CONSTANTS RELATED TO THE EARTH AND MOON

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RÉSUMÉ. — L'auteur discute diverses déterminations des coefficients du développement du champ gravitationnel terrestre en harmoniques sphériques ainsi que les valeurs de la parallaxe solaire et des constantes liées à la figure et au mouvement de la Lune.

ABSTRACT. — The author discusses various determinations of zonal and tesseral harmonics of the Earth's gravitational field, the values of the solar parallax, and the constants related to the figure of the Moon and its motion.

ZUSAMMENFASSUNG. — Verf. diskutiert verschiedene Bestimmungen der zonalen und tesseralen Harmonischen des Gravitationsfelds der Erde, die Werte der Sonnenparallaxe und die Konstanten, die sich auf die Figur und die Bewegung des Mondes beziehen.

Резюме. — Автор изучает различные определения коэффициентов разложения гравитационного поля Земли по сферическим функциям, солнечного параллакса и постоянных связанных с фигурой Луны и её движением.

I have given (Jeffreys [1]) a solution combining the data concerning the figures, masses and sizes of the Earth and Moon into a consistent system. The principal changes from previous standards were that the Earth's equatorial radius was changed from Hayford's value, 6 378.388 km, to 6 378.099 ± 0.116 km, and the mass ratio from 81.53 to 81.278 ± 0.025. These and other results were reported to the Paris Symposium in 1950.

Nearly every datum used in the calculation has since been revised, but the results have not yet been combined; and it seems practically certain that any attempt to combine them will be out of date before it is published.

Low harmonics in the Earth's gravitational field. — My analysis [2] of gravity was based on free-air reductions. The residuals against a standard formula were found to be correlated with height, and a linear approximation by least squares was used to reduce to the mean height

over a " square " bounded by lines of latitude and longitude 10° apart. The differences between neighbouring 10° squares were found to be greater than the apparent uncertainties would explain, and I included an additional variation of standard error τ_1 affecting the whole of a 10° square. After allowance for this, means and standard errors were found for 30° squares. Again the differences were too great, and a τ_2 variation affecting the whole of a 30° square was assumed. Finally, the results were analysed for harmonics up to degree 3. Apart from the constant and the main ellipticity term, four harmonics containing longitude factors were found apparently significant, though they had standard errors of 1/3 to 1/2 the estimates.

Another analysis was carried out by I. D. Zhongolovitch [3]. He used a reduction to mean height, but did not allow for τ_1 and τ_2, and he treated all his 10° squares (which were more nearly square than mine) as of equal weight. His results differ considerably from mine, though the data were much the same, and I think the differences must arise mainly from his treating all squares as of equal weight irrespective of the number and consistency of the observations within them. This must have overweighted the scanty data south of 30°S. I have discussed this further elsewhere [4].

The gravity data have been much extended since these analyses, but the methods of analysis used seem to me unsatisfactory. In particular, isostatic reductions, sometimes used, must systematically change the external field. Kaula is engaged on what should prove to be a better one, but I am not convinced that any *great* improvement on the method I used in 1943 is possible. The position has been considerably changed by the information given by perturbations of artificial satellites. My analysis gave the ellipticity $\frac{1}{296.2 \pm 0.7}$; the international value based on Hayford is $\frac{1}{297.0}$. But the study of early artificial satellites by King-Hele derived about $\frac{1}{298.2}$ from the motions of the nodes, and this has been fully confirmed by later work. Then O'Keefe found a significant systematic difference of eccentricity according as the perigee was north or south of the equator, and attributed it to a term in $\frac{P_3}{r^3}$ in the potential. I had not found this term but it is within my uncertainty. Zhongolovitch's estimate is much larger.

It is much more difficult to detect effects of longitude terms on artificial satellites, since they necessarily cancel out in periods of the order of a day, whereas those of zonal harmonics accumulate for months. Results have nevertheless been obtained, especially by Kozai [5].

Kozai has sent me seven solutions based on different satellites. He says that he did not carry out a complete least-squares solution; if I understand

him correctly, his solutions are the first cycle in a relaxation solution. This would tend to underestimate the coefficients and the standard errors.

However, as the data are independent we can get a check by comparing them. It is clear that the stated uncertainties are too low; but means and revised standard errors are as follows for $10^8(C_{nm}, S_{nm})$ with normalized harmonics. All are, of course, on 6 degrees of freedom.

C_{22}	$+ 99 \pm 7$	C_{41}	$- 34 \pm 1$
S_{22}	$- 80 \pm 13$	S_{41}	$- 48 \pm 3$
C_{31}	$+142 \pm 13$	C_{42}	$- 18 \pm 2$
S_{31}	$+ 2 \pm 4$	S_{42}	$+ 64 \pm 8$
C_{32}	$+ 37 \pm 3$	C_{43}	$+ 44 \pm 5$
S_{32}	$- 39 \pm 6$	S_{43}	$+ 26 \pm 5$
C_{33}	$- 4 \pm 8$	C_{44}	$+ 78 \pm 6$
S_{33}	$+ 64 \pm 7$	S_{44}	$+145 \pm 11$

It is interesting that the four terms that I found from gravity, C_{22}, C_{31}, C_{32} and S_{32}, stand out, but my analysis did not reveal any of the others. I am surprised that C_{44} and S_{44} should be detectable. Since the solutions are incomplete it is probable that closer analysis would increase them and their standard errors, possibly by factors of 1.5 to 2.

Kaula has made two further solutions, giving coefficients up to C_{64}, S_{64} and C_{70}, [6]. None of the higher ones is conspicuous. He has given great attention to possible systematic errors and I think that his standard errors are genuine.

R. R. Newton (unpublished) has made an analysis for the satellite *Transit* 4A. Terms in $P_{22}(\cos 2\lambda, \sin 2\lambda)$ would produce a small semi-diurnal perturbation along the track on such a satellite, $P_{41}(\cos\lambda, \sin\lambda)$ a diurnal one, other harmonics having little effect. Thus a good separation of these harmonics should be possible. Results are, in the standard notation,

$$10^6 C_{22} = \quad 2.1 \pm 0.4, \qquad 10^6 S_{22} = -0.8 \pm 0.4,$$
$$10^6 C_{41} = -2.5 \pm 0.5, \qquad 10^6 S_{41} = -0.4 \pm 0.5,$$

or, in normalized notation,

$$10^8 C_{22} = \quad 130 \pm 26, \qquad 10^8 S_{22} = -52 \pm 26,$$
$$10^8 C_{41} = -241 \pm 48, \qquad 10^8 S_{41} = -38 \pm 48.$$

The difference in C_{41} from the result based on Kozai is serious.

If we had four observations of gravity for every 10^0 square over the Earth's surface, we could estimate any normalized coefficient to about 10^{-6} of mean gravity. The apparent uncertainties of determinations from artificial satellites suggest that even up to P_{44} we can already do much better than this; but the differences between different determinations

still leave this in doubt. I see no reason to doubt the estimates of the zonal harmonics; the doubt is about the longitude terms. The results from gravity differ from those for artificial satellites even for the main ellipticity term, for which I am confident that the artificial satellite solution is right. The difference probably arises in part from errors in the comparison of base stations, which have now been largely corrected, and in part from inadequate data from high southern latitudes.

The principal difficulty about the analysis of observations of artificial satellites is air resistance, which introduces additional unknowns depending on the distribution of density with height; this may also have solar diurnal components. Fortunately, for small eccentricities the effects are nearly independent of those of the additional terms in the potential; much valuable information has been derived on the density. The latest results of King-Hele [7] for the terms of even degree are (with $J_n = -C_{n0}$)

$$10^6 J_2 = 1082.78 \pm 0.05, \quad 10^6 J_4 = -0.78 \pm 0.2,$$
$$10^6 J_6 = 0.70 \pm 0.1, \quad 10^6 J_8 = 0.24 \pm 0.2,$$
$$10^6 J_{10} = -0.50 \pm 0.2, \quad 10^6 J_{12} = 0.28 \pm 0.2.$$

If only the first three are retained, the solution is

$$10^6 J_2 = 1082.79 \pm 0.05, \quad 10^6 J_4 = -1.09 \pm 0.20, \quad 10^6 J_6 = 0.73 \pm 0.20.$$

For odd harmonics King-Hele [8] has given summaries based on several different analyses :

$$10^6 J_3 = -2.4 \pm 0.1, \quad 10^6 J_5 = -0.2 \pm 0.2, \quad 10^6 J_7 = -0.45 \pm 0.1.$$

More details will be given in a paper in the *Geophysical Journal.*

Only J_2 and J_4 could exist on the hydrostatic theory of the figure of the Earth. Further, it is now possible to proceed as follows.

The P_2 term in the gravitational field determines the ratio $\frac{C-A}{Ma^2}$; the precessional constant determines $\frac{C-A}{C}$. Then division gives $\frac{C}{Ma^2}$, which can now be taken as a known quantity. Using it we can work out what the ellipticity would be on a hydrostatic theory for the actual rate of rotation. S. W. Henriksen has done this and gets $\frac{1}{300.0}$. I get $\frac{1}{299.67 \pm 0.05}$. The most direct comparison is that J_2 on the hydrostatic theory would be 0.0010721 ± 0.0000004. Thus the Earth is more elliptic than on a hydrostatic theory. It should be remarked however that de Sitter's hydrostatic theory contains several errors in the second order terms (Bullard [9], Jeffreys [10], Message [11]). It is not clear whether Henriksen has corrected theese.

There is considerable variation in notation for the external potential. The standard form is

$$U = \frac{\mu}{r}\left\{1 + \sum\sum\left(\frac{a}{r}\right)^n P_n^s (C_{ns}\cos s\lambda + S_{ns}\sin s\lambda)\right\}$$

but this has the peculiarity that the mean squares of the various harmonics vary enormously with s, and statements of their coefficients give practically no idea of relative importance. My wife and I suggested [12]

$$p_n^s = \frac{(n-s)!}{n!} P_n^s,$$

which greatly reduces but does not remove the variation of the mean square with s. This also simplifies the relation to Bessel functions; for given $n\theta$, as $n \to \infty$, $p_n^s(\cos\theta) \to J_s(n\sin\theta)$. A resolution proposed by Hori, and adopted at Berkeley, was that factors should be introduced so as to make the mean squares of all the P_{nm} the same as for P_n, namely $\frac{1}{2n+1}$. It does not appear, however, to have been used yet in any published work. Kaula has used a complete normalization, making the mean square of every harmonic equal to 1.

I still think myself that most of the advantages are with p_n^s. The complicated square roots introduced by normalization will make theoretical work very difficult and increase the difficulties of tabulation; and the relation between the potential and gravity introduces a factor $n-1$, so that normalization for different n will not hold for both.

The factor in λ is often expressed in the form $A_{ns}\cos s(\lambda - \lambda_0)$. Then λ_0 is ambiguous since the expression is unaltered if it is increased by $\frac{2\pi}{s}$. In practice the value with least modulus is used, and this produces a spurious apparent concentration about zero.

The radius of the Earth. — Knowledge of gravity or the external potential can settle the form of the ocean surface and of level surfaces outside it. It does not determine the actual size. Since my work the United States survey has been extended to Alaska and Chile, and connexion has been established between the European and South African meridian arcs; and measures are now available between Manchuria and Japan. These have been discussed by Irene Fischer of the U. S. Army Map Service. Unfortunately, some of the data used have not been published. Discussions between mine and hers have given values of the equatorial radius between Hayford's and mine. Fischer's " World Datum ", 6 378.166 km (no uncertainty stated) agrees with mine within my standard error. Kaula gets 6 378.163 ± 0.021 but

thinks that the real uncertainty may be 0.04. However, I am still not altogether satisfied. Triangulation gives distances; the curvature disclosed by observations of stars gives angles between normals to the level surfaces. Comparison gives the size of the Earth. But for modern accuracy it is necessary to allow for departures of the level surfaces from a spheroid of revolution. I did this in my study, but at that time allowance for the low harmonics hardly affected the comparison between different arcs.

I have suggested [13] that a modification of Stokes's formula may be useful. If free-air gravity is known everywhere, this formula gives the elevation of the co-geoid above whatever spheroid is used for comparison. But data are too incomplete, and if they were complete the reduction would only reproduce the trial spheroid. But if the formula is modified by the omission of harmonics of degrees $\leqq 4$ it will be much less affected by uncertainties for distant zones, and leave harmonics of these degrees unaltered. Then comparison of survey areas can supply additional evidence about the low harmonics. I think that the state of the subject is such that we cannot afford to neglect any possible source of information.

Fischer used a method of Molodensky. In triangulation the standard of level is the spirit level, and ordinary survey gives heights above a level surface, the co-geoid. But the theodolite can also give a complete three-dimensional survey, and with modern accuracy the errors over long arcs accumulate to less than the elevations of the geoid. Hence it is possible to get the actual form of the geoid along a survey arc and choose the spheroid that fits it best. My opinion is, however, that we cannot assume that other low harmonics will not introduce errors in this procedure, and that we should try to reach some approach to agreement about them before we adopt any new value for the radius.

A major difficulty in long-distance surveys is refraction. This does not affect horizontal distances much, but can accumulate considerably in the vertical. Astronomical observations compare directions of fixed stars with the level surfaces and are much less affected, but are affected by local irregularities of the level surfaces.

A question that is becoming of increasing importance is whether the size of the Earth should continue to be stated in terms of the equatorial radius. De Sitter showed that there are theoretical advantages in development of the theory of the second-order terms if we use the mean radius. I found in my theory of the figure of Saturn that he had really not gone far enough, since with a slight redefinition of the ellipticity the equation for it was quite independent of the fourth harmonic. It has also been emphasized by many writers that the majority of the observations are for temperate latitudes, and interpolation to mean latitude implies less error than extrapolation to the equator and poles.

Thus standard values used would be less affected by possible errors in the ellipticity. This is true for most of the constants. It has not always been true for the radius. For surveys along the meridians and parallels in intermediate latitudes the ellipticity affects the estimated equatorial radius in opposite ways; this is why Hayford was able to get estimates for both a and e from observations within the United States. In my analysis, incorporating several different surveys, I found a and e nearly independent. The two long meridian arcs seem to have altered this position, and it may now be better to use the mean radius after all.

There are, however, several different possible definitions of the mean radius. Given a particular ellipsoid, it might be chosen to be :

(1) the radius of a sphere of equal volume,

$$a(1-e)^{\frac{1}{3}} = a\left(1 - \tfrac{1}{3}e - \tfrac{1}{9}e^2\right);$$

(2) the arithmetic mean of three orthogonal radii, $a\left(1 - \tfrac{1}{3}e\right)$;

(3) the radius where $P_2(\sin\varphi)$ vanishes, where φ is the geographical latitude; this is $a\left(1 - \tfrac{1}{3}e + \tfrac{5}{9}e^2\right)$;

(4) the radius where $P_2(\sin\varphi')$ vanishes, where φ' is the geocentric latitude; this is $a\left(1 - \tfrac{1}{3}e - \tfrac{1}{3}e^2\right)$;

(5) the mean value of r over the whole surface, taken with respect to $\cos\varphi'\,d\varphi'$; this is $a\left(1 - \tfrac{1}{3}e - \tfrac{1}{5}e^2\right)$.

Hitherto the second-order differences have not mattered, but ae^2 is about 30 m, and comparable with the present uncertainty.

The solar parallax and the lunar inequality. — My value for the lunar inequality [14] was $6''.4378 \pm 0''.0018$ (s. e.), a revision of Spencer Jones's from the 1931 Eros data. Later determinations, also from Eros, are by Rabe [15] and Delano [16]. As the data, especially those most directly relevant, are largely the same, the differences arise from the statistical methods used and not from the observations. In my treatment the data from intervals between consecutive zeros of the inequality varied by more than randon error seemed to explain, and I adopted the hypothesis that the anomalies were in four parts (1) the lunar inequality itself (2) a part that varied slowly, keeping the same sign for several months (3) a part that could affect approximately a fortnight in the same sense, but could be treated as random for different fortnights (4) truly random error. The hypothesis makes no assumption about the explanation of (2) and (3), but obviously errors

in the ephemeris and time-keeping could contribute largely to (2) and errors in star places to (3). I understand that it has since been argued that errors in star places are too small to account for (3), but even so the variation under (3) existed and implied that errors at short intervals of time were positively correlated, and would lead to an underestimate of uncertainty if this was not allowed for.

E. Rabe began by reducing to Universal Time, and used observations from 1926 to 1945. His main object was redetermination of planetary masses, but after this he classified the residuals at 10-day intervals and analysed for the correction to the lunar inequality. His value is $6''.4356 \pm 0.0028$, nearly the same as mine. His treatment would presumably clear out variations of type (2) above; on the other hand the use of 10-day intervals instead of intervals between zeros of the inequality would lose some weight.

E. Delano did not use universal time, and used only the 1930-1931 opposition. Consequently some of his orbital elements must be badly determined. His unknowns are the orbital elements of Eros, the Earth's mean longitude, and the lunar inequality. He first computes to zero dates of the lunar equation, and then finds seven unknowns. One of them is badly determined, and he gives two trial values for it. The results, however, seem to disagree with Rabe wherever comparison is possible. His results for L on his two hypotheses are

$$6''.4428 \pm 0.0014; \quad 6''.4430 \pm 0.0017.$$

He appears to have applied no check for consistency between different fortnights, and in view of the close agreement between Spencer Jones, Rabe and me, I think there must be something wrong.

To get the mass of the Moon we need also the solar parallax. When my 1948 solution was made Spencer Jones's value $8''.7888 \pm 0''.0011$ appeared the best. (I recalculated to two figures in the uncertainty.) But Rabe's work included a determination of $\frac{E+M}{S}$, which, combined with the lunar parallax, implied a parallax of $8''.79835 \pm 0''.00039$. This is a strong disagreement, and what appear to be the more reliable determinations by other methods, notably the Doppler effect, supported Rabe. However, a recent determination of the distance of Venus by radar, reported at Berkeley, gives $8''.795$, thus returning part of the way toward Spencer Jones's value.

Atkinson has suggested that flexure may have introduced systematic error in Spencer Jones's parallax but has not published his argument. Rabe's apparent accuracy for all the masses was so great as to invite suspicion, and I made a very rough test [17] by comparing means of three consecutive residuals with the standard deviation to test possible persistence of errors. They varied a little more than expectation on

the hypothesis of complete randomness, but not enough to give evidence against it. In any case there was no ground for multiplying the uncertainties by more than about 1.5, and this would not make the result consistent with the radar value. There may, however be a systematic error. Newcomb remarked that, though another planet perturbs the Earth, and its mass can be estimated by observing parallactic effects on other planets, it cannot be found by observing the planet itself. The perturbations tend to repeat themselves at equal differences of longitude, and thus are liable to be mixed up with seasonal systematic errors and those depending on differences of illumination. Now in the case of an asteroid observed only near opposition and consequently gibbous, the centre of the illuminated area would be displaced from the true centre, in opposite directions before and after opposition, and the difference would be strongly correlated with the perturbation in longitude due to the Earth. In that case, while Rabe's masses of Mercury, Venus and Mars might be valid, there is just a possibility that that of Earth + Moon is not so accurate as appears. At least I think that the question should be asked.

With my own value of L, a compromise between the visual and dynamical parallaxes of the Moon, and Spencer Jones's solar parallax, the mass ratio $\frac{\text{Earth}}{\text{Moon}} = 81.178 \pm 0.025$, and the sine of the lunar parallax $3'22''.419 \pm 0''.024$. With Rabe's parallax the mass ratio becomes 81.356 with a somewhat smaller uncertainty. (Jeffreys and Vicente [18]). Recalculating, I get 81.299 ± 0.022. With the radar value it would be about 81.32.

The mean distance of the Moon according to my solution would be 384 400 ± 6 km. Again there are new data. The time of a reflected radio wave has been determined by Yaplee and others. O'Keefe and Anderson observed four occultations at nine stations in the U. S. A., the distances between which were known by direct measurement. Fischer [19], knowing now the actual distance between Greenwich and the Cape, uses the visual parallax directly. She makes some corrections to all the data, and concludes that all the results agree with 384 401 ± 1 km. The Gill-Christie determination gives 384 415 km, but the standard error of this cannot be put below 10 km. Fischer states that it is inconsistent, but does not mention the uncertainty. It appears that the distance of the Moon is now relatively better known than the radius of the Earth. Combining the distance of the Moon with a dynamical parallax (which depends on the mass of the Moon) gives for the equatorial radius 6 378.155 (Spencer Jones's solution), 6 378.205 (Rabe's solution) or 6 378.132 km (Delano's solution). I think the value based on Rabe's solution is to be preferred. Its own uncertainty depends about equally on the Moon's distance and the

dynamical parallax, and is probably about 1 part in 200 000 or 0.032 km. It is, in any case, within my uncertainty.

I have not tried in the above to evaluate standard errors precisely; as stated above, and especially in view of the uncertainty of allowance for elevation of the geoid, I think that would be premature.

Figure of the Moon. — The constants α, β, γ are defined by

$$\alpha = \frac{C - B}{A}, \qquad \beta = \frac{C - A}{B}, \qquad \gamma = \frac{B - C}{C},$$

where $\beta = \alpha + \gamma$. The inclination of the Moon's axis to the ecliptic gives β with a very small correction and the libration in longitude gives γ. It is usual to define

$$f = \frac{\alpha}{\beta} = 1 - \frac{\gamma}{\beta}.$$

I think that the introduction of f leads to unnecessary complications and that it should be dropped. The observations lead to estimates of β and γ nearly independently, and the ratio mixes up the uncertainties. Also calculating it requires a standard value of β, and it is not always clear what standard is being used.

Many series of observations have been reduced to determine β and γ. Those for β still differ by far more than the apparent uncertainties. A summary, taking account of the discordances, is 0.0006279 ± 0.0000015, but the true uncertainty may be four times as great (Jeffreys [20]). The data are displacements in latitude of a crater near the centre of the disk. Watts has introduced a new method, in which the Moon is photographed against a grid, and effectively rotations about the line of sight can be measured; this ought to be less subject to personal and systematic errors, but it needs further test.

γ has been determined from the annual libration in longitude. Until recently the values of $\frac{\gamma}{\beta}$ found have clustered about 0.5 and 0.2, with apparent standard errors about 0.05. There has been a great change recently. Both the latitude and the longitude of the crater should be subject to free librations, which had never been detected. But Yakovkin [21] detected what he interpreted as a free libration with a period near three years. If this right it leads to

$$\gamma = 0.0002098 \pm 0.0000022 \quad \text{(Jeffreys [22])}.$$

However Koziel had argued that there is theoretically a term in the Moon's longitude with argument $2(\varpi - \Omega)$, which would have nearly

this period. Inspecting Yakovkin's data I found that this agrees well in period, which might be an accident, but it also agrees in phase, which would require another accident, since a free vibration might have any phase. Thus it is very likely that the Yakovkin term is another forced libration. If so, it gives a much closer estimate of γ, namely $0.00^{\prime}02049 \pm 0.0000009$. f would be near 0.67, $\frac{\gamma}{\beta}$ near 0.33.

Now, though workers in the subject seem to have some reluctance to accept Yakovkin's term as genuine, there has been a great change in the results derived from the annual term. They now cluster in the range $f = 0.6$ to 0.7, and are reasonably consistent in comparison with their stated uncertainties.

What I wish to emphasize is that if there is any possibility of the existence of a term with period close to 3 years, the data should be analysed in such a way that the estimates of the 1-year and 3-year terms should not bias each other. The data should be grouped in 3-year intervals and each interval should be analysed to estimate both terms. Then phases in different intervals can be used to improve estimates of the 3-year period. The annual and 3-year terms will give nearly independent estimates of the values of γ.

The secular motions of the Moon's node and perigee. — These are produced chiefly by the Sun but are affected by the figures of the Earth and Moon. There is good reason to believe that the Moon is nearly homogeneous and that the ratios $\frac{C - A}{M a^2}$ and $\frac{B - A}{M a^2}$, which enter into the effects, can be calculated from β and γ. Without this assumption de Sitter tried to estimate $\frac{C}{M a^2}$ from the secular motions, and found that it was greater than for a homogeneous body and approached the value for a spherical shell.

The secular motions are anomalous in the respect that the surviving uncertainty in the calculation of the solar effect was greater than that in the observed values. Using the latest values of the data for the figures I found [20] that the differences between the theoretical and observed values were in the neighbourhood of de Sitter's guesses about the surviving errors. However, it has long been desirable that Brown's theory should be extended to bring the uncertainty of the calculation down to that of observation. (A resolution to this effect was passed in 1950.) W. J. Eckert reported at Berkeley in 1961 that he had succeeded in doing so. The perigee is now in good agreement, but a discrepancy remains in the node. A rough examination, however, made me think that this can be explained by an error in the secular change of the obliquity of the ecliptic, for which there is other evidence. I understand

that Eckert does not accept this, but so far he has not published his results.

Nutation and variation of latitude. — J. Jackson pointed out in 1930 that the observed amplitude of the 19-yearly notation does not agree with that calculated from the rate of precession and the lunar inequality. Following a suggestion of Bondi and Lyttleton, I examined whether the fluidity of the Earth's core could account for it; it overdid it — with the simplest model, of a rigid shell and a homogenous liquid core, the effect was three times too great. R. O. Vicente and I [18] used two better models. Both used an elastic shell based on one of Bullen's models, the theory for which had been worked out by Takeuchi. For the core we used two models. It is desirable to allow for both compressibility and for a discontinuity of density at the inner core boundary, but the analysis for them seemed prohibitive and we made two solutions, one for an incompressible core with a central particle, the other of a single compressible material. Both were adjusted to make the mass and moment of inertia agree with Bullen's model. Solutions were made for 19-yearly, semi-annual and fortnightly nutations. The Love numbers for the correcponding bodily tides were calculated. The amplitudes are less than for the rigid Earth for the 19-yearly terms, greater for the others; and in all cases the nutations in obliquity and longitude are altered in different ratios.

While this work was in progress, E. P. Fedorov [23] completed his analysis of 35 years' data from the International Latitude Service. This study is incomparably more detailed than any previous one. In particular, the nutations in obliquity and longitude are estimated separately for the first time, without assumption about their ratio.

A translation into English by my wife [24] has been published by the Pergamon Press. The observed nutation is found to be less than Newcomb's value. The new observed values have been compared with the theoretical ones (Jeffreys [25]). The agreement is much better than in previous comparisons, but still not quite satisfactory. In particular Fedorov's 19-yearly motions in obliquity and longitude are still practically in the ratio for a rigid Earth. One of our models agreed for one component, the other for the other. I think that some error must have survived, because the elliptic motion can be regarded as the resultant of two circular ones, whose amplitudes would be altered in opposite ratios if altered at all. This seems a necessary property of any model. Thus any explanation of a reduction of amplitude in one component should imply a different reduction in the other.

After a reasonable allowance for the effect of oceanic tides, the period of the free nutation was consistent with observation.

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