Discussion

J. A. O'Keefe, J. L. Worzel, R. W. Girdler and M. H. P. Bott

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XXII. Discussion

Dr J. A. O'Keefe (N.A.S.A. Goddard Space Flight Center, Greenbelt, Maryland, U.S.A.) was invited to open the discussion:

In his contribution, Professor MacDonald referred briefly to the non-hydrostatic component of the Earth's flattening, along with other non-hydrostatic terms in the gravity field. Professor Runcorn did not mention this component at all. Both employed maps of the geoid which referred to an ellipsoid with a flattening of 1/298·3—the actual flattening—and which therefore showed no effects of the second harmonic.

I should like to discuss this particular harmonic in some detail, because it is the best-established and the largest deviation from fluid equilibrium. Its most conspicuous effect is shown in figure 1. Here the solid line is the trace of a great circle on a Mercator map.
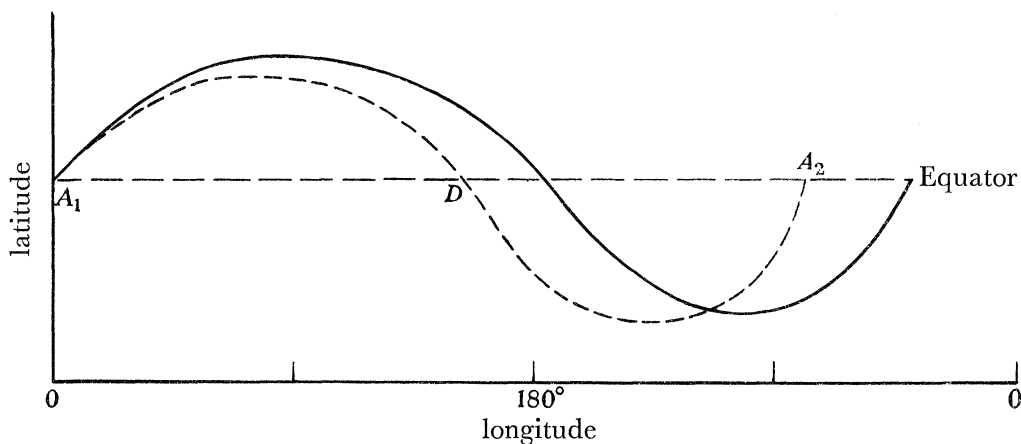

FIGURE 1

If the Earth were spherical, a satellite would pass over it on a path like this. But, because of the Earth's equatorial bulge, an actual satellite is constantly drawn toward the Equator: thus it completes its rise and fall in latitude a little too soon. The point D, the *descending node*, is thus a little short of 180° from the first *ascending node*, A_1. Similarly, the second ascending node, A_2, falls even further short of 360° as the satellite continues around its orbit. Therefore, we see that the ascending node will steadily *regress*, i.e. will fall back in a direction opposite that of the satellite's motion. The rate at which the node regresses is a very precise measure of the flattening of the earth.

The first attempt at an improvement of the constant of the flattening was made by L. Jacchia, on a Harvard Announcement Card in March 1958, from measures of Sputnik II, which was considerably disturbed by irregular atmospheric drag. By the summer of 1958 it was possible to use the American satellites with their relative freedom from atmospheric disturbances; numerous independent determinations from both Russian and American satellites, and from the U.S., the U.K., and Czechoslovakia all indicated a flattening of the Earth near to or a little over 1/298. All subsequent measures have confirmed that the flattening is between 1/298·2 and 1/298·3, very close to the value adopted by the Russians

for their Krassowsky Ellipsoid, and decisively different from the value of 1/297 adopted for the International Ellipsoid or, more surprisingly, from the value of 1/297·3 which was then regarded as representing the figure of fluid equilibrium.

It was now necessary to make a new calculation of the figure of fluid equilibrium. This was because the old calculation, which gave 1/297·3 for the flattening, involved as an essential step the assumption that the actual flattening was equal to the flattening for fluid equilibrium; and this was plainly no longer good enough. We begin with the fact that the effect of the Earth on a satellite is proportional to the quantity $C-A$, where C is the moment of inertia of the Earth around its polar axis, while A is its moment of inertia around an equatorial axis. To get it into convenient units, we divide by Ma^2, the product of the Earth's mass by the square of its radius. It is proved in such texts as Jeffreys's *The Earth* that (to the first order)

$$\frac{C-A}{Ma^2} = \tfrac{2}{3}(f - \tfrac{1}{2}m), \tag{1}$$

where m is the ratio of centrifugal force at the Equator to gravity at the Equator, and f is the actual flattening. This equation is a purely mathematical one; it does not depend in any way on assumptions about the internal constitution of the Earth; at the present time it (with high-order terms) is the working definition of the Earth's actual ellipticity, and it is from this equation, with slight modifications, that the above values of the ellipticity were found.

Numerically, $(C-A)/Ma^2 = 0·001083$.

The same equation applies to the motion of the node of the Moon's orbit; in this case, however, the direct effect is masked by similar and much larger effects of the Sun. On the other hand, the Moon is large enough so that its reaction on the Earth, which is, of course, also proportional to $C-A$, is significant. There is a similar solar effect; the combined torque is responsible for the precessional motion of the axis of the Earth. As usual, the rate of precession is proportional to the applied torque and inversely proportional to the angular momentum, $C\omega$, where ω is the Earth's angular velocity; thus from a measure of the lunisolar precession we obtain the quantity

$$H = (C-A)/C.$$

The value is approximately 1/305·6. Dividing, we have

$$\frac{C-A}{Ma^2} \Big/ \frac{C-A}{C} = \frac{C}{Ma^2},$$

for which A. H. Cook finds the numerical value 0·3306.

From these values it is possible to calculate what the flattening ϵ of the Earth *would be*, if the Earth were in a state of fluid equilibrium. The equations, which result from two centuries of mathematical study are, to the first order, in Jeffreys's notation

$$C/Ma^2 = \tfrac{2}{3}\{1 - \tfrac{2}{5}\surd(1 + \eta a)\},$$

where

$$\eta a = \tfrac{5}{2}(m/\epsilon) - 2.$$

Henriksen solved the second-order equivalents of these equations; it turns out that they differ from the first-order equations only to the extent of about 0·1 in the reciprocal; the result is

$$\epsilon = 1/299·8.$$

This is the flattening which the Earth *would have*, if it were in hydrostatic equilibrium. We have found it by using the numerical values of $(C-A)/Ma^2$ and H plus equations (4) and (5). Prior to 1958, reliable values of $(C-A)/Ma^2$ were not available; hence the necessary fourth equation was obtained by assuming that $\epsilon = f$; this method led to the value $1/297 \cdot 3$ for f, the so-called hydrostatic value.

The load on the equator which is represented by the excess bulge ($1/298 \cdot 3$ instead of $1/299 \cdot 8$) is equivalent to 200 m of land elevation. The mechanical consequences can best be presented by analogy with a pontoon bridge. A single, detached boat will displace its own weight of water; thus

FIGURE 2

Now the Earth is like a pontoon bridge, extending from the north pole to the south pole, thus:

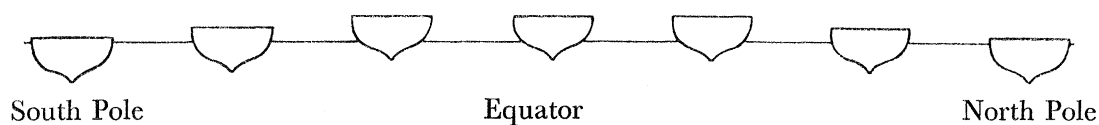South Pole   Equator   North Pole

FIGURE 3

Near the poles there is less mass than would be expected; the boats are displacing more than their weight of water, and are lifting. Near the Equator, the boats are displacing less than their weight of water; the total mass per square centimetre (boat plus water) is thus more than expected.

Clearly this is only possible if there is some mechanical tie between the boats, capable of carrying the load from the Equator to the poles.

In the case of the Earth, we find that the hydrostatic assumption does not do the trick; the equilibrium is either not hydro or not static. If it is ordinary solid static equilibrium (something like a beam to which all the boats are connected, as Jeffreys has suggested, then clearly we cannot have convection currents in the mantle.

The alternative is hydrodynamic support of some kind. Since the currents contemplated are very slow, we can exclude the question of support by the inertia of the stream and we must think in terms of the forces of viscosity.

The mere passive resistance of the kind of viscous forces generally adduced by exponents of convection theory is insufficient. The prime example generally used is the uplift of Fenno–Scandia. This, if interpreted as the result of viscosity in the mantle, leads to a value of 10^{22} P. Collapse would ensue in a time less than the characteristic time for the Fenno–Scandian uplift of, say, 6000 y. The consequence would be changes of sea level over very large parts of the world at the rate of a foot every few years, which is too large by orders of magnitude.

Hence the distortion must be maintained by dynamic effects, i.e. by some sort of convection currents which produce forces which raise the equator as fast as it collapses. I should think that these currents would rise under the Equator and flow toward the poles, where they would sink; and Licht has worked out this idea. Others, I am told, feel that the currents would flow the other way.

In any case, if mantle-deep convection currents really exist, and if they drag the continents around on their backs, then the continents should be stacked up either at the Equator or at the poles.

Professor J. L. Worzel (Lamont Geological Observatory, Columbia University):

There is a disagreement between the sea surface gravity data and the geoid determined by satellite data as shown by both Runcorn and O'Keefe. These latter show a deep depression of the geoid in the eastern North Pacific. If such a depression occurred the surface pendulum data would show this to be a region of large negative anomalies, which it is not.

It is difficult to see how the pendulum data can be in error since the observations were made on a number of different cruises and the land connexions on both ends of each passage agree within a couple of milligals with the land values. Therefore it must be considered that the satellite data may be in error, perhaps because of the distribution of the orbital observation points, or that an insufficient number of harmonics have been included in the analysis.

Dr R. W. Girdler (University of Newcastle) wondered how Dr MacDonald could explain the world-wide predominance of transcurrent faults in the light of his concept of the rigid nature of the upper mantle. He noted that we have to account for horizontal displacements of the order of 10^3 km and it is hard to find a driving force for causing these without invoking convection.

Further, Dr MacDonald's concept that continental structure extends for 500 km seems surprising in view of evidence from isostasy. Estimates for the depth of compensation have ranged from 30 to 120 km but never much more and these seem satisfactory.

Dr M. H. P. Bott (University of Durham) explained a new hypothesis for the mechanism of continental drift depending on mantle convection. The hypothesis assumes that the continental crust contains a sufficiently great content of radioactive materials to contribute significantly to terrestial heat flow in continental regions; if correct, the continental crust is less efficient than the oceanic crust as a sink for heat transported through the mantle by convection. Thus if other factors are equal convection should occur preferentially in the mantle beneath oceans. The pattern of terrestial heat flow suggests that this is the case at the present time.

If convection is normally absent beneath continents then the temperatures in the underlying mantle should rise progressively due to radioactive heating (by the order of 100 degC in 10^9 y if reasonable assumptions are made). Eventually subcontinental convection may begin. This may cause continents to be moved onto portions of the mantle which previously underlay oceanic crust, since the creation of new oceanic crust is the most efficient way of disposing of the convected heat provided the process can be initiated. This process may involve the splitting of a continental mass.

An account of the hypothesis has recently been published (*Nature, Lond.*, **202**, 583–584).