THURSDAY, FEBRUARY 1, 1872

THE INTERNAL FLUIDITY OF THE EARTH

WE have been favoured with permission to reprint the following extract from a letter addressed by Sir Wm. Thomson to Mr. G. Poulett Scrope:—

The University, Glasgow, *Jan.* 15, 1872

DEAR SIR,—I thank you very much for the copy of your beautiful book on Volcanoes, which you have been so kind as to send me through Professor Geikie. It is full of matter most interesting to me, and I promise myself great pleasure in reading it.

I see with much satisfaction, in your prefatory remarks, that you "earnestly protest against the assertion of some writers, that the theory of the internal fluidity of the globe is or ought to be generally accepted by geologists on the evidence of its high internal temperature." Your sentence upon the "attractive sensational idea that a molten in terior to the globe underlies a thin superficial crust; its surface agitated by tidal waves and flowing freely towards any issue that may here and there be opened for its outward escape," in which you say that you "do not think it can be supported by reasoning, based on any ascertained facts or phenomena," is thoroughly in accordance with true dynamics. It will, I trust, have a great effect in showing that volcanic phenomena, far from being decisive, as many geologists imagine them to be, in favour of a thin crust enclosing a wholly liquid interior, tend rather, the more thoroughly they are investigated, to an opposite conclusion.

I must, however, take exception to your next sentence, that in which you say that "M. Delaunay has disposed of the well-known astronomical argument of Mr. Hopkins and Sir W. Thomson, as to the entire or nearly entire solidity of the earth, derived from the nutation of its axis." Delaunay's deservedly high reputation as one of the first physical astronomers of the day, has naturally led many in this country to believe that his objection to the astronomical argument in favour of the earth's rigidity cannot but be valid. It has even been hastily assumed that the objection is founded on mathematical calculation, an error which the most cursory reading of Delaunay's paper corrects. His hypothesis of a viscous fluid breaks down utterly when tested by a simple calculation of the amount of tangential force required to give to any globular portion of the interior mass the precessional and nutational motions, which, with other physical astronomers, he attributes to the earth as a whole. Thus: taking the ratio of polar diameter to equatorial diameter as 299 to 300, and the density of the upper crust as half the mean density of the earth, I find (from the ordinary elementary formulæ) that when the moon's declination is $28°\frac{1}{2}$, the couple with which she tends to turn the plane of the earth's equator towards the plane of her own centre and the equinoctial line has for its moment a force of $\cdot 285 \times 10^{18}$ times the gravity of one gramme at the earth's surface, or rather more than a quarter of a million million tons weight, on an arm equal to the earth's radius. A quadrant of the earth being ten thousand kilometres, the area is five hundred and nine million square kilometres, or 5·09 million million million square centimetres. Hence a force of $\cdot 285 \times 10^{18}$ grammes weight distributed equally over two-thirds of the earth's area would give ·084 of a gramme weight per square centimetre. This supposition is allowable (for reasons with which I need not trouble you) in estimating roughly the greatest amount of tangential force acting between the upper crust and a spherical interior mass in contact with it, from the preceding accurate calculation of the whole couple exerted by the moon on the upper crust. It is thus demonstrable that the earth's crust must, as a whole, down to depths of hundreds of kilometres, be capable of transmitting tangential stress amounting to nearly $\frac{1}{10}$ of a gramme weight per square centimetre. Under any of such stress as this any plastic substance which could commonly be called a viscous fluid would be drawn out of shape with great rapidity. Stokes, who discovered the theory of fluid viscosity, and first made accurate investigations of its amount in absolute measure, found that a cubic centimetre of water, if exposed to tangential force of the millionth part of $\frac{1}{10}$ of a gramme weight on each of four sides, would even under so small a distorting stress as this, become distorted so rapidly that at the end of a second of time its four corresponding right angles would become one pair of them acute and the other obtuse, by as much as a two-hundredth part of the angle whose arc is radius, that is to say by ·29 of a degree. Not as much as a ten-million-millionth part of this distortion could be produced every second of time by the lunar influence in the material underlying the earth's crust without very sensibly affecting precession and nutation; for the effect of the maximum couple exerted on the upper crust by the moon is to turn the whole earth in a second of time through an angle of a one-hundred-million-millionth of ·57 of a degree, so as to give to it at the end of a second the position obtained by geometrically compounding this angular displacement with the angular displacement due simply to rotation. Hence the viscosity assumed by Delaunay, to produce the effect he attributes to it, must be more than ten million million million times the viscosity of water. How much more may be easily estimated with some degree of precision from Helmholtz's mathematical solution of the problem of finding the motion of a viscous fluid contained in a rigid spherical envelope urged by periodically varying couples.* The most interesting part of the application of this solution to the hypothetical problem regarding the earth, is to find how rapidly the obliquity of the ecliptic would be done away with by any assumed degree of viscosity in the interior; such an amount of viscosity, for example, as would render the excesses of precession and nutation above their values for a perfectly rigid interior, not greater than observation could admit.

The hypothesis of a continuous internal viscous fluid being disposed of, the question occurs, what rigidity must the interior mass have, even if enclosed in a perfectly rigid crust, to produce the actual phenomena of precession and nutation? The solutions given by Lamé and myself of the problem of the vibrations of an elastic solid globe, may be readily applied to determine the influences on precession and on the several nutations, which would be produced by elastic yielding with any assumed rigidity

* Helmholtz and Piotrowski, "Ueber Reibung tropfbarer Flüssigkeiten," Imp. Acad. Vienna, 1860.

short of infinite rigidity. This application I have no time at present to make; but without attempting a rigorous investigation, it is easy roughly to estimate an inferior limit to the admissable rigidity. In the first place, suppose, with perfect elasticity, the rigidity be so slight that distorting stress of $\frac{1}{10}$ of a gramme weight would produce an angular distortion of a half degree or a degree. The whole would not rotate as a rigid body round one "instantaneous axis" at each instant, but the rotation would take place internally, round axes deviating from the axis of external figure, by angles to be measured in the plane through it and the line perpendicular to the ecliptic in the direction towards the latter line. These angular deviations would be greater and greater the more near we come to the earth's centre, and the greatest angular deviation would be comparable with 1°. Hence the moment of momentum round the solsticial line would be sensibly less than if the whole mass rotated round the axis of figure. Now suppose for a moment our measurement of force to be founded on a year as the unit of time. We find the amount of precession in a year by dividing the mean amount of the whole couple due to the influence of moon and sun by the moment of momentum of the earth's motion round the solsticial line. Hence the amount of precession would be sensibly augmented by the elastic yielding; for the motive couple is uninfluenced by the elastic yielding, if we suppose the earth to be of uniform internal density. An ordinary elastic jelly presents a specimen of the degree of elasticity here supposed, as is easily seen when we consider that the mass of a cubic centimetre of such material is a gramme, and therefore that the weight of a cubic centimetre of the substance is the "gramme weight" understood in the specification $\frac{1}{10}$ of a gramme weight per square centimetre. If then, the interior mass of the earth were no more rigid than an ordinary elastic jelly, and if the upper crust were rigid enough to resist any change of figure that could sensibly influence the result, the precession would be considerably more rapid than if the rigidity were infinite throughout. The lunar nineteen-yearly nutation proves a higher degree of elasticity than this; the solar semi-annual nutation still a higher degree; and still higher yet the imperceptibility of the lunar fortnightly nutation; provided always we suppose the interior mass to be of uniform density, and the upper crust rigid enough to permit no influential change of figure.

The motive of the nineteen-yearly precession may be mechanically represented by two circles of matter pivoted on diameters fixed in the plane of the earth's equator, bisecting one another perpendicularly at the earth's centre. These two circles must oscillate round their pivot-diameters, each through an angle of about 5° on one side and the other of the plane of the equator, in a period of about nineteen years, to produce the lunar nineteen-yearly nutation (more nearly eighteen years seven months). If the radius of each of the supposed material circles is equal to the moon's mean distance from the earth, the mass of each must be a little less than the moon's mass, and one of them a little less than the other.* The diameter on which the latter is pivoted is to be the equinoctial line. The latter alone causes the nutation in right ascension; the former the nutation in declination. The phases of maximum and of zero deflection, in the oscillations of the two circles, follow alternately at equal intervals of time, so that when either is in the plane of the earth's equator, the other is at its greatest inclination of 5° on either side. Taking one of the constituents of the nutational motive alone, we find, on the principles indicated above, $\frac{1}{100}$ of a gramme weight per square centimetre as a very rough estimate for the greatest tangential stress produced by it in the material underlying the earth's crust. Now it is clear that the central parts of the earth and the upper crust cannot, in the course of the nutatory oscillations, experience relative angular motions to any extent considerable in comparison with the nutation of the upper crust, without considerably affecting the whole amount of the nutation. The nutation in declination amounts to 9″·25 on each side of the mean position of the earth's poles, and therefore the tangential stress of $\frac{1}{100}$ of a gramme weight per square centimetre cannot produce an angular distortion considerable in comparison with 9″.

An angular distortion of 8″ is produced in a cube of glass by a distorting stress of about ten grammes weight per square centimetre. We may, therefore, safely conclude that the rigidity of the earth's interior substance could not be less than a millionth of the rigidity of glass without very sensibly augmenting the lunar nineteen-yearly nutation. The lunar fortnightly nutation in declination amounts theoretically to about ·1″, and it is so small as to have hitherto escaped observation. It probably would have been so large as to have been observed were the interior rigidity of the earth anything less than $\frac{1}{200000}$ of that of glass, always provided that the upper crust is rigid enough to prevent any change of form sensibly influencing the nutational motive couple. To understand the degree of rigidity meant by "$\frac{1}{200000}$ of the rigidity of glass," imagine a sheet of some such substance as gutta-percha or vulcanised india-rubber of a square metre area and a centimetre thickness. Let one side of the sheet be cemented to a perfectly hard plane vertical wall, and let a slab of lead 8·8 centimetres thick (weighing therefore a metrical ton)* be cemented to the other side of it. If the rigidity of the substance be $\frac{1}{200000}$ of the rigidity of glass,† and the range of its elasticity sufficient, the side of the sheet to which the lead is attached will be dragged down relatively to the other through a space of $\frac{1}{12}$ of a centimetre.

In the argument from tidal deformations of the solid part of the earth's material, which I communicated to the Royal Society ten years ago, and mentioned incidentally at the recent meeting of the British Association, I showed that though precession and nutation would be augmented by want of rigidity in the interior, they would be diminished by want of rigidity in the upper crust, and that on no probable hypothesis can we escape the conclusion that the earth as a whole is less yielding than a globe of glass of the same dimensions and exposed to the same forces. That argument, therefore, proves about 200,000 times greater rigidity for the earth as a whole than what I

* The greater is equal to the moon's mass multiplied by the cosine of the obliquity of the ecliptic; the less is equal to the moon's mass multiplied by the cosine of twice the obliquity of the ecliptic.

* The metrical ton, or the mass of a cubic metre of water at temperature of maximum density, is ·9842 of the British ton. The thickness of a slab of lead of a square metre area, weighing a metrical ton, is, of course, equal to a metre divided by the specific gravity of lead.

† Everett's measurements give 244×10^6 centimetres weight per square centimetre for the rigidity of the glass on which he experimented. Instead of this I take 240×10^6, for simplicity.

have now written to you proves for the interior of the earth on the supposition of a thin preternaturally rigid crust.

I must apologise to you for having troubled you with so long a letter. I did not intend to make it so long when I commenced, but I have been led on by considerations of details, inevitable when such a subject is once entered upon.—I remain, yours very truly,

WILLIAM THOMSON

G. Poulett Scrope, Esq., F.R.S.

THE SOLAR ECLIPSE

IN the communication to NATURE, written from Ootacamund, I promised another when I was in possession of more information as to the work done, not only by the British Association parties, but by those representing the Indian and French Governments. Let me now endeavour to redeem my promise, seeing that since that communication was penned I have had the happiness of hearing from M. Janssen's own lips an account of what he did; have met Captain Waterhouse, the last representative at Ootacamund of Colonel Tennant's party; have visited Mr. Pogson at Madras, who obligingly gave me an account of the results obtained at Avenashi; and last, but not least, have learnt since my return home that the Jaffna party were successful, not only with the polariscope, but also with the camera and spectroscope.

Within a few minutes of the despatch of my last article I found that Captain Waterhouse, who assisted Mr. Hennessy in exposing the photographic plates taken by Colonel Tennant's party, was still at Ootacamund, and this welcome intelligence was soon followed by Captain Waterhouse himself, who was so good as to bring with him a drawing of one of the photographs; the plates themselves having been taken down the ghaut by Colonel Tennant, with the intention of comparing them at Pothonore with those taken by Mr. Davis. Unfortunately, as has been already stated, we missed each other, and so an absolute comparison of photographs did not take place; but from the drawing it was evident that in the two series the main form of the corona was the same. The photographs I learned were very sharp and good, and one appreciates their value the more when it is known that only a very little time before they were taken, any success, even a partial one, seemed out of the question, so persistently did cloud and mist hang over Dodabet on the eventful morning. I gathered that the spectroscopic observations had also been successful, and that a continuous spectrum with 1474 had been observed. If more lines than this were not seen, it is easily to be accounted for by the relatively long focal length of the object-glass employed to throw an image of the eclipsed sun on the slit.

Not until the morning after my interview with Captain Waterhouse did I learn the whereabouts of Dr. Janssen, who, from a study of the habits of the clouds and their prevailing drift, had concluded that the neighbourhood of Ootacamund was not the best that could be chosen. He had consequently taken his departure, and it seemed at first as if his whereabouts was known to no one. At last, however, Prof. Respighi and myself came upon his spoor; he was at Sholoor, on the N.E. flank of the range, at the solitary house of a tea-planter, to which there was no road, but which might be reached on ponies if a guide to it could be found. This guide Captain Sargeant, of the Revenue Department, obligingly provided, and in no very long time we reached the beautiful spot which Dr. Janssen had chosen.

It will be better that I should state his results in his own words. In a letter* to Prof. De La Rive, dated December 26, he thus writes:—

"J'ai été favorisé par un ciel d'une pureté presque absolue. Cette circonstance, et surtout les dispositions optiques toutes nouvelles que j'avais prises, m'ont permis de faire sur la couronne des constatations qui démontrent son origine solaire (pour la meilleure partie).

"Dans mon télescope,† le spectre de la couronne s'est montré non pas continu, mais remarquablement complexe. J'y ai constaté:

"Les raies brillantes du gaz hydrogène qui forme le principal élément des protubérances et de la chromosphère.

"La raie brillante verte déjà signalée aux éclipses de 1869 et 1870, et quelques autres plus faibles.

"Des raies obscures du spectre solaire ordinaire, notamment D. Ces raies sont beaucoup plus difficiles à apercevoir.

"Mes observations prouvent que, indépendamment des matières cosmiques qui doivent exister dans le voisinage du Soleil, il existe autour de cet astre une atmosphère très étendue, excessivement rare, à base d'hydrogène.

"Cette atmosphère, qui forme sans doute la dernière enveloppe gazeuse du Soleil, s'alimente de la matière des protubérances, lancée avec une si grande violence, des entrailles de la photosphère. Mais elle se distingue de la chromosphère et des protubérances, par une densité énormément plus faible, une température moins élevée, et peut-être par la présence de certain gaz différents.

"Il y a donc lieu de distinguer cette nouvelle atmosphère solaire. Je propose de la nommer *atmosphère coronale*, désignation qui rappelle que c'est elle qui produit la meilleure partie des phénomènes lumineux qui ont été désignés jusqu'ici sous le nom de couronne solaire.

"En annonçant ce résultat, je n'oublie pas, quant à moi, tout ce que nous devons aux travaux qui l'ont préparé, notamment ceux des astronomes américains aux éclipses de 1869 et 1870."

It will be seen that the importance of the brilliancy of the image, so strongly insisted upon by the Eclipse Committee in their Instructions, had been fully recognised by Dr. Janssen, whose instrument had more light even than those used by the British parties, who used "Browning With" reflectors of 9¼ inches aperture, and some 6 feet focus.

Although my account, in this place and at this time can only be of the most general character, the coincidence obtained by Janssen, Respighi, and myself on one point may be briefly referred to, namely, the distinct proof obtained by each of us that above the most vivid chromospheric layer, and even the prominences, we have hydrogen with its most familiar bright lines, and with much of the "structure" of its spectrum; these proofs being derived not only from the old method of inquiry, but from the new one employed by Professor Respighi and myself.

We spent the remainder of the day at Sholoor in mounting the hill at the back of the house to see the observatory, and to admire the wonderful view of the plains of Mysore, which was visible between a break in the hills; while the immediate neighbourhood, with its water-

* Bibliothèque Universelle, January 15, 1872, p. 103.

† Ce télescope a une ouverture de $0^{m}\,37$, et $1^{m}\,42$ seulement de distance focale. Les images y sont de 12 à 16 fois plus lumineuses que dans une lunette astronomique ordinaire. Le spectroscope avait été construit pour utiliser toute cette lumière.