usually rather young, who take a sort of perverse joy in pessimistic visions of the futility of life, and in the contemplation of the unlovelier characteristics of the weed, a shallow and sophomoric epicureanism. But why waste one's time in exaggerating or gloating on the unloveliness of the weed? Even the weed has its hour of charm. There is a moment at which even the weed flowers. And then is it not the weed with its modest blossom that time, the mind of the botanist and the hand of the gardener have transformed into the perfect flower? What if the botanist and the gardener in the beginning had been content with pessimistic or cynical contemplation of the unloveliness of the weed?

Consider the world of the middle ages and the renaissance. What remains to-day? Is it the picture of the sordid ignorance and vice and eternal discord of the population? Or is it rather the lofty naves and domes and graceful spires, the glittering jewels of Chartres, the tombs of the Medici, the harmonies of the painter's art? The weeds are long forgotten; the flowers remain, more radiant and more lovely in the tender light of receding years. It is the flower that counts. Is it not our function to feed and nourish and transform the modest and transient blossom of the weed into the more perfect flower? And if our neighbor choose to devote himself to the contemplation of weeds, and close his eyes to the flowers; if he choose to dwell upon the unloveliness of the weed rather than upon *its* flower; if he be blind to the circumstance that in its modest and blundering way even the weed is seeking for beauty, let us not be annoyed. So, somehow or other, in a devious way, is our perverse friend. It is the flower that counts. "Cultivons notre jardin."

W. S. THAYER

BALTIMORE, MD.

THE FIGURE OF THE EARTH AND THE NEW INTERNATIONAL ELLIPSOID OF REFERENCE

AT the meeting of the International Geodetic and Geophysical Union, held October, 1924, at Madrid, the section of geodesy of that union adopted a so-called international ellipsoid of reference, that is, it adopted certain parameters defining an ellipsoid of revolution which, among all such possible ellipsoids, was believed to represent the best, or perhaps merely the most convenient, approximation to the actual figure of the earth. The figure which this ellipsoid of revolution is intended to represent is not, of course, that of the actual physical surface of the earth, but the ideal *geoid,* an equipotential surface which coincides approximately with the surface of the ocean as far as the latter extends and which would exactly thus coincide if the disturbing effects of winds, differences of temperature, barometric pressure, etc., were removed. The geoid is continued in imagination under the continents and could be defined physically at any point by digging a very small sea level canal connecting that point with the ocean. The water in such a canal would rise to the level of the geoid.

Several questions arise in connection with this vote of the section of geodesy, among which might be mentioned: (1) Exactly what ellipsoid was adopted? For on this point it happens that some degree of misunderstanding is possible. (2) What was the purpose in adopting this international ellipsoid? (3) How closely does it represent the actual geoid? It is the purpose of this article to give some sort of answer to these questions and also to give for reference a few numerical magnitudes derived from the fundamental parameters adopted by the section of geodesy.

The two parameters defining the ellipsoid of revolution adopted by the section of geodesy are:

Semi-major axis (equatorial radius) = 6 378 388 meters
Ellipticity (flattening) = 1/297.

These figures are those deduced by Hayford[1] in 1909 from the deflections of the vertical then available in the United States, these deflections being corrected for topography and isostatic compensation. This recognition of the importance of isostatic compensation and its systematic use in deriving a figure of the earth marked a long step in advance. The importance of this piece of Hayford's work has been increasingly recognized with the passage of time and on May 26, 1924, he was awarded the Victoria Medal of the Royal Geographic Society of London "for conspicuous merit in scientific research." The further recognition accorded by the section of geodesy in adopting his figures as the dimensions of the international ellipsoid of reference came only a few weeks before the illness that caused his premature death on March 10, 1925.

The possibility of misunderstanding arises from the fact that, since these figures were given by Hayford, the international ellipsoid based on them has often and very justly been called the Hayford ellipsoid. The semi-minor axis, however, given by Hayford as 6 356 909 meters, differs by about three meters from that determined by a simple calculation based on the parameters adopted by the section of geodesy; these latter give

Semi-minor axis = 6 356 911.946 meters

[1] J. F. Hayford, "Supplementary investigation in 1909 of the figure of the earth and isostasy." (Published by the U. S. Coast and Geodetic Survey.) 1910.

and this must be considered as the international value regardless of the value given by Hayford.[2] Or to state the matter in a somewhat different form, if the axes given by Hayford be taken as exact, the flattening comes out 1/296.959 instead of 1/297 exactly, as adopted by the section of geodesy.

This discrepancy between the two values of the minor axis is, of course, small in comparison with the uncertainty of the quantity itself. It is desirable, however, that there should be no misunderstanding about the dimensions of the international ellipsoid, especially because even a small inconsistency in the elements of the ellipsoid would lead to annoying discrepancies in the results of geodetic computations when two different formulas for the same purpose are used to check one another.

It happens that the parameters of ellipsoid of reference are the same as those adopted by Finland for its future geodetic work. Long before, at the Paris Conference[3] of 1911, the astronomers had adopted a flattening of exactly 1/297, based on Hayford's work, then but recently published.

The motives determining the adoption of the international ellipsoid of reference are well set forth in the report of the executive committee of the section of geodesy.

There is no intention whatever of forcing upon nations that have their triangulations either long completed or well advanced a new ellipsoid upon which they must recalculate their triangulation. If they are in a position to do so, so much the better, but obviously they can not be compelled to do this and any ruling of this sort would remain nugatory.

The international ellipsoid should be used in preference to any other:

(1) In regions recently opened to geodetic work, for triangulations very recently undertaken or scarcely begun, the calculation of which could be easily recommenced, and for triangulations to be undertaken in the future.

(2) In regions already covered by geodetic operations, when for any reason the work is to be revised.

(3) Whenever for purposes of higher geodesy the deflections of the vertical are to be calculated with reference to a definite ellipsoid.

It is thus to be hoped that the triangulation of the vast territories still to be won over to geodesy will all be calculated on the same ellipsoid and that some countries will make over their triangulation on the same system. Even though the homogeneity can not be complete, at least a long step will have been taken towards that unification of systems so much desired by geodesists, a fact which will make it easier to discuss and to solve a large number of important problems.

Item No. 3 of the quotation above suggests a distinction which it is well to bear in mind between the geographic and the strictly geodetic purposes of a geodetic survey. This distinction is illustrated by conditions prevailing in the United States. For many years the United States Standard Datum (later the North American Datum), based on the Clarke spheroid of 1866, has been the basis on which all geodetic triangulation has been calculated. The triangulation thus calculated is in turn the basis of maps and geographic positions in current use, many of the latter having a legal significance in connection with county, state or international boundaries. To recalculate the geographic positions of the 23,000 points that have been published and to redraw all current maps would obviously involve enormous labor and produce no little confusion and uncertainty, especially during the period of transition, and it is, therefore, the intention of the U. S. Coast and Geodetic Survey to continue to use the Clarke spheroid of 1866 (as part of the North American Datum) for all strictly geographical purposes.

In the scientific problem, however, of determining the figure of the earth, including local irregularities of shape with their important geophysical implications, much use is made of the deflections of the vertical, and it is obviously advantageous to have these deflections all over the world referred to the same ellipsoid. The question of recalculating the deflections of the vertical to refer them to the new international ellipsoid stands on an entirely different footing from that of recalculating geographic positions. In the United States there are less than 900 stations at which deflections of the vertical, either in the meridian or in the prime vertical or in both, are available, as compared with the 23,000 stations previously mentioned of which geographic positions have been published. The labor of making the change is obviously much less for the deflection stations alone than for all published stations. The confusion that would attend the change in the case of geographic positions in general should not exist in the case of the deflection stations, for the latter would be used only by

[2] This is the interpretation of the president and of the secretary of the section of geodesy, and in view of the way in which the vote on the international ellipsoid was taken, the two defining parameters being separately voted on at two different sessions, it is hard to see how any other interpretation could be tenable. In the tables for facilitating calculations on this new international ellipsoid, to be computed under the direction of the secretary, Colonel Perrier, the semi-minor axis will be taken as 6 356 911.946 meters.

[3] A meeting of the directors of nautical almanacs and ephemerides. (See Congrès international des éphémérides astronomiques tenu à l'Observatoire de Paris du 23 au 26 octobre, 1911. Published for the Bureau des Longitudes, Paris, 1911, pp. 36 and 42.)

specialists who would take care to find out the exact basis on which the deflections had been computed. For these reasons the U. S. Coast and Geodetic Survey hopes soon to undertake the task of referring deflections in the United States to the new international ellipsoid.[4]

The probable errors given by Hayford for the semi-major axis and the reciprocal of the flattening were, respectively, ± 18 meters and ± 0.5. Helmert[5] revised these figures by taking account of the uncertainty in the computed depth of isostatic compensation and thus increased the probable errors to ± 35 meters and ± 0.8. Helmert's figures represent the uncertainties as estimated solely by the residuals of the deflection stations in the United States and include no allowance for systematic errors such as possible constant errors in the standards of length used or errors due to reduction of bases to the geoid instead of to the spheroid. On this account Helmert's figures must be considered as still somewhat too small, judged merely by the data from which they were derived. On the other hand, there is confirmatory evidence of other sorts which tends to prove that Hayford's figures are perhaps more nearly correct than Helmert's computed probable errors would indicate. This is particularly true of the ellipticity, otherwise termed the flattening. From gravity observations Helmert[6] found 1/296.7 ± 0.4, Bowie[7] 1/297.4 ± 1.0 and Heiskanen[8] 1/296.7 ± 0.5. The precession of the equinoxes furnishes another means of deriving the flattening, if we grant the supposition that the density distribution within the earth is consistent with hydrostatic equilibrium, and since we know isostatic adjustment to be almost complete, this supposition seems unlikely to lead to serious error. Theoretically, in order to derive a flattening from the precession, we must assume not merely hydrostatic equilibrium in general but a definite law of variation from center to surface. The effect of changing the law is, however, surprisingly small, in fact, almost negligible. Any reasonably plausible law of density—and some not so plausible—will serve. Véronnet[9] derived from the precession a flattening of 1/297.12 ± 0.38. Here the ± 0.38 indicates, not probable error in the ordinary sense, but the estimated range of uncertainty due to our ignorance of the law of density, no allowance being made for the uncertainty of the observed value of the constant of precession nor of other quantities involved. De Sitter[10] after a careful discussion finds even narrower limits. His result is 1/296.92 ± 0.136. Most of this small probable error he finds to be due to the uncertainty in the mass of the moon, the uncertainty due to our ignorance of the law of density being but a comparatively small part, and the uncertainty due to the probable error in the precessional constant being negligible.

All these results are so close to 1/297 that we seem jutsified in estimating the actual uncertainty in this value of the reciprocal of the flattening as much less than Helmert's ± 0.8, or even than Hayford's original ± 0.5. That geodesists in general believe the flattening to be not far from 1/297 is shown by the practically unanimous vote of the section of geodesy at Madrid in favor of this value. There is, however, evidence from lunar observation and theory in favor of a larger flattening, about 1/294, and this can hardly be passed over in silence in a discussion of this sort. To present this matter at this particular point would, however, involve a long excursus, so a note dealing with this phase of the subject has been added at the end of this article.

The case for Hayford's semi-major axis is undoubtedly weaker than the case for his flattening. The comparative weakness is perhaps reflected in the vote of the section of geodesy, which was much less decisively in favor of Hayford's major axis that of his flattening.

The flattening can be derived by methods other than those involving triangulation (or traverse), but the semi-major axis can not.[11]

[4] This work is by no means so simple as might be supposed. The entire triangulation might be recalculated from the earliest stage on, but any of the known approximate short cuts may involve multiple values of the deflection at any given station, depending on the route by which the station is approached. See J. de Graaff Hunter, "The earth's axis and triangulation," Trigonometrical Survey of India, Professional Paper No. 16, 1918. Also W. D. Lambert, "Effect of variations in the assumed figure of the earth on the mapping of a large area," U. S. Coast and Geodetic Survey Special Publication No. 100.

[5] F. R. Helmert, "Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften," 1911, p. 19.

[6] F. R. Helmert, "Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften," 1915, p. 676.

[7] W. Bowie, "Investigations of gravity and isostasy," U. S. Coast and Geodetic Survey Spec. Pub. No. 40, 1917.

[8] W. Heiskanen, "Untersuchungen über Schwerkraft und Isostasie," Veröffentlichung des Finnischen Geodätischen Institutes, No. 4, 1924.

[9] A. Véronnet, "Journal des mathématiques pures et appliquées," Vol. 8 (1921), p. 416.

[10] W. De Sitter, "On the flattening and the constitution of the earth," Proceedings Koninklijke Akademie van Wetenschappen te Amsterdam, Vol. 27, 1924, p. 244.

[11] There is a method involving the observed values of the lunar parallax and of absolute gravity which may be made to give either the semi-major axis or the flattening, if the other is assumed to be known (see Helmert, "Höhere Geodäsie," Vol. II, p. 460; or De Sitter, "On the mean radius of the earth, the intensity of gravity

The data from the various pieces of triangulation scattered over the globe have not been comprehensively discussed to deduce the most probable value of the semi-major axis, and the writer in this connection can merely state his personal opinion that Hayford's determination of 6 378 388 meters, although larger than all preceding ones, is probably about correct, and that Helmert's probable error of ± 35 meters, although perhaps too small, is not very much too small. The writer would be somewhat surprised if a careful discussion of existing and future triangulation does not confirm Hayford's semi-major axis within 100 meters.

But the question of how closely the adopted ellipsoid of reference represents the actual geoid is by no means exhausted even when we have stated to our own satisfaction the probable errors of the parameters that determine the ellipsoid. It is also of interest to know how far the geoid and the ellipsoid may deviate from one another. To take an ideal case, let us suppose a perfectly ellipsoidal mass as large as the earth, its outer physical surface and the surfaces of equal density within being also equipotential surfaces. Then let us suppose that the outer crust down to a certain depth shrinks and swells here and there so as to form oceans, continents and mountains like the existing ones, but with perfect isostatic adjustment always maintained. The geoid, which formerly coincided with the outer surface, would go up with the continents and mountains and down with the ocean bottoms, but to a much less extent, the exact amount depending on the law of isostatic compensation. For existing areas of elevation and depression and a depth of compensation of 100 kilometers, the distance between the geoid and the ideal ellipsoid of closest fit (which need not be exactly our international ellipsoid[12]) would be 50 meters or less. If, however, isostatic compensation is imperfect to the extent of 10 or 15 per cent. of the total excess or deficiency of load, which is about the generally accepted estimate, and over wide areas, the 50 meters might be more than doubled. We should expect, however, these extreme figures of 100 or 150 meters to occur only very exceptionally, say in the Himalayan region or near the great ocean deeps.

Geoid contours were constructed by Hayford in connection with his earlier investigation[13] of his figures of the earth. Within the United States he found a variation in the elevation of the geoid above the Clarke spheroid[14] of 1866 amounting to 38 meters. This is somewhat greater than would be inferred from known differences of elevation and perfect isostasy with a depth of compensation of 100 kilometers but is by no means more than might be expected in view of ignorance of the real depth of compensation and the known imperfectness of isostatic adjustment.

There is a further possibility which might tend to increase the previous estimate of 100 or 150 meters for the extreme departure of the geoid from the ideal spheroid of reference. The longitude terms introduced by Helmert[15] and Heiskanen[15] into the formula for theoretical gravity, while not yet generally accepted, nevertheless should not be rejected out of hand as impossible. The amplitudes of these longitude terms are from six to nine times their probable errors, and the general improvement in the internal consistency of the gravity observations when these terms are included can be seen in other ways. Moreover, there is some slight evidence quite independent of the gravity data in favor of these terms.

Heiskanen's longitude term implies that a triaxial ellipsoid is a better approximation to the geoid than an ellipsoid of revolution; the longest equatorial radius is 172 meters greater than the mean, the shortest 172 meters less. This means a *systematic* departure of the geoid from the spheroid over a wide area to a maximum of 172 meters. Superposed on this systematic departure there would necessarily be local variations of considerable amount, so that the total departure of the geoid from the ellipsoid of revolution might considerably exceed 200 meters.

It should be noted, however, that the significance of these longitude terms, even if their existence and amount can be clearly established, is geophysical rather than geographical. There is no doubt that the

and the moon's parallax,'' Proceedings Koninklikje Akademie van Wetenschappen te Amsterdam, Vol. 1, 1915, p. 1291). This method may be of some value for determining the flattening (see note at end of this article) but is in no way to be considered as comparable in accuracy with triangulation for determining the semi-major axis.

12 When such an ellipsoid is actually used in connection with detached portions of triangulation, it is not really a single surface that is used, but many, all being ellipsoids of the same size and shape, to be sure, but not coincident in positions of their centers, nor even in the directions of their axes.

13 ''The figure of the earth and isostasy from measurements in the United States,'' U. S. Coast and Geodetic Survey, 1909.

14 This is not, of course, the ideal spheroid of closest fit to the geoid as a whole, but it is conceivable that in the region of the United States it might fit the geoid even better than the ideal spheroid. Only differences in elevation of geoid above spheroid can be deduced from triangulation. The zero contour line must be taken arbitrarily.

15 See references previously given, footnotes 6 and 8.

longitude terms would be important geophysically, for the existence of terms of that size would imply a widespread systematic departure from perfect isostasy of rather considerable amount, perhaps due to a tendency to over-compensation in oceanic areas.

On the other hand, it is doubtful whether geodesists will consider it desirable, unless these longitude terms should turn out to be much larger than now seems at all probable, to use a triaxial ellipsoid as a surface of reference rather than an ellipsoid of revolution. A triaxial spheroid involves considerable mathematical complication and does not abolish either the departure of the geoid from the surface of reference or the deflections of the vertical; it merely diminishes their average amounts. Unless these diminutions turn out to be very considerable, geodesists will probably continue to use an ellipsoid of revolution as the surface of reference.

It only remains to give the values of some of the geometrical and dynamical magnitudes inferable from the size and shape of the international ellipsoid of reference adopted on October 6 and 7, 1924, by the section of geodesy meeting at Madrid as part of the International Geodetic and Geophysical Union.

Fundamental Elements of the International Ellipsoid of Reference

a = semi-major axis (equatorial radius) = 6 378 388 meters

f = ellipticity (flattening) $= \frac{a-b}{a} = 1/297 = 0.003367\ 0034$

Derived quantities

b = semi-minor axis (polar radius) = 6,356,911.946 meters

e^2 = square of eccentricity $= \frac{a^2-b^2}{a^2} = 0.006\ 722\ 6700$

Length of quadrant of the equator	= 10,019,148.4 meters
Length of quadrant of the meridian	= 10,002,288.3 meters
Area of the ellipsoid	= 510 100 934 sq. km.
Volume of the ellipsoid	= 1 083 319 780 000 cu. km.
Radius of sphere having same area as ellipsoid	= 6 371 227.7 meters
Radius of sphere having same volume as ellipsoid	= 6 371 221.3 meters
M = mass ellipsoid[16]	5.988×10^{21} metric tonnes

Relation between geographic latitude, φ and geocentric latitude φ'.

$$\varphi - \varphi' = 695''.6635 \sin 2\varphi - 1''.1731 \sin 4\varphi + 0''.0026 \sin 6\varphi$$
$$= 695.6635 \sin 2\varphi' - 1.1731 \sin 4\varphi' + 0.0026 \sin 6\varphi'$$

Formula for theoretical gravity at the surface of ellipsoid (which is assumed to be an equipotential surface)

$$\gamma = \gamma_e \ (1 + 0.005288 \sin^2\varphi - 0.000006 \sin^2 2\varphi)\ \mathrm{cm/sec^2}$$

$$\gamma = \gamma_{45}\ (1 - 0.002637 \cos 2\varphi + 0.000006 \cos^2 2\varphi)$$

γ_e = gravity at equator at sea level =

978.038 cm/sec²	Bowie
978.052	Helmert
978.052	Heiskanen

γ_{45} = gravity in geographic latitude 45° at sea level =

980.621 /cm sec²	Bowie
980.629	Helmert
980.630	Heiskanen

Coefficient in the relation connecting the difference of equatorial and polar moments of Inertia, $C-A$, the Mass E and the equatorial radius a

$$C - A = 0.001092\ E\ a^2.$$

Note on Lunar Methods of Determining the Flattening of the Earth[17]

There are two lunar methods of determining the flattening of the earth: the first a mixed geometrical and dynamical method dependent on the parallax of the moon and going back in principle to Newton's famous calculation that identified terrestrial gravitation with the force that retains the moon in its orbit; the second method purely dynamical and dependent on the lunar perturbations. This second method might be subdivided into several methods according to the particular perturbation used.

Newton calculated what the parallax of the moon would be if terrestrial gravitation, diminishing inversely as the square of the distance, were the controlling force, and on comparing this with the observed parallax he found them to agree within the uncertainties due to errors of observation.[18] The so-called observed parallax, however, that is, the equatorial horizontal parallax, really involves an assumption as to the ellipticity of the earth, and still other assumptions as to the local deflections of the vertical and as to the elevations of the geoid above the spheroid at the observatories where the parallax is determined. The usual and almost inevitable assumption hitherto has been that these deflections and elevations are all

16 Mean density taken as 5.527, the value found by both Boys (Phil. Trans. A. Vol. 186 (1895) p. 1) and Braun, "Denkschriften der Akademie der Wissenschaften zu Wien," *Mathematisch-Naturwissenschaftlichen Classe,* 64, 1896, p. 187.

17 Professor E. W. Brown gave valuable assistance in the preparation of this note. He is not, however, to be considered responsible either for the statements made or for the opinions expressed.

18 There are terms involving also the mass of the moon and the effect of the sun which require no more than mention in this connection.

zero.[19] The calculation by which terrestrial gravitation is extended out to the moon and the parallax of the latter thus found is also affected by the assumed flattening of the earth, though not so much as the observed parallax. The flattening is found by assuming different values for it until the calculated equatorial horizontal parallax comes out equal to the observed parallax. This happens for a flattening equal to 1/293.4, according to De Sitter,[20] on the assumption that the deflections of the vertical at Greenwich and Cape of Good Hope observatories are zero. To make the two parallaxes equal for a flattening of 1/297 would require a deflection of about 11″ at both observatories. A calculation of these deflections on the theory of isostasy, with uniform compensation down to a depth of 120.9 km, gave deflections of 1″.16 at Greenwich and 3″.25 at the Cape. These had the proper signs to shift the reciprocal of the flattening from 293.4 towards 297 but are not large enough to change it all the way. They could be increased somewhat by adopting some other distribution of isostatic compensation, but the most probable explanation of much of the discrepancy in the flattening is observational error in the determination of the parallax. De Sitter concludes that the parallax calculated on the theory of gravitation is more reliable than the parallax directly observed and the parallax used in Brown's new lunar tables is likewise derived from theory. These conclusions of De Sitter and Brown and the uncertainty due to ignorance of the actual deflections of the vertical are thus unfavorable to the accuracy to the flattening obtained from it.

The purely dynamical lunar methods do not determine the flattening directly but instead the quantity $J = \dfrac{C - \dfrac{A+B}{2}}{E\,a^2}$, where A, B and C are the principal moments of inertia of the earth in ascending order of magnitude, E is the mass of the earth and a its equatorial radius. This quantity J occurs in the developments in series of the earth's gravitational potential. If the earth were composed of homogeneous concentric spherical shells, J would eventually be zero. From J the flattening, f, is deduced by a simple formula.

The fact that the earth is not spherical, that is, that J is not zero, causes a number of perturbations in the moon's motion, both periodic and secular. From the observed values of these perturbations the value of J can be deduced and hence the flattening.

The *periodic* perturbations due to the figure[21] of the earth have been used to determine the flattening by various astronomers from Laplace down to the present. These periodic perturbations are, however, rather difficult to disentangle by observation from the perturbations due to other causes and hence the flattening deduced from them is not entitled to much weight.

The principal *secular* perturbations due to the earth's figure are those of the node and perigee of the lunar orbit. These, being cumulative with the lapse of time, can be determined very accurately. Unfortunately, however, for our present purpose much the larger part of these perturbations is due directly or indirectly to the action of the sun and thus must be determined and subtracted from the observed effect in order to determine the portion due to the figure of the earth.[22] There is also a portion, minute but by no means negligible in the present problem, due to the figure of the moon. Fortunately the portion due to the sun depends mainly on the ratio of the month to the year, a quantity known with great precision, so that apart from possible but not very probable errors in the theory of the smaller terms representing the sun's effect[23] and the uncertainty due to the figure of the moon, the perturbation due solely to the figure of the earth may be considered as having almost the same observational error as the entire observed secular perturbation, whether of perigee or node.

In connection with the preparation of new lunar tables the flattening was determined by Brown,[24] who found 1/293.5 from the combined results for the perigee and node. His adopted result 1/294.0 comes from including his value 1/294.4 determined from the

[19] The deflections and elevations are understood to refer to the unknown ideal ellipsoid that fits the earth as a whole as closely as possible.

[20] W. De Sitter, "On the mean radius of the earth, the intensity of gravity and the moon's parallax," Proceedings Koninklijke Akademie van Wetenschappen te Amsterdam, Vol. 17, 1915, p. 1294.

[21] The word *figure* is used in the extended sense of *figure and constitution,* more specifically its figure and constitution as they affect the value of J.

[22] Of the total annual motion of 146435″ in the perigee and 69679″ in the node all but about six or seven seconds of each are due to the action of the sun.

[23] The formulas for this occupy many lines of the quarto page of Delaunay's "Lunar Theory." Even so, they are not sufficiently accurate for the present purpose and the more modern theory, such as Brown's, does not use a complete and explicit formula, but is based on a process of numerical approximations. Delaunay's formulas, however, bring out the fact that the solar effect depends essentially on quantities known to a high degree of precision.

[24] E. W. Brown, Monthly Notices Royal Astronomical Society, Vol. 74 (1914), p. 563.

Greenwich–Cape of Good Hope parallax observations. By changing the assumed data for the figure of the moon De Sitter[25] found 1/296. The data for the figure of the moon depend partly on observation of the lunar librations, partly on assumptions as to the way in which the density of the moon varies from center to surface.

The process of deducing the flattening from lunar perturbations is thus seen to be far from simple. The possibilities for reconciling the flattening of 1/294 found by this method with the 1/297 of the international ellipsoid lie: (1) in changes in the secular variations deduced from observation; (2) in data for the figure of the moon; and (3) in possible corrections to a very complex mathematical theory.

Jones[26] has recently used occultations at the Cape of Good Hope to correct the motions of the perigee and node, and his corrections tend to diminish the disagreement between Brown's value of the flattening and the new international value. The older lunar observations, however, are not so well represented and it is questionable whether the full amount of his corrections can be accepted without further consideration.

The figure of the moon, as De Sitter has shown, gives one way of reconciling, partially at least, the two values of the flattening. Because the moon is smaller and cooler than the earth its figure might depart relatively more from hydrostatic equilibrium than the figure of the earth can, and there is evidence to show that it does. In the present state of our knowledge, or ignorance, a wide range of suppositions is admissible. In the absence of direct evidence on this point the disagreement between Brown's value of the flattening and the value obtained by other methods might be used as evidence to show what the figure of the moon actually is.

The theory of these secular variations, like the lunar theory in general, is exceedingly long and intricate, but it has been so much worked over that it is probably now correct and must be assumed to be so, at least until some one with the necessary ability, energy and inclination discusses the matter further and finds an error. There seems to be room, however, for some slight improvement in a much simpler theoretical matter, the calculation of the flattening, f, from J. Astronomers, as a rule, have omitted certain second order geodetic terms in the equation connecting these two quantities. The inclusion of these terms increases the reciprocal of the ellipticity by 0.4 or 0.5, or perhaps more, the exact amount depending on the exact form of the approximate equation between J and f, which may be stated in more than one way when correct to the first order only. These second order terms would thus help to diminish the discrepancy still further.

The only conclusion that can safely be drawn at the present time is that the discrepancy between the value of the flattening from lunar observations and the new international value is probably not quite so large as it first appeared to be, since various considerations all working in the same direction tend to bring the two values together. The ideal and the problem of astronomers and geodesists is, of course, complete reconciliation of the flattening as determined by geodetic methods with the values determined by all the various astronomic methods.

WALTER D. LAMBERT
U. S. COAST AND GEODETIC SURVEY

[25] W. De Sitter, Koninklijke Akademie van Wetenschappen te Amsterdam, Vol. 27 (1915), p. 1309.

[26] H. S. Jones, Monthly Notices Royal Astronomical Society, Vol. 85, 1924, p. 11.

CHARLES VANCOUVER PIPER

A BOTANIST of great ability, a man of unusual breadth, Charles Vancouver Piper, died on February 11, in his fifty-eighth year, and those who knew him well sustained a loss that can never be repaired. His work as botanist and agronomist is too well known in the United States and abroad to require comment on my part; it is of the man, not the scientist that I would say a personal word. Piper was built on big lines, mentally and physically. His mind worked directly and he was straightforward and fearless in his pursuit of truth and in working for what he believed to be the right course to follow. He believed in looking facts in the face impersonally and his free spirit could never understand why men should evade facts and beat about the bush. Piper's views were positive, but withal he was ever ready to listen to and to respect a dissenting voice. He never resented a difference of opinion on the part of a subordinate, but his logical mind was prompt to demand a reason for any view expressed. It was this willingness to entertain another view and the reasonableness of the man that endeared him to those whose privilege it was to work under his direction, while his broad knowledge and sound views won their respect. Contact with Piper was always stimulating; he was interested in all phases of life and he could suggest more problems in one interview than the average worker could tackle in a life time.

Piper gave himself freely to his work, worked with unusual rapidity, lost no time in arriving at decisions and consequently accomplished a prodigious amount in a short time. He read rapidly and would go over a manuscript so quickly that it seemed he could not have read it carefully, yet it was rare that a weak