecting the data which mathematical and physical science places at their disposal. It thus seems advisable on the present occasion to clear the ground by briefly considering the sense attached to these terms by the more exact school of physicists. To render the following statements intelligible it is necessary to explain the meaning scientifically attached to the terms *stress* and *strain*. By stress is meant a force referred to unit of area of the surface across which it acts, by strain the increase in the distance between two material points divided by the original distance. For instance if a vertical bar  $n$  square inches in cross section fixed at the upper end, sustain a load of  $t$  tons, and the load be uniformly distributed over the cross section, the longitudinal stress is  $tn$ , taking the square inch as unit of area

\* Communicated by the Author.

*Phil. Mag.* S. 5. Vol. 32. No. 196. Sept. 1891. R

and the weight of one ton as unit of force. If a portion of the bar increase in length from 100 to 100·01 inches, and the increase be uniformly distributed over the portion lengthened, the longitudinal strain is  $(100·01 - 100) \times 10^{-2}$ , or ·0001.

The writers who have had most influence on the present scientific usage of English terms dealing with physical properties are unquestionably Professor Clerk Maxwell and Sir William Thomson. The former gives the following definitions in his 'Theory of Heat'\*:—"A body which when subjected to a stress experiences no strain would, if it existed, be called a Perfectly Rigid Body. There are no such bodies. . . ."

"A body which when subjected to a given stress at a given temperature experiences a strain of definite amount, which does not increase when the stress is prolonged, and which disappears completely when the stress is removed, is called a Perfectly Elastic Body."

"If the form of the body is found to be permanently altered when the stress exceeds a certain value, the body is said to be soft or plastic, and the state of the body when alteration is just going to take place is called the Limit of Perfect Elasticity."

"If the stress, when it is maintained constant, causes a strain . . . which increases continually with the time, the substance is said to be viscous."

A viscous material may be either solid or fluid. It is regarded by Maxwell as fluid when any stress, *however small*, produces a constantly increasing strain. Maxwell draws a distinction between elasticity of bulk and elasticity of shape—the latter being peculiar to solids—which is more fully treated of by Sir W. Thomson. A body possesses perfect elasticity of bulk when on the removal of the stress it returns to its original volume, even though the form of its surface be permanently altered. Both writers regard it as certain that solid bodies will retain perfect elasticity of bulk under compressive stresses which far exceed the limit of elasticity of shape. The following statement embodies the views of Sir W. Thomson†:—"If we reckon by the amount of pressure, there is probably no limit to the elasticity of bulk in the direction of the increase of pressure for any solid or fluid; but whether continued augmentation produces continued diminution of bulk towards zero without limit, or whether for any or every solid or fluid there is a limit towards which it

\* 5th edition, chapter xxi.

† Mathematical and Physical Papers, vol. iii. pp. 7-8.

may be reduced in bulk, but smaller than which no degree of pressure, however great, can condense it, is a question which cannot be answered in the present state of science."

Maxwell, by denying the existence of a perfectly rigid body, maintains that every solid can sustain stress or transmit force only by suffering strain. Thus on depositing a feather on the most solid block of iron we produce in the iron a system of strains, infinitesimally small it is true, but whose existence can no more be questioned than the existence across the surface separating the iron and the feather of forces balancing the portion of the feather's weight left uncompensated by the air-pressure. The hypothesis quoted above from Sir W. Thomson, that there may be a limit beyond which no body can be compressed, is not inconsistent with Maxwell's statement. The hypothesis regards the ratio of the increment of strain to the increment of pressure as ultimately becoming infinitesimally small, but it in no way implies that this ratio ever becomes absolutely zero.

In a solid bar, supposed perfectly elastic, exposed to longitudinal stress, the ratio of the stress to the strain is styled Young's Modulus. In many materials Young's modulus varies in magnitude according to the direction in which the axis of the bar is taken. Thus, in ordinary woods, there is a marked difference between the value of Young's modulus in the direction of the pith of the tree and in any perpendicular direction. Materials in which Young's modulus is independent of the direction in which the axis of the experimental bar is taken are termed *isotropic*, all others are termed *æolotropic*.

In an isotropic elastic solid it is supposed, on the ordinary British or *biconstant* theory, that the value of Young's modulus,  $E$ , alone is insufficient to define the elastic structure, and that some other elastic constant must be known. For many purposes the most convenient additional constant is the ratio of the lateral contraction to the longitudinal extension—each measured per unit of length—in a bar exposed to simple longitudinal traction. For instance, if the diameter of a bar under uniform longitudinal stress change from 10 to 9·9997 inches the lateral contraction is ·00003, and if the longitudinal strain be ·0001, the ratio of lateral contraction to longitudinal extension is ·3. This ratio is termed Poisson's Ratio, and is represented here by  $\eta$ .

On the *uniconstant* theory of isotropy  $\eta$  must have the value ·25, which certainly accords well with experiments on glass and some of the more common metals, especially iron and steel under certain conditions.

On the *biconstant* theory  $\eta$  may have any value within

certain limits. The existence of these limits, it must be admitted, is seldom recognized, and experimental results are not infrequently referred to which are inconsistent with the view taken here, viz. that  $\eta$  must lie between 0 and  $\cdot 5$ . If, however,  $\eta$  were negative in any material a circular bar of this material, when subjected to uniform longitudinal tension, would increase in diameter; while if  $\eta$  were greater than  $\cdot 5$ , the bar, when fixed at one end and subjected to a torsional couple at the other, would twist in the opposite direction to the applied force. Until these phenomena are shown to present themselves in isotropic materials—and the experimental verification ought to be easy—it seems legitimate to suppose that when experimentalists deduce values for  $\eta$  which lie outside of these limits, their experiments refer to bodies whose constitution is different from what is assumed in their mathematical calculations.

The properties attributed to an isotropic elastic solid by the ordinary mathematical theory are as follows:—

(A) The strain must be elastic, *i.e.* it must disappear on the removal of the stress.

(B) The ratio of stress to strain must be independent of the magnitude of the stress, or, in Professor Pearson's words, the stress-strain relation must be *linear*.

(C) The strains must be small.

(D) The values of Young's modulus and Poisson's ratio in a bar of the material must be independent of the direction in which the axis of the bar is taken.

The last property alone distinguishes isotropic from æolotropic elastic solids.

(A) answers to Maxwell's definition, but (B) and (C) are not assumed by Maxwell. In other words, a solid may be perfectly elastic without showing a linear stress-strain relation, or possibly even after the strains have become large. Thus, for the sake of clearness, I shall call Maxwell's limit of perfect elasticity the Physical limit, and the limits supplied by (B) and (C) the first and second Mathematical limits respectively.

It is not infrequently taken for granted that the physical and the first mathematical limit are necessarily identical, *i.e.* that the elasticity is certainly not perfect when the stress-strain relation ceases to be linear. According, however, to some experimentalists cast iron is as perfectly elastic as any other metal in the sense of Maxwell's definition, but the stress-strain relation for even small strains is sensibly not linear\*.

\* See Todhunter and Pearson's 'History of Elasticity,' vol. i. art. [1411] and pp. 891-3.

This is of course a question for experimentalists to decide, but in any case where their final verdict is that the stress-strain relation is sensibly not linear the employment of the ordinary mathematical theory is unjustifiable. It must be admitted that the principle (C) is a very vague one, leading to no exact limit, and that it seldom receives any very formal acknowledgment. It is, however, clearly recognized, and a reason for it assigned in the following statement due to Thomson and Tait \* :—"The mathematical theory of elastic solids imposes no restrictions on the magnitudes of the stresses, except in so far as that *mathematical necessity requires the strains to be small enough to admit of the principle of superposition.*" The italics are mine. The meaning is that the strains must be small fractions whose squares are negligible compared to themselves. If this principle be neglected and the mathematical equations be supposed to apply when the strains are large, the difficulty of giving them a consistent physical interpretation is very great if not wholly unsurmountable.

In most materials having any claim to be regarded as elastic solids, the stress-strain relation for most ordinary stress systems certainly ceases to be linear while the strains are still small. We shall thus in the meantime leave the condition (C) out of account, though we shall have to return to it in treating of the so-called "theories of rupture."

The existence of the properties (A), (B), (D), presupposed by the mathematical theory, is determined not solely by the chemical constitution of the body, but also by the treatment to which it has been subjected. Thus a freshly annealed copper wire may, when loaded for the first time, be far from satisfying conditions (A) or (B), and yet by the process of loading and unloading it may be brought into a *state of ease*, wherein these two conditions are very approximately, if not exactly fulfilled, so long as the stress does not exceed a certain limit. Again, the fact that a large mass of metal is sensibly isotropic is no sufficient reason for attributing isotropy to the same metal when rolled into thin plates or drawn into thin wires.

It is quite possible that the three conditions (A), (B), and (D) represent an ideal state which is never actually reached, and that a divergence may always be shown by the use of very delicate apparatus. If this be true, then the results obtained by the mathematical theory cannot claim absolute correctness. It seems, however, to be satisfactorily established

\* Nat. Phil. vol. i. Part ii. p. 422.

that many materials in the state of ease satisfy these conditions with at least a very close approach to exactness, so that the results of the mathematical theory when properly restricted are then sufficiently exact for practical purposes.

From the preceding statements it will be seen that it is of the utmost importance to know what are the limits within which the conditions assumed by the mathematical theory are satisfied with sufficient exactness to justify its application. This question must of course be settled by experiment, but it is beset by various difficulties which ought to be clearly recognized. These arise in part from the serious obstacles in the way of a complete experimental knowledge, and in part from the want of a proper understanding between those interested in the practical and theoretical sides of the subject, and a consequent confusion in the terms used.

To avoid complication let us begin by supposing the mathematical limit of perfect elasticity to coincide with the physical. Let us consider the simple case of a bar under uniform longitudinal traction. We may suppose the bar isotropic, and in consequence of suitable treatment perfectly elastic for loads not exceeding  $L_1$ . No mechanical treatment, we shall suppose, can render it perfectly elastic for loads greater than  $L_2$ . It does not follow that a load  $L_2$  will necessarily rupture the bar either immediately or in course of time, but simply that for any load greater than  $L_2$  the strain is not perfectly elastic. Increasing the load from zero we should reach a load  $L_3$ , probably greater than  $L_2$ , that would in process of time rupture the bar, or a load  $L_4$  greater than  $L_3$  that produces immediate rupture. All these loads are supposed to refer to unit of area.

Now in the initial state of the bar we should be entitled to apply the mathematical theory only until the load  $L_1$  was reached. When we aim at finding the utmost capability of the material under longitudinal load, we may perhaps apply the theory until the load  $L_2$  is reached, but here we must stop. To apply it until the loads  $L_3$  or  $L_4$  are reached—assuming these greater than  $L_2$ —is clearly inadmissible.

Results of a similar kind hold for all the comparatively simple forms of stress,—such as pure compression, torsion, or bending—in which practical men are interested. There are limits to the state of perfect elasticity lower than the limits at which rupture takes place, at least immediately.

The usual aim of the engineer is that no part of the structure he is designing should ever be strained beyond the elastic limit, and this end he of course desires to obtain with the least possible expenditure of material. Thus ideally he

might be expected to calculate the dimensions of each piece, so that for the maximum load it is to be subjected to it shall just not pass beyond the limit of perfect elasticity. There are, however, in general agencies, such as wind pressure, dynamical action of a moving load, &c., whose effects are not very fully understood and whose magnitude cannot always be foreseen. Thus it is the custom to allow a wide margin for contingencies. Now the limit of perfect elasticity seems the natural quantity to employ in allowing for this margin, but the uncertainties attending its determination are such that it is customary to employ the breaking-load instead. The breaking-load for the particular kind of stress the member in question is to be exposed to is divided by some number, *e. g.* 4 or 5, called a *factor of safety*, and the dimensions of the member are calculated so that its estimated load shall not exceed the quotient of the breaking-load by the factor of safety. The engineer varies the factor of safety according to the nature of the load, and according to the confidence he possesses in the uniformity of the material and in the completeness of his knowledge as to the vicissitudes the structure is exposed to. It has thus come to pass that attention has been largely directed to the breaking-loads, and theories have been constructed which aim professedly at supplying a law for the *tendency to rupture*, under the most general stress-systems possible, of materials whose rupture-points have been found under the ordinary simple stress-systems employed in experiment.

There are only two such theories of rupture for isotropic materials that at present possess any general repute. To understand them the reader requires to know that for any stress-system there are at every point in an isotropic elastic material three *principal stresses* along three mutually orthogonal directions, and likewise three *principal strains*, whose directions coincide with those of the principal stresses. If an imaginary small cube of the material be taken at the point considered with its faces perpendicular respectively to the three principal stresses, then no tangential stresses act over these faces. In a bar under a uniformly distributed longitudinal stress  $L$  per unit of cross section, two of the principal stresses are everywhere zero, and the third is parallel to the axis and equals  $L$ . If  $E$  be Young's modulus, and  $\eta$  Poisson's ratio for the material, supposed isotropic and elastic, the greatest principal strain is everywhere  $L/E$  and its direction is parallel to the axis. The two remaining principal strains are each  $-\eta L/E$ , and they may be supposed to have for their directions any two mutually perpendicular lines in the cross section of the bar.



One of the theories referred to above is that when the algebraic difference between the greatest and least of the principal stresses at any point—a pressure being reckoned negative—attains a certain value, rupture will ensue at this point. Thus, if in descending order of magnitude the principal stresses at a point be  $T_1, T_2, T_3$ , then  $T_1 - T_3$  is the *stress-difference*\* at this point, and the theory asserts that rupture will ultimately ensue if the stress-difference anywhere equals  $L_3$ , the load for ultimate rupture of a bar of the material by longitudinal traction; while if the stress-difference anywhere equals  $L_4$ , the load for immediate rupture by longitudinal traction, then immediate rupture will ensue.

The second theory, which is supported by the great authority of de Saint-Venant†, replaces the stress-difference of the first theory by the greatest strain. It thus asserts that the condition for rupture is found by equating the largest value found anywhere for the greatest strain to the longitudinal strain answering to longitudinal traction  $L_3$ , or to that answering to the traction  $L_4$ , according as the rupture is ultimate or immediate. This theory maintains that extension in some direction is necessary for rupture.

The two theories may, as in the case of pure longitudinal traction, lead to the same result; but in general they do not, so one at least of them must be wrong. When we examine the theories, still supposing the mathematical and physical limits of perfect elasticity the same, a very obvious difficulty‡ presents itself. It is assumed that the stress-difference and greatest strain are derived by the mathematical theory; but that theory applies only so long as the material is everywhere perfectly elastic, whereas rupture, at least when immediate, presents itself after the elastic limit has been passed. Thus if the application of the mathematical theory lead to values for the maximum stress-difference and greatest strain equal to the values of these quantities answering to rupture, at all events when immediate, the true conclusion would seem to be that the fundamental hypothesis on which the treatment proceeds, viz. that the material follows the laws assumed by the mathematical theory, has been shown to be incorrect. Nothing has been proved except that the elastic limit must be passed and that the mathematical theory does not apply.

The only logical way of interpreting the theories is to

\* See Professor Darwin, Phil. Trans. 1882, pp. 220-1, &c.; also Thomson and Tait's Nat. Phil. vol. i. Part ii. p. 423.

† See Pearson's 'The Elastical Researches of Barré de Saint-Venant,' Arts. 5 (c), &c.

‡ *Ibid.* Arts. 4 (γ), 5 (a), &c.

suppose that the maximum stress-difference and greatest strain are to be compared not with the values that answer to rupture, but either with those that answer to the limit of perfect elasticity or with those derived by dividing the values answering to rupture by some factor of safety. This factor must then be large enough to prevent the limit of perfect elasticity being passed. Thus from either point of view we encounter a formidable difficulty, viz. the uncertainty of what is the limit of perfect elasticity.

We have supposed that a bar may be brought into a state in which it is perfectly elastic for longitudinal tractions not exceeding  $L_2$ . Answering to this we have  $L_2$  for the stress-difference, and  $L_2/E$  for the greatest strain. Now if the two theories described above really apply to the limit of perfect elasticity, the one would seem to maintain that  $L_2$  is the limiting value of the stress-difference, the other that  $L_2/E$  is the limiting value of the greatest strain for all possible stress-systems in material of the same kind as that in the bar. The complete experimental proof or disproof of such theories is not likely to be easy. Thus taking, for instance, the case of longitudinal traction, suppose it were shown that a certain method of treatment which raises the elastic limit for load parallel to the axis of a bar does not raise the elastic limit for longitudinal load in a bar whose length lay in the cross section of the original bar. This would only suffice to prove that the treatment adopted did not give a fixed elastic limit the same for all kinds of strain, it would leave the possibility of such a limit being obtained in some other way an open question.

In the preceding remarks the mathematical and physical limits of perfect elasticity have been supposed identical. When they differ, the mathematical limit is of course that which must be employed in determining the range of the mathematical theory. It will certainly not exceed the physical limit. I may add that, while for certain structures such as isolated boilers the physical limit may most nearly concern the practical engineer, in other structures, such as girder bridges, the stress-strain relation is assumed to be linear in designing the several parts, so that the first mathematical limit is then of the utmost practical importance.

In the previous discussion of the stress-difference and greatest strain theories, as settling the limits of application of the mathematical theory, it has been taken for granted that the condition (C) was safeguarded by them. Now in most ordinary systems of loading this is probably the case, but it is not always so. For instance, if we assume the mathe-

mathematical theory to hold, a solid isotropic sphere under a uniform surface-pressure shows none but negative strains, and the three principal stresses are everywhere equal. Thus the greatest strain is everywhere negative, and the stress-difference everywhere zero. This is true irrespective of the magnitude of the surface-pressure, and so, according to both theories, the stress-strain relation would be linear and the mathematical theory would apply, however large the pressure was. According to the theories, one might continue to employ mathematical formulæ which indicated a reduction of the sphere to one millionth of its original volume. It is obvious, however, that a reduction of the volume by even a tenth would produce strains which are probably far in excess of those admitted by the principle (C). In formulating an objection to the universal application of the theories, I have preferred to attack them on the side of the principle (C) so as to show clearly that the high authority of Thomson and Tait is on my side. The example considered raises, however, what seems to me at least an equally strong argument against the theories from the side of the principle (B). For we must remember that the stresses inside the material are determined by the intermolecular forces. Now, whatever molecules may be, and however they may act on one another, it seems incredible that the molecular forces should lead to one and the same stress-strain relation, however much the mean molecular distance may be reduced. The fact that Sir W. Thomson regards the existence of an irreducible minimum volume as possible may, I think, be taken as proof that he is opposed to the view that it is possible for the stress-strain relation to remain linear under such circumstances. It thus seems to me, on various grounds, that the inevitable conclusion is that while one or other of the two theories may, under ordinary circumstances, be sufficient to define the limits of the mathematical theory, the result must always be checked by reference to the condition (C), or, what comes to the same thing, we must give up the mathematical theory when the strains it indicates are such as would markedly alter the mean molecular distance.

I next proceed to discuss the possibility of the earth's possessing an elastic solid structure, deriving the necessary data from three papers published in the 'Transactions' of the Cambridge Philosophical Society. For brevity these will be referred to as (a)\*, (b)†, and (c)‡.

\* Vol. xiv. pp. 250-369.

† Vol. xiv. pp. 467-483.

‡ Vol. xv. pp. 1-36.

The strains due to the action of the sun and moon being comparatively insignificant, we need consider only the "centrifugal" forces due to the earth's diurnal rotation, and the gravitational forces due to the mutual attraction of its parts.

The data supplied by Geology do not enable us to formulate any likely theory as to a probable distribution of density and elasticity throughout the earth regarded as an elastic solid. All we know with certainty is that the surface strata are on an average considerably below the mean density, that they differ widely in character, many being markedly æolotropic, and that frequently they are far from horizontal. Thus, as our object is merely to consider what are the possibilities on the hypothesis of solidity, it will be best to make the hypothesis as simple as possible. Now, if the deviations from the earth's mean density and from an isotropic elastic structure were limited to the surface-strata, where alone we are certain of their existence, the effect of the "centrifugal" forces would be nearly the same as if these deviations did not exist; but the effect of the gravitational forces on the eccentricity of the surface may depend largely on the nature of the deviations. I thus propose to treat the problem in stages.

The first stage neglects entirely the gravitational forces and regards the earth as a slightly spheroidal body—which has departed from the spherical form in consequence of its rotation—of uniform density and of the same isotropic elastic structure throughout, rotating with uniform angular velocity  $\omega$  about its polar axis.

Let  $a$  denote the mean radius,  $d$  the difference between the equatorial and polar semi-axes of the surface,  $E$  Young's modulus, and  $\eta$  Poisson's ratio for the material. Then the ratio  $d : a$  is given for various values of  $\eta$  in the following Table\* :—

TABLE I.

$\eta =$	0	.2	.25	.3	.4	.5
$\frac{d}{a} \div \frac{\omega \rho a^2}{E} =$	.286	.330	.341	.352	.373	.395

In the case of an originally spherical solid assuming the shape of the earth under rotation, it is of no practical importance whether we regard  $a$  as the radius of the original spherical surface, or as the mean radius under rotation, nor does it matter practically whether the density be supposed uniform previous to or during the rotation. There is, it is

\* See (a) formula (5) p. 287 ; or (c) Tables III., V., and VI.

true, for all values of  $\eta$  except  $\cdot 5$ , a slight increase in the volume\*, and consequent diminution in the mean density accompanying the rotation, but for our present purpose this may be neglected.

The mathematical solution on which Table I. is based treats the spherical surface of radius  $a$  as that over which the conditions for a free surface are satisfied. Now some uncertainty may exist, depending on the physical interpretation put upon the mathematical equations, whether these surface conditions should be applied over what is the surface before the displacement—in this case the surface of the true sphere which it is assumed the earth would form if the rotation disappeared,—or over what is the surface during the rotation. This uncertainty might constitute a very serious difficulty if the deformations were supposed to be large—a contingency which may arise when the limitation (C) in the magnitude of the strains is neglected; but in such problems as the present where the strains are, as we shall see presently, of the same order of magnitude as occur in ordinary engineering structures, it is of no material consequence. In the present case complete assurance on this point may be derived from figures 1 and 2, plate ii. of (c), which show the changes induced by rotation in the equatorial and polar semi-axes of spheroids of various shapes.

For given values of  $d$ ,  $a$ ,  $\omega$ , and  $\rho$ , Table I. shows that  $E$  and  $\eta$  increase together. Giving  $\omega$  the value it has for the earth, and assuming  $\rho = 5\cdot 5$ ,  $a = 3950$ ,  $d = 13\cdot 25$ , I find for the values of  $E$ , measured in grammes weight per square centim., answering respectively to the values 0,  $\cdot 25$ , and  $\cdot 5$  of  $\eta$ , the approximate numbers

$$1020 \times 10^6, \quad 1220 \times 10^6 \quad \text{and} \quad 1410 \times 10^6.$$

It is obvious from Table I. that to equal increments in  $\eta$  there correspond nearly equal increments in  $E$ ; thus the numbers given above will enable a sufficiently close approximation to the value of  $E$  for any other value of  $\eta$  to be immediately written down.

For the sake of comparison with the values found for  $E$  in some of the commoner materials under ordinary conditions I append the following data, taken from Sir W. Thomson's article on 'Elasticity' in the *Encyclopædia Britannica*. The units are the same as above.

\* See (b) Table II., and compare Tables V. and VI. of (c).

TABLE II.  
Values of  $E/10^6$ .

	Iron and Steel.	Copper.	Slate.	Zinc.	Stone.	Lead.
Highest value .	2953	1254	1120	955	about 350	199
Lowest value...	984	1052	910	873		51

This table will give a general idea of the limits within which  $E$  may reasonably be expected to lie, though some of the data refer to material which is hardly likely to have been isotropic. It shows that if the influence of the gravitational forces on the eccentricity were negligible—which, however, is not the case—the earth, though perfectly solid and elastic, might reasonably be expected to display not a smaller but a considerably greater eccentricity than it actually does.

The question next arises whether the strains and stresses produced by the rotation are such as are consistent with the principles on which the application of the mathematical theory rests. In the actual case of the earth this question is of importance only in exceptional circumstances, owing to the preponderating influence of the gravitational forces, still it possesses sufficient interest to claim separate consideration. The following table gives a sufficiently close approximation to the numerical results obtained for the rotating body treated above, when for  $E$  are substituted the values which answer to the production by rotation alone of an eccentricity equal to that of the earth.

TABLE III.\*

$\eta =$	0	.25	.5
Maximum stress-difference in tons weight per square inch .....	32½	32½	32
Greatest strain .....	.0040	.0029	.0018
Longitudinal stress in tons per square inch which would produce a strain equal to the greatest strain.....	26	23	16

\* See (c) Tables III., VII., and IX.

The maximum stress-difference and the greatest strain, as given in the table, are both found at the centre.

The result on the stress-difference theory is nearly independent of  $\eta$ , and is more unfavourable in every case than that given by the greatest strain theory to the view that the material remains perfectly elastic. A stress of 16 tons per square inch is not one that an engineer would view with complacency in any structure intended to be permanent, but it is a low value for the tenacity of good wrought iron. A stress of even 33 tons per square inch can easily be borne without rupture by good steel, and is perhaps not in excess of the stress under which the best steel remains practically perfectly elastic. The greatest strains are not of such a magnitude as to raise any presumption against the linearity of the stress-strain relation. Thus, according to all the tests, it is quite possible that an originally spherical solid of the earth's mass but devoid of gravitation should remain solid and elastic while assuming the form of the earth under rotation. Its material, however, at least if homogeneous and isotropic, would require to possess an unusually high limit of perfect elasticity.

The next subject for consideration is how the question is affected by the existence of gravitational forces such as are found in the case of the earth. The strains and stresses in a slightly oblate spheroid, treated as an isotropic elastic solid, all consist of two parts, the first part being the same as if the surface were truly spherical, the second depending on the eccentricity. It is the second parts that represent the action of the gravitational forces in modifying the eccentricity, but these parts are in general insignificant so far as the question of the applicability of the mathematical theory is concerned. I shall therefore postpone consideration of them until an account has been given of the strains and stresses which are independent of the eccentricity.

The mathematical difficulties in applying the ordinary theory to the case of a homogeneous solid gravitating sphere are trifling, but the difficulty of putting a physical interpretation upon the mathematical expressions answering to most values of  $\eta$  is such as very forcibly to call attention to the necessity of the limitation (C). Since the gravitational force at an element of a solid sphere depends not only on the total mass which lies nearer the centre than does the element, but also on its absolute distance from the centre, we must assume that the equations supplied by the ordinary mathematical theory, if they apply at all, hold for the position of final equilibrium after the deformations have taken place. This

seemingly requires that strain should be defined as the ratio of the increase of length to the final length, which is not in accordance with the usual interpretation of Hooke's law unless the square of the strain be negligible. Supposing the internal equations to refer to the final deformed condition, the surface equations will undoubtedly also refer to this condition. Thus, so far as the terms independent of the eccentricity are concerned, we may suppose the mathematical theory applied to a sphere whose density  $\rho$  is uniform throughout, and whose radius  $a$  equals the earth's mean radius.

In this case the maximum stress-difference and the algebraically greatest strain are both found at the surface. Let us denote these by  $\bar{S}$  and  $\bar{s}$  respectively; and let  $s_0$  denote the greatest compression, which occurs at the centre, and  $u_a$  the radial displacement at the surface. Employing  $E$  and  $\eta$  as before, and denoting by  $g$  the acceleration due to the sphere's attraction at its surface, I find\*

$$\bar{S} = \frac{1}{5} g \rho a \frac{1-2\eta}{1-\eta}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\bar{s} = \frac{2}{5} \frac{g \rho a}{E} \frac{\eta(1-2\eta)}{1-\eta}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$s_0 = -\frac{3}{10} \frac{g \rho a}{E} \frac{(1-2\eta)(1-\eta/3)}{1-\eta}, \quad . \quad . \quad . \quad (3)$$

$$u_a = -\frac{1}{5} \frac{g \rho a^2}{E} (1-2\eta). \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Assuming for a moment these results to hold for a sphere in which  $g$  = gravity † at the earth's surface,  $\rho = 5.5$  times the density of water, and  $a = 3950$  miles, the following are the approximate numerical values answering to the values 0, .25, and .5 of  $\eta$  :—

TABLE IV.

$\eta =$	0	.25	.5
$\bar{S}$ , in tons weight per square inch .....	4440	2960	0
Longitudinal stress $E \bar{s}$ , in tons weight	0	1480	0
per square inch, which would produce			
a strain $\bar{s}$ .....			
$-s_0$ (see below) .....	1.03	.53	0
$-u_a$ , in miles (see below) .....	2700	1130	0

\* See (a) formulæ (17), (18a), and (19a), p. 281.

† The calculations treat the attraction on a cubic centimetre of water at the surface as equal to the weight of one gramme. In reality of course "gravity" includes the "centrifugal" force.



For a given value of  $\eta$  the value of  $\bar{S}$  is independent of  $E$ . It diminishes continually as  $\eta$  increases from zero. Since the value of  $\bar{s}$  depends on  $E$ , I have given the value of  $E\bar{s}$ , or the longitudinal stress which would produce in a bar of the material a strain equal  $\bar{s}$ . The value of  $E\bar{s}$  has a maximum of about 1520 tons weight per square inch, for  $\eta = 1 - \sqrt{1/2}$  or .293 nearly.

For  $\eta = .5$  the values of  $s_0$  and  $u_a$  are zero supposing  $E$  finite, but for other values of  $\eta$  one can obtain numerical measures of these quantities only by assigning numerical values to  $E$ . Now if the earth were an elastic solid truly spherical but for its rotation, the value of  $E$  answering to a given value of  $\eta$  would be determined from the eccentricity of the surface. But the action of the gravitational forces, as will be seen more clearly presently, largely reduces the eccentricity which rotation would produce in a sphere of given material. Thus the eccentricity varying inversely as  $E$ , the value of  $E$  answering to a given eccentricity is necessarily considerably smaller when gravitational forces act along with the "centrifugal" than when the latter act alone. Since the surface-strata are very variable and of much smaller mean density than the earth as a whole, any calculation of the reduction of our estimates of  $E$ , when gravitational forces are allowed for, which treats the earth as of uniform density cannot lay claim to great accuracy. For this reason, and also because I am specially desirous not to overstate the case against the application of the mathematical theory, I have in calculating the values of  $s_0$  and  $u_a$  in Table IV. ascribed to  $E$  the values it would possess in the total absence of gravitational forces, viz. the values  $1020 \times 10^6$  for  $\eta = 0$  and  $1220 \times 10^6$  for  $\eta = .25$  in the same units as before. The numerical values ascribed to  $s_0$  and  $u_a$  in the table are thus essentially minima, which would in reality have to be increased probably to a considerable extent.

It will be seen from the formulæ and from Table IV. that when  $\eta$  is zero or is small, the application of the mathematical theory would be fully justified on the greatest strain theory, while utterly condemned on the stress-difference theory. The principle (C) is in this case entirely in agreement with the stress-difference theory, and the application of the mathematical theory can in fact be supported only by those who reject this principle, and consider it possible for the stress-strain relation to remain linear though a solid sphere is reduced to one fourth or less of its original volume.

Noticing from (1) and (2) that  $E\bar{s}/\bar{S} = 2\eta$ , we see that for

all values of  $\eta$  less than  $\cdot 5$  the stress-difference theory is less favourable to the view that the mathematical theory is applicable than the greatest strain theory. If there is any truth in either theory, the earth's material cannot possibly possess a linear stress-strain relation for values of  $\eta$  such as  $\cdot 25$  (*i. e.* with a structure such as that of the metals) unless it be of a strength compared to which that of steel is insignificant. For such values of  $\eta$  the strains are also enormously in excess of those which can be admitted according to the principle (C).

When, however,  $\eta$  approaches the limiting value  $\cdot 5$  a complete change comes over the features of the case. The maximum stress-difference and all the strains diminish, eventually vanishing when  $\eta = \cdot 5$ . Thus none of the objections hitherto encountered can be urged against the application of the mathematical theory when  $\eta$  equals or nearly equals  $\cdot 5$ . To the exact value  $\cdot 5$  of  $\eta$  there is, I admit, a physical objection, which would doubtless have been urged by Maxwell, *viz.* that, supposing Young's modulus to be finite, this implies the material to be absolutely incompressible. There is, however, no obvious physical objection to the hypothesis that the material is very nearly incompressible, *i. e.* that  $\cdot 5 - \eta$  is very small\*; and an isotropic sphere with such a structure would, according to all our tests, remain perfectly elastic when possessed of the earth's mass and exposed to its gravitational forces.

In our previous estimate of the value of  $E$  the action of the gravitational forces in reducing the eccentricity was not taken into account. If the principles we have laid down as regulating the applicability of the mathematical theory be conceded, we need only consider the case when  $\cdot 5 - \eta$  is very small; and since the formulæ show that in this case a small variation in the value of  $\eta$  is of little consequence, we may for simplicity suppose  $\eta = \cdot 5$  exactly.

In order to show the nature of the uncertainty that must in reality be attached to the result, it seems desirable to give a general idea of the way in which the existence of gravitational forces affects the eccentricity. Let us imagine, then, that over the surface of a perfect sphere weightless matter is piled up, which transforms the surface into that of a slightly oblate spheroid whose polar and equatorial semi-axes are respectively  $a - 2d/3$  and  $a + d/3$ . Now suppose the heaped-up material to become heavy. The pressure it exerts on the surface below it is greatest in the equator and is zero at the

\* Stewart and Gee, in their 'Elementary Practical Physics,' vol i. pp. 192-5, give data from which they conclude that india-rubber is such a material.

poles. Thus the originally spherical surface will tend to sink at the equator and to rise at the poles; consequently the difference  $d$  between the equatorial and polar semi-axes of the spheroidal surface will diminish, but the diminution is clearly less the smaller the density of the heaped-up material.

It must be understood that this does not profess to be a complete account of what actually happens; but it may suffice to show that the gravitational forces tend to reduce the eccentricity which the centrifugal forces tend to develop, and also that this reduction may depend largely on the density of the surface layers. If the departure of the surface layers from the earth's mean density occurs mainly near the equator, then the action of the gravitational forces in reducing the eccentricity may be much less than it would seem to be on the hypothesis of an earth of uniform density.

Treating the density as uniform and  $\eta$  as equal  $\cdot 5$ , I find that, for a given value of  $E$ , the existence of the gravitational forces would in such a case as that of the earth reduce the difference between the equatorial and polar diameters called for by the rotation in the ratio of 9 : 40 approximately\*. Thus, for a given eccentricity, the value of  $E$  when the gravitational forces act is to its value when the centrifugal forces alone exist as 9 : 40. So in the supposed case of the earth, we should have to reduce  $E$  from  $141 \times 10^7$  to  $32 \times 10^7$  grammes weight per square centim. The maximum stress-difference reduces to 7.2 tons weight per square inch. The greatest strain remains  $\cdot 0018$ , as before, but it would answer to a purely longitudinal stress of only 3.6 tons per square inch. Owing to the less density of the surface-strata these reductions may be considerably too great, so that it is advisable to regard  $32 \times 10^7$  as essentially a lower limit to the value of  $E$ . As stated above, the numerical result for the value of  $E$  would be but little altered if we supposed  $\eta$  slightly less than  $\cdot 5$ ; but unless  $\cdot 5 - \eta$  be very small, the terms independent of the eccentricity become of importance in estimating the maximum stress-difference and greatest strain.

The conclusion to which the previous investigations lead is, that none of the principles at present recognized in the biconstant theory of isotropy are opposed to the hypothesis that the earth possesses in its interior an isotropic elastic solid structure with a linear stress-strain relation, provided its material be very nearly incompressible. But the hypothesis that the material in the interior shows an isotropic elastic structure, such as that of the ordinary metals under the ordinary conditions to which they are exposed on the earth's

\* Cf. (a) formula (21), p. 283, and (5), p. 287.

surface, can be maintained only by those who are prepared to reject the usual theories of the rupture, the limitation (C) in the size of the strains, and the argument introduced here from the theory of intermolecular forces. This raises no presumption against the hypothesis that the interior is in a perfectly solid state, and possessed of such a chemical constitution, say, as iron, if it be admitted that it is of a material in which the linearity of the stress-strain relation ceases when the compression becomes large.

The results obtained raise no presumption for or against the theory that the earth is in a liquid or plastic state. They merely show that any argument against the possibility of an elastic solid structure in a body of the earth's *form* is without foundation; and that any argument based on the destructive tendency of the enormous gravitational forces in a solid of its mass is inconclusive, even as directed against such structures as are compassed by the ordinary mathematical theory. It has not been shown that an æolotropic solid structure of some kind, or of a variety of kinds, may not satisfy all the conditions as well as or even better than a nearly incompressible isotropic material. The presumption is, in fact, that the conditions may be satisfied in an infinite number of ways.

It must be borne in mind that there may be fatal objections to an elastic solid structure which do not arise immediately from the theory of elasticity. Such an objection may arise from the rapid increase with the depth shown by the temperature near the earth's surface. My principal reason for referring to this is to point out that the common argument against the production of fluidity by the high internal temperature (viz. an assumed raising of the melting-point by pressure) has just as much weight for a nearly incompressible solid earth as for any other, because while the stress-difference in such an earth is small the internal pressures are extremely large.

Before passing to the second part of the paper, I have to confess that there is no reason to believe that some of the limitations assigned here to the application of the mathematical theory will be accepted by all or even by a majority of elasticians. In fact the mathematical theory has actually been applied by several recent writers under circumstances when most or all of the limitations proposed here are violated. For instance, this is to a certain extent the case in Professor Darwin's paper\*, "On the Stresses caused in the Interior of the Earth by the Weight of Continents and Mountains." In the principal part of the paper he supposes  $\eta = \cdot 5$ , when, as we

\* Phil. Trans. 1882, pp. 187-230.

have seen, none of the objections apply; but in his § 10, in order "to know how far the results . . . may differ, if the elastic solid be compressible," he supposes that while the rigidity constant is finite the bulk modulus is very small. In other words, he applies mathematical formulæ which assume  $\eta$  nearly equal to  $-1$ . Such a value has been here regarded as impossible. It should also be noticed that if  $\eta$  were equal  $-1$  then  $E$  would vanish, and if  $\eta$  be nearly  $-1$  the value of  $E$  must be very small. Thus the strains and displacements given by equations (2) to (4) would in the case supposed by Professor Darwin be enormously greater than even those given in Table IV. I do not observe, however, that either in the paper itself or in one supplementary\* to it Professor Darwin makes any explicit reference to the terms in the strain independent of the angular coordinates, from which the equations (1) to (4) are derived. I am thus unable to say whether his neglect of the limitations that these terms are here regarded as setting to the application of the mathematical theory is intentional or not. Again, in a recent paper†, "On Sir William Thomson's estimate of the Rigidity of the Earth," Mr. Love has also considered the problem of the earth treated as an isotropic elastic sphere, more especially for the value  $\cdot 25$  of  $\eta$ . In his equations (14) and (18) Mr. Love determines the values of two arbitrary constants which occur in the terms independent of the angular coordinates; and it is easily seen that the expression he would thence obtain for these terms is identical with mine‡. After determining the second constant he, however, dismisses the subject with the remark, "This . . . gives the mean radial displacement, a matter which need not detain us here." So far as I can see, Mr. Love makes no reference to any principle such as (C), nor to the possibility of the stress-strain relation ceasing to be linear.

I ought also to explain that in my paper (a), directing my attention solely to the theories of rupture, I left out of sight any such limitation as (B) or (C), and treated the case of an earth in which  $\eta=0$  as one in which, according to the greatest-strain theory of rupture, the mathematical theory was applicable. I also failed to notice that the case  $\eta=\cdot 5$  was sanctioned by the greatest-strain theory as well as by the stress-difference theory.

[To be continued.]

\* Proceedings of the Royal Society, vol. xxxviii. (1885), pp. 322-8.

† Trans. Camb. Phil. Soc. vol. xv. pp. 107-118.

‡ (a), equation (17), p. 281.