HOPKINS versus DELAUNAY

WE have received from Archdeacon Pratt a copy of a paper communicated to the *Philosophical Magazine* on Delaunay's objection to Hopkins's method of determining the thickness of the earth's crust by the precession and nutation of the earth's axis.

The archdeacon, on this most important question, states :—

"I am ready to allow, and so would Mr. Hopkins have allowed, that if the crust of the earth revolved round a steady axis, always parallel to itself in space, and if at some particular epoch a difference existed between the rate of movement of the crust and of the fluid within it, the resulting friction would gradually destroy this difference and bring about a conformity in the motion of both parts. I will even go further, and allow that the effect of the internal friction and viscidity of the fluid may be such that the resulting rotary motion may be the same as that which the whole mass would have had at the epoch if it had suddenly become one solid body and thereby suddenly retarded the rotation."

He thus illustrates his position :—

"Suppose a spherical shell or crust of mass C to have within it a solid spherical nucleus, of radius b and mass N, fitting it exactly; and the crust to receive an angular velocity of rotation around an axis fixed in the crust, the nucleus at that moment having no angular velocity; but suppose that a slight force of friction between the surfaces gradually generates a rotary motion in the nucleus; and suppose this force to vary as the difference between the angular velocities of the crust and nucleus—that is, of the surfaces in contact. Let ω and ω' be the angular velocities at the time t, k and k' the radii of gyration of the two bodies, $F(\omega-\omega')$ the force at the equator of the nucleus which represents the friction between it and the crust. Then the equations of motion are

$$\frac{d\omega}{dt} = -\frac{Fb}{Ck^2}(\omega-\omega'), \quad \frac{d\omega'}{dt} = \frac{Fb}{Nk'^2}(\omega-\omega') \quad . \quad . \quad . \quad (1)$$

Suppose also that β would have been the angular velocity, when the primitive impulse was given, on the hypothesis of the crust and nucleus being rigidly connected so as to be one mass. Then

$$\beta(Ck^2+Nk'^2)=\alpha Ck^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Subtracting the second of equations (1) from the first, putting

$$Fb\left(\frac{1}{Ck^2}+\frac{1}{Nk'^2}\right)=c, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and integrating, we have

$$\omega-\omega' = \text{const.} \times e^{-ct}.$$

When $t=0$, $\omega=\alpha$ and $\omega'=0$;

$$\therefore\ \omega-\omega'=\alpha e^{-ct}.$$

Hence, by the first of equations (1),

$$\frac{d\omega}{dt}=-\frac{Fb\alpha}{Ck^2}e^{-ct}=-\frac{Nk'^2c\alpha}{Ck^2+Nk'^2}e^{-ct}, \text{ by (3)};$$

$$\therefore\ \omega=\alpha-\frac{Nk'^2}{Ck^2+Nk'^2}\alpha\,(1-e^{-ct});$$

and also

$$=\beta\left(1+\frac{Nk'^2}{Ck^2}e^{-ct}\right), \text{ by (2)"}$$

The paper then continues :—"The first of these expressions shows that the angular velocity of the crust begins with α; and when ct becomes very large indeed, it is reduced to β. Hence the effect of the constant friction of the nucleus against the inner surface of the crust is *at last* to reduce the velocity of the crust to what it would have been at first if the crust and nucleus had been one solid mass.

"We may conclude perhaps that the same effect would be produced, though in a much longer time, if the interior were not a solid sphere, but a fluid mass.

"The above reasoning shows that if the disturbing force producing precession and nutation did not exist, and the interior of the earth were fluid (whatever the thickness of the crust), it may be fairly assumed that the motion of rotation of the crust would now, the earth having existed so many ages, be exactly what it would have been had the earth been one solid mass, all difference of motion having been long ago annihilated by the internal friction and viscidity.

"But the disturbing force producing precession and nutation does exist. It consists of two parts, one constant and the other variable and periodical. The constant part is that which produces the steady precession of the axis (and which I will call for convenience the precessional force); the other produces the nutation. I will consider the precession first. Suppose now, for the sake of argument, that at the present moment, as M. Delaunay imagines, the crust and the fluid are revolving precisely as one mass, all previous differences of motion, even under the action of the disturbing force which produces precession and nutation, having been annihilated by friction and viscidity. I ask—What will be the action of the precessional force from this moment? It tends to draw the pole of the crust towards the pole of the ecliptic: and this tendency, as mathematical physicists well understand, combined with the rotary motion of the crust, produces this singular result, viz., the pole does not move towards the pole of the ecliptic, but shifts in a direction at right angles to the line joining the poles towards the west; so that the inclination of the axis to the ecliptic remains constant, but the axis shifts towards the west. The space through which it shifts in an infinitesimal portion of time varies as the length of the time and the force directly, and as the inertia of the mass to be moved inversely. The inertia of the mass depends upon the thickness of the crust only; for the friction of the fluid against the inner surface of the crust (which might, as I have shown, in the course of years, produce a sensible effect) cannot do so during the infinitesimal portion of time I am considering before the precession is actually produced. The precessional force has its full effect in producing the precession of the solid crust, the fluid not having time to diminish that effect before the axis has assumed a new position; and in this new position of the axis the precessional force is precisely the same in amount as before, to go on causing the precession as before. The precessional force is, in fact, ever alive and active, and shows this in incessantly producing the effect I have deseribed; and the precession goes on steadily, the amount of it depending upon the mass of the crust thus moved, which the fluid has not time to retard or lessen. M. Delaunay says that 'the additional motion due to the above-mentioned causes (the disturbing forces which give rise to precession and nutation) is of such slowness, that the fluid mass which constitutes the interior of the globe must follow along with the crust which confines it, exactly as if the whole formed one solid mass throughout." In reply to this, I say that it is not the slowness of the motion, but the want of solid connection between the crust and the fluid in contact with it that affects the problem. The motion, whatever its amount, is incessantly being generated by the disturbing force, and owing to this want of solid connection, the friction of the fluid has not time during the successive moments during which the precession is generated, to stop or even sensibly to check it.

"It will thus be seen that at every instant the precessional force proceeding from the action of the sun and moon on the protuberant part of the earth's mass will, if the earth be a solid mass, have to move the whole mass; and if the earth have a solid crust only with a fluid interior, the force will have to move only the crust against the evanescent resistance of the fluid within during so short a space of time as it takes to produce precession. The resulting precessional motion will be different in the two cases; and therefore the actual amount of the precession which the earth's axis has (and which is a matter of observation) is a good test of the solidity or fluidity of the interior. This is Mr. Hopkins's method.

"The force producing nutation is much smaller, even at its maximum, than the precessional force. Its effect, however, is precisely the same in this respect—that it depends upon the mass of the solid crust, and in no respect upon the friction of the fluid within it, which has not time to influence the nutation before the nutation is actually produced.

"I do not here undertake to go into Mr. Hopkins's numerical calculations; I simply vindicate his method. I do not here consider what modification the elasticity of the solid material of the earth may have upon his numerical results. I conceive that it would have *no* effect, if the disturbing force were constant and there were no nutation. For, under the dragging influence (if I may so call it) of the constant precessional force, the solid material would be under a steady strain, and would communicate the effect of the force, continuously acting, from particle to particle of the solid part as if it were really rigid; and the resulting precessional motion would be greater or less as the mass of the solid part may be smaller or larger—that is, the solid crust thinner or thicker. But as the disturbing force is not constant, but variable, and there is constantly nutation of the axis as well as precession, the action above described will be somewhat modified;

and the elasticity of the solid material may be expected to have some influence on the result. This influence, however, will be minute, as the part of the disturbing force which is variable and produces nutation is very much smaller, even at its maximum, than the precessional force. The consideration of this matter, however, has no bearing upon the validity or not of Mr. Hopkins's method, but simply upon the numerical value of his final result, not upon the question of the fluidity or solidity of the earth's mass."

The Archdeacon is of opinion "that the strictures of M. Delaunay upon this method, which the genius of Mr. Hopkins devised, betray an oversight of the real point upon which the success of his method depends, and that this method stands unimpaired."

SCIENTIFIC SERIALS

THE *Geological Magazine* for July (No. 73) contains rather ewer original articles than usual, but what there are will be found interesting. The series of notices of eminent living geologists is continued in a notice of one of the most accomplished of the number, Professor John Phillips, of whom we have a good biography, but a very unsatisfactory portrait. Mr. Carruthers gives a notice of the so-called fossil forest near Cairo; he distinguishes a new species of *Nicolia* (*N. owenii*), and illustrates its microscopic structure as compared with that of the old species *N. egyptiaca* Unger. — Mr. Kinahan communicates a paper containing a comparison of the geological features of Devon, Cornwall, and Galway, with a discussion of the means by which they have been produced; and Miss E. Hodgson a long disquisition on the origin and distribution of the granite-drift of the Furness district. The longest article in the journal is a report of Mr. David Forbes' lecture on Volcanoes, which will be read with much interest.

The *Journal of the Asiatic Society* for April, contains the following Natural History papers— Observations on some Indian and Malayan Amphibia and Reptilia, by Dr. F. Stoliczka. The species described in this paper have been partially collected by the author along the Burmese and Malayan coast, in Penang and Singapore, partially at the Nicobar and Andaman islands, only a few species are noticed from Java, and a few also from the N. W. Himalayas. Short notes on the geographical distribution, and on the general character of the amphibian and reptilian fauna of the Andamans and Nicobars, form a brief preface to the detailed descriptions. Complete lists of all the known species occurring on the two last-named groups of islands are appended. Dr. Stoliczka gave a short sketch of the relations existing between the Andaman and Nicobar reptilian fauna and that of Burma on the one, and that of Java, Sumatra, and the Philippine islands on the other hand. All these islands, he said, have many species in common. He also specially notices the very great number of viperine snakes (*Trimeresurus*) which are to be met with at the Nicobars, but fortunately these species appear to be less dangerous than continental forms usually are. The Nicobarese say that not a single fatal case has resulted from the bite of these *Trimeresurus*, and certainly all the specimens examined had a comparatively small poison-gland. The result of the bite is said to be only a swelling of the wounded part. Dr. Stoliczka also exhibited a specimen of the rare *Callophis intestinalis* obtained from Upper Burma. The species has the poison glands extending from the head to about one-third of the total length of the body, lying free in the cavity of the anterior part, and causing the heart to be much further removed backward than is generally the case in other species of snakes. The President thought there were one or two remarkable features in Dr. Stoliczka's interesting paper. One to which he particularly referred was the relative inefficiency of the poison in certain snakes of Penang and the Nicobars in comparison with the poison of the cognate species found in this country. He did not know whether the circumstances which rendered the possession of an invariably fatal weapon necessary to particular classes of snakes in the struggle for life, while others could maintain themselves without it, had yet received much attention. *A priori*, he thought, one would be disposed to expect that a poison which would disable without causing immediate death, would be more deterrent in its effects, and, therefore, more widely useful to its possessor, than one which killed instantly. At any rate it was curious to find some of the insular species of snakes, though provided with a perfect poison apparatus, much less fatal in the effect of their bite than other closely allied species in Bengal were. The investigation of the causes which had led to this difference ought to be attractive. A short discussion on the effects of snake poisoning ensued. Mr. Waldie desired to know what the symptoms resulted from the bite of the Nicobar vipers, and whether they are the same as are usually known to originate from the bite of other poisonous snakes. Dr. Stoliczka said that the Nicobarese only speak of a swelling of the bitten part, and that they exhibit very little fear of these snakes. Dr. Stoliczka also observed that the poison gland in the species of *Trimeresurus* which he had examined, has a simple glandular form without any appendages, but the skin forming it is very tough, and internally partitioned by numerous irregular lamellæ. The poison of the fresh snake was always present in a comparatively small quantity, and appeared less viscose than the Cobra poison. The differences between the effects of poisoning of the cobra and daboia had been pointed out by Dr. Fayrer.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 22.—II. "On the Physics of Arctic Ice as explanatory of the Glacial Remains in Scotland." By Dr. Robert Brown, F.R.G.S., &c. In this paper the author entered into an extended inquiry how far the formation of the boulder-clays and other glacial remains in Scotland and the North of England can be accounted for, on the theory of a great ice-covering having at one time overlain the country in much the same manner as it does now Greenland and other extreme Arctic countries. Taking the hypothesis of Agassiz as his groundwork, Dr. Brown entered into a minute description of the present glacier-system of Greenland, and the nature of Arctic ice-action; and into an inquiry how far glacial remains in Britain correspond with those at present in course of formation in Greenland and at the bottom of Baffins Bay, Davis Straits, and the fjords and bays adjoining these seas. These inquiries were commenced in the year 1861, and have been continued at intervals ever since up to the present summer in various portions of the Arctic regions, the Continent of Europe, in Great Britain, and in North America across to the Pacific. The results of these extended researches have led him to conclude—1. That the subazoic boulder-clay corresponds with the *moraine profonde* which underlies glaciers, and in all likelihood is the immediate base on which the ice-cap of Greenland rests. 2. That the fossiliferous, laminated, or brick-clays find their counterpart in the thick impalpable mud which the subglacial streams are pouring into the sea, filling up the fjords, even shoaling the sea far out, and absolutely in some cases turning the glaciers from their course into other valleys. Allowing the very moderate computation that this impalpable mud accumulates at the rate of only six inches per annum, a deposit of fifty feet in a century must form. If Scotland was at one time covered with an ice-cap, or had glaciers of any extent (as cannot be doubted), then this deposit must have been equally forming, and as a geological formation must be accounted for. No difference could be detected between this glacial mud and the present brick-clays, and every fact went to show that it was to this that we must look for the formation of these laminated fossiliferous clays. The amount of earth deposited on the bottom by icebergs was very insignificant indeed, and could in no degree account for the *boulder-clay*, though it was shown that much of the *boulder-drift* in *some places* could be so accounted for. It was, however, demonstrated that there was a great distinction between the boulders which belonged to the *moraine profonde* and those which were carried off on icebergs as part of the ordinary lateral moraines. The fjords, as already partially advocated in a paper in the Journal of the Royal Geographical Society (vol. xxxix.), he considered due to glacial action, the glaciers having taken possession of these fjords when they were mere valleys, when the coast was higher than now. He further showed that the American explorers are in error when they describe the coast of Greenland as rising to the north of 73°, and subsiding to the south of that parallel. There had been a former rise of the coast, and a fall was now in course of progress through the whole extent. Whether these had previously alternated with other rises and falls is not clearly evidenced by remains, but no doubt exists that a rise preceded the present subsidence. Numerous facts were adduced in support of this assertion. The remainder of Dr. Brown's paper was occupied in