INSTABILITY OF THE EARTH'S AXIS OF ROTATION

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THE axis of rotation of the earth undergoes certain changes of direction, some of which are the results of known causes, whereas others are not yet understood. The major movement is the precession, whereby the axis of the geoidal figure of the earth together with the vector of the angular velocity and that of the angular momentum sweep out a cone of 23° once in twenty-six thousand years. One effect of this is that the year as defined by the seasons is different by one part in twenty-six thousand from the year as defined by the earth's position on its orbit. The precession is due to the motion of the spinning earth under the action of an external couple, namely the gravitational couple exerted by the sun and the moon upon the non-spherical earth. That couple depends on the relative position of these objects and is therefore not steady in time. The result is a superimposed wobble in the direction of the axis of rotation, and it has been given the name of 'nutation'.

There exist, however, other effects not produced by external couples but by a redistribution of matter or angular momentum on or within the earth. They are movements whereby the direction of the axis of rotation remains nearly fixed in space, while the earth itself moves relative to it. The absolute angle by which the axis of rotation moves is some three or four hundred times smaller than the movement of the earth, and it is therefore only the latter which is a measurable effect, usually noted as a variation in the apparent latitude of observation points on the surface of the earth. The angle involved is small, of the order of $\frac{1}{4}''$; the motion is complicated and irregular, but two components can be recognized, one of a period of one year and one of a period of fourteen months.

These movements of the earth relative to its axis are of great significance to theoretical work on geophysics. While the other motions are accurately described by the rigid-body dynamics of the system, this one has its origin, period and damping related to physical conditions on or in the earth. The annual term is thought to have its origin in atmospheric effects[1,2]. The origin of the excitation of the fourteen monthly motion is unknown; but it is understood that the period is the one corresponding to the 'free' or 'Eulerian' nutation. That period is given for a rigid body by $\left(\frac{A}{C - A}\right) T$, where A, A, C are the principal moments of inertia and T is the rotation period. This value would be 305 days for the earth, but a very modest amount of elastic deformation would lengthen it to the observed 420 days. The mean Young's modulus of the earth has to be of the same order as that of steel for the period to suffer as little lengthening due to the deformation[1].

The question of the excitation and the damping of the motion is more difficult to treat. There is no clear-cut way of distinguishing the component of the motion which corresponds to the unknown excitation from that which corresponds to the decay due to internal damping. It can, however, be inferred, on the assumption that the excitation possesses no consistency, that the damping time must be fairly short and cannot be more than a few periods, ten at the most, for a decay to $1/e$ of the original amplitude in the absence of further excitation. Alone the actual amplitude of the motion, together with an estimate of the excitation taken from just those occasions when the amplitude suffered an increase, puts a limit of this order on the damping time; with lesser damping the amplitude would be expected to be greater according to the statistical addition of exciting impulses. (Jeffreys quotes an estimate of fifteen years; Walker and Young, recently, one of three years[3].)

Where does this high damping arise? The requirement is for some relative motion to occur in which energy can be dissipated. But the obvious places on the surface, such as the atmosphere and the oceans, cannot be responsible, for the material that moves relative to the solid part of the earth requires to have more than a certain moment of inertia in order to be able to cause the damping at all; and it then requires to be coupled in a suitably dissipative way to the solid part of the earth. The moment of inertia of the atmosphere and oceans is insufficient. The liquid core of the earth has been discussed as a possible source of the damping, but its small moment of inertia (10 or 12 per cent of the whole) can be shown to rule it out, too[4].

The large damping must then arise from a dissipative deformation of the solid part of the earth. The existence of a high Young's modulus is no guide to the plastic flow that may occur in the material, or to the relation between the flow and the stress. It is difficult to see in what other way this lack of permanent stiffness could be measured. The damping of earthquakes is scarcely relevant, as it is concerned with periods 10^8 times shorter. The solid-body tides of the earth are concerned with a period a thousand times shorter than the nutation, and even there the presence of a small amount of dissipation would escape detection.

The existence of gravity anomalies over large regions is sometimes taken as an indication of long-term stiffness down to a depth of at least a few hundred kilometres. Such gravity anomalies are related to long-lived tectonic features and are therefore quite reasonably inferred to be long-lived themselves. But on the other hand, the bulk of the materials on the surface of the earth—the continents, the mountains—are not supported by substantial non-hydrostatic forces from depths below a hundred kilometres. If the occurrence of permanent stiffness down to great depths much below a hundred kilometres were a world-wide phenomenon, then it would be a matter for surprise to find that all the major deformations of the crust that have occurred have managed only so rarely to pile material up on an underlying stiff base to produce a very large gravity anomaly. The information obtained from gravity surveys therefore suggests that, except for a few areas, there is no appreciable permanent stiffness below about a hundred kilometres. Stiffness in some localities is, of course, not decisive for the behaviour of the wobbling earth.

Another line of argument also suggests the absence of permanent stiffness of the main geoidal shape of the earth. At present the solid part of the earth is very nearly the equilibrium shape appropriate to the present speed of rotation. Any severe departure either way from this shape would show itself very simply: the oceans which take up the equilibrium shape instantly would then dominate either at the poles or in equatorial regions. In fact, the distribution of land and water shows no significant tendency of such a type. On the other hand, the earth's speed of rotation is believed to have changed significantly in geological time. It has been changed either by giving up angular momentum to the moon through the agency of tidal friction—and the moon shows signs of a tidal bulge appropriate to a greater proximity to the earth—or the speed of rotation may be determined by the resonance with atmospheric tides, in which case it must have changed substantially with changes in the structure of the atmosphere[5].

There exist many forms of non-elastic behaviour of solids other than plastic flow, and all of them lead to dissipation of energy in a stress cycle. "Elastic after working", for example, has been mentioned by Jeffreys in this context[1]. Plastic flow is, however, the simplest type of behaviour, and it occurs in most solids in the vicinity of the melting point. It is a behaviour in which there is no 'memory' in the material, the instantaneous stress defining the instantaneous elastic strain and the instantaneous rate of flow. There seems to be no case for invoking any of the more complicated types of behaviour in which the material possesses a 'memory' determining its reaction to a stress, such as is often the case with materials composed of large and complicated molecules. Also plastic flow must in any event be invoked to account for the absence of long-term rigidity which has been inferred from other considerations.

The speed of plastic flow can now be deduced on the basis of the assumption that the damping of the free nutation arises entirely from this cause, and that the characteristic time is ten periods. If there were an angular separation between the axis of figure and the axis of rotation, then the plastic flow would be such as to allow the figure to become deformed and hence its axis of symmetry to move. In the absence of other effects, the axis of symmetry would then approach the axis of rotation by ten per cent of their angular separation in each period of fourteen months. For the motion of nutation itself this plasticity would result in dissipation only; but other motions with slow secular effects are made possible.

The long-term stability of the axis must then be considered. A spinning sphere would possess no stability of its axis of rotation at all; the smallest beetle walking over it would be able to change the axis of rotation relative to markings on the sphere by an arbitrarily large angle; the axis of rotation in space would change by a small angle only. What stability the earth's axis possesses against movement relative to the solid earth is derived from the geoidal shape, implying that it is spinning around the principal axis of inertia of greatest moment and hence possessing less kinetic energy than if it spun with the same angular momentum around any other axis. This complete stability against a secular change is therefore dependent upon the stability of the shape. If, as a result of a slight redistribution of mass, the instantaneous axis of rotation were to be moved by a small angle (such as is the case in the free nutation) then, if there is plastic flow, the axis of figure would slowly follow. For example, let us consider a redistribution of mass the dynamical effects of which are the same as those of an excess mass situated somewhere neither at the equator nor at the pole. The damping will assure that the initial wobble is stabilized to a spin around the new principal axis. But so long as there exists the excess mass at one place, the axis of the geoidal shape cannot be also a principal axis. Therefore, as the geoidal shape alters so as to become adjusted to the new axis of rotation, so the axis of rotation moves on. The magnitude and the angle of latitude of the excess mass define, in fact, not simply an angle of displacement of the earth's axis relative to the solid surface: they define an angle between the instantaneous axis of rotation and the axis of the geoid. The magnitude of this angle in turn defines the stress and hence the rate of flow altering the shape, given by the constants of plasticity of the earth. The resulting rate of flow gives the rate at which the axis of the geoid moves relative to markings on the earth, and that is now also the rate at which the rotational axis moves relative to the earth, though, of course, remaining nearly fixed in space. It is as with the ass and the carrot hanging from a stick held by the rider; to be more precise, as if the ass would run faster the farther the carrot was held in front of his nose. But how far can the ass be made to run by this method?

In the movement of the earth the latitude and the excess mass were decisive. The angle between the two axes and therefore the rate of polar wander will be greatest for an excess mass at a latitude of 45°; and there will be no position reached in the motion where the angle vanishes until the axes have swung around so as to make the new equator go through the position of the excess mass. The angle α between the axis of figure and the axis of rotation is proportional to $M \sin\theta \cos\theta$, where M is the excess mass and θ its latitude. The rate of deformation and hence of polar wander must increase with increasing α, but the actual relation between the quantities depends upon the details of the non-elastic properties of the material. The rate of plastic flow as well as other departures from elasticity generally increase with increasing stress faster than in proportion to the stress. If, therefore, the long-term effects were connected with the occurrence from time to time of larger angles α than can be seen in the free nutation at present, one would merely be under-estimating the amount of plastic flow if one assumed the rate to be proportional to α and given by the damping of the free nutation. The distance to which the wander will proceed is independent of M: it is simply the angle θ. One has here the situation of a secular instability where a small disturbance does not produce a small effect but where it produces a large effect slowly.

Sir George Darwin considered a motion of that sort[6] and described it qualitatively; but in summing up he considered it unlikely that the motion had, in fact, been taking place, for he would have expected it to have resulted in a change of the distribution of land and water. Geologists, however, had assured him of the permanence of the oceans. Due to the new knowledge of isostasy, this argument has by now lost all force. In the case of perfect isostasy, an ocean basin would be characterized by the occurrence of a region of greater average density below the ocean floor. The depth of any point of the basin below a certain equipotential surface such as the sea-level would be given solely by the local distribution of density, and would therefore be unaffected by a change of the geoidal shape to a new axis. The actual isostatic compensation is not perfect; but still it is known that there is a considerable degree of correspondence between the heights of the surface and the densities below it. In so far as that is true, the distribution of oceans and continents would remain unaffected by polar wander. The details of the nutation were also not available to Darwin, and the case for plastic flow was therefore weaker and lacking a quantitative estimate.

The dynamics of the problem do not introduce any long time constants, and the speed of polar wander must be entirely dictated by the rate of plastic flow. With the plasticity derived from the damping of the free nutation, we can make an estimate of the speeds involved; for the rate of movement of the axis of figure would have to be one-tenth of the angle α, or more, per annum. If a continent of the size of South America were suddenly raised by 30 metres, an angle of separation of the two axes of the order of one-hundredth of a degree would result. The plastic flow would then amount to a movement of one-thousandth of a degree per annum. The earth would hence topple over at a rate of one degree per thousand years or by a large angle in about 10^5 years. The order of magnitude of the speed of this process would therefore be preserved if the raising of a continent by 30 metres occurred not suddenly but in a time of the order of 10^5 years. This rate is perhaps higher than could be expected. A continent of that size does not rise perhaps at such a rate; and the observed degree of isostasy on the earth suggests that the rise of a mass is associated with such a redistribution of material that the actual increase of moment (in the sense of the excess mass discussed before) is very much less than corresponds to the elevation of the surface. As in this discussion the rate of tipping is proportional to the square root of the rate of elevation of mass, the estimate will not be as rough as the geological data must necessarily be. A rate of elevation of the continent by three metres in 10^6 years is certainly a low figure and would result in a time of 10^6 years for a large-angle change in the orientation of the earth relative to its axis of rotation.

This, then, is the possible effect using the plasticity suggested by the nutation. But what motion must we then expect in geological times, when there are constantly variations in the distribution of masses? It is perhaps easiest to consider this with the aid of a model of the earth which, though it may be false in other respects, possesses just the bare features required for these dynamical processes. That model would consist of a plastic spheroid representing all the earth below a hundred kilometres depth, of such plasticity that no forces of hydrostatic disequilibrium can be upheld there. On it there is floating a crust consisting of a close mosaic of pieces of different densities. This model would possess different values of the moments of inertia about different axes of rotation. Though in each case the interior will become the appropriate spheroid, the non-uniform crust will be in a different relation to it; and the blocks of the crust made of lighter materials will make a greater contribution to the moment of inertia at each latitude than the heavier ones, as they represent the same quantity of material, but at a greater distance from the axis. The secular instability that we have discussed is such as to rotate this body relative to the axis of rotation until its spin is around the axis of the greatest possible moment of inertia. Movements of material on the surface of the earth correspond to changes in the masses and mean densities of the blocks. It will therefore occur from time to time that a new distribution arises that would produce a greater moment of inertia for the body if it spun around another axis. Whether this situation must now be expected to produce a secular motion whereby the axis is continuously hunting the direction of the greatest possible moment of inertia is not immediately clear, for the movement of the axis in turn causes certain redistributions of masses of air and water, and those may be in such a sense as to help or to hinder the movement. Possibly the most important effect of that sort is circumpolar glaciation producing large and rapid variations of mass at latitudes not so far from 45°, where the dynamical effect would be a maximum, and altering the sea-level everywhere, also in intermediate latitudes. In considering the small present-day movements of the axis, Munk and Revelle[7] came to a similar conclusion and stated that the pole should be expected to move towards the location at which most ice is melting at present.

It may be, then, that instead of a passive stability of the pole due to stiffness of the shape of the earth, there is merely from time to time a stability arising from a feed-back process that balances out slight asymmetries that arise. If indeed glaciation is the dominant effect then, for example, the pole situated in a circular ocean extending over some 20° of latitude would tend to be in a stable position. Any departure of the pole from the centre of that ocean would cause glaciation on the land towards which it had moved (the ice floating on the water has no significant dynamical consequence), and such an addition of mass in that locality would act in the sense of pushing the pole away from that land. From the work of Jeffreys[1], Mintz and Munk[2] and others, it is clear that atmospheric effects may also contribute to such mechanisms.

The movements of the pole relative to the surface of the earth that have been measured in the past fifty years do not amount to much. If an estimate is made of the centre of the curves of travel of the North pole during the nutations, then it appears that this centre may have been drifting by something of the order of $0 \cdot 1''$ in the direction of Newfoundland in the past fifty years (corresponding reasonably well to the continued melting of Greenland ice)[7–9]. Such a rate of movement, even if it continued in the same direction, would thus take about 2×10^8 years to turn the earth over by 90°. There is also other evidence in past geological epochs for long intervals, of the order of fifty million years, of no drastic climatic change. On the other hand, there is ample evidence that drastic changes of climate have occurred occasionally in geological time, and such changes have served geologists for the division of time into geological epochs. It is thus tempting to suggest that there have been just a few occasions when the axis has been 'free' and has swung around as rapidly as would be given by the stiffness of the earth and the rates of tectonic movement, leading to a time-scale of the order of 10^5 or 10^6 years, but scarcely longer. But then in its rapid movement it has encountered a trap where it was caught within a small angle by the shifts in masses which its movement itself induced. It must then have remained in that neighbourhood until either there was a sufficient tectonic change to lead to an unbalance greater than could be compensated by glaciation or atmospheric effects, or until the distribution of land and water had been so modified as to destroy the mechanism of the trap. There would then have followed another period of hunting until a new trap became operative. The recent observations of fossil magnetism of rocks and sediments show such great promise that we can expect before long to have a good indication of the movements of the pole that have, in fact, occurred in geological history. It is known already that a movement of at least some regions relative to the position of the pole is indicated by the results, and this constitutes a further item of observation along with such geological items as the glaciation of now tropical zones, that force geophysicists to consider that either continental drift or polar movement must have occurred on a substantial scale. In due course the magnetic data will make the distinction between the two. The occurrence of continental drift over great distances would imply new and surprising data about the construction of the earth and in particular its crust; while the occurrence of wandering of the poles over great distances would fit in well with all that is known about the earth, and would reaffirm what can already be inferred from other data.

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SCIENTIFIC INTELLIGENCE AND DEFENCE

By CHAPMAN PINCHER

SCIENTISTS in Britain are to play a much bigger part in Intelligence work as a result of recent changes effected by the Defence Ministry. *Ad hoc* panels of scientists and technologists expert on particular matters are to be set up to help in the interpretation and collation of certain kinds of information collected through Intelligence sources.

These scientists will be drawn from the universities and from industry as well as from Government establishments. After security clearance they will be asked to assist in the analysis of information about foreign weapons, equipments and defence potentialities.

Since the War, responsibility for this type of work has rested nominally with the rather small Directorate of Scientific Intelligence in the Defence Ministry. That arrangement has proved to be impracticable. The Fighting Services have become so technical that it is no longer possible to draw a line between the routine Intelligence work for which they should be held responsible and scientific Intelligence. In fact, Intelligence chiefs have despaired of defining scientific Intelligence except in the general terms that it is the knowledge and appraisal by scientific methods of foreign developments in science and technology.

The Directorate of Scientific Intelligence has therefore ceased to exist as a separate unit. It has been absorbed into the Joint Intelligence Bureau, which, though still retaining its name, has been given much wider functions.

The Joint Intelligence Bureau will henceforth serve as a central Intelligence agency to which a wide range of strategic Intelligence information will be transmitted. Its staff will be responsible for ensuring that the utmost possible use is made of such information.

This change brings the British system more closely into line with the Intelligence methods adopted by the United States Government after the War. The failure to appreciate the imminence of the Japanese attack on the American Fleet at Pearl Harbour was later attributed to defects in the higher levels of the Intelligence system. Information clearly pointing to the attack and its approximate date was available in the various separate Intelligence agencies; but because there was no central authority it had never been properly 'appreciated' and had not been put forward at the levels where decisions are made. The U.S. Central Intelligence Agency, now headed by Mr. Allen Dulles, brother of Secretary of State John Foster Dulles, was therefore set up to ensure proper utilization of Intelligence information in the future. Its necessity has been enhanced by the growing danger of the possibly decisive influence in future wars of 'Atomic Pearl Harbours'. All Intelligence information from the Fighting Services, the Secret Service and the Atomic Energy Commission is now canalized to the U.S. Central Intelligence Agency.

The British reorganization has left the Fighting Services responsible for their own operational Intelligence work—an arrangement which is thought to be more flexible as it leaves the Services less isolated from the central agency. It is believed that the people who interpret scientific Intelligence will be more effective if they retain closer connexion with the Service in which they themselves are most expert.

Major-General Sir Kenneth Strong, former chief of the Joint Intelligence Bureau, remains head of the enlarged organization. Mr. H. S. Young, who has been acting director of scientific Intelligence since the resignation of Prof. R. V. Jones from that post last year, has been appointed a deputy director of the Joint Intelligence Bureau.

After collation by the Bureau, matters of importance will be put forward to the Joint Intelligence Committee, a high-level group which includes the heads of all the Intelligence agencies and a Foreign Office representative. This committee will produce a 'considered view' on the matters in hand and make it available to Service commanders, ministers, senior Civil servants, ambassadors and others who need it.

The enlarged Joint Intelligence Bureau is responsible to the Defence Minister, Mr. Harold Macmillan, who has a direct voice in the Cabinet. As an additional link with the Joint Intelligence Committee and the Intelligence chiefs, a post of Scientific Adviser on Intelligence to the Defence Minister has been created as part of the new arrangements. Mr. E. C. Williams, formerly director of operational research in the Admiralty, has been appointed to it.

Though these changes have been made entirely on the basis of British experience, it is believed they will cause satisfaction in Washington—an important consideration, as fears about British security have held up interchange of information on atomic weapons and other defence developments.