Like many inventors, Weston has been engaged extensively in patent litigation. To uphold some of his rights, he had to spend on one set of patents nearly $400,000, a large amount of money for anybody, but as he told me, he begrudges less the money it cost him than all his valuable time it required—a greater loss to an inventor thus distracted from his work. What is worse, most of this litigation was so long-winded that when finally he established his rights, has patents had aged so much that they had lost, in the meantime, most, if not all, of their seventeen years' terms of limited existence. And here I want to point out something very significant. In the early periods of his work, between 1873 and 1886, Weston took out over three hundred patents. Since then, he has taken considerably less, and of late, he has taken out very few patents—after he became wiser to the tricks of patent infringers. Formerly, as soon as he published his discoveries or his inventions, in his patent specifications, he was so much troubled with patent pirates that instead of being able to attend to the development of his inventions, he was occupied in patent litigation. As an act of self-preservation, he has had to adopt new tactics. He now keeps his work secret as long as possible, and in the meantime, spends his money for tools and equipment for manufacturing his inventions. In some instances, this preparation takes several years. Then by the time he sends any new type of instruments into the world, and others start copying, he has already in preparation so many further improvements that pretty soon the next instrument comes out which supersedes the prior edition. He had to utilize these tactics since he found how impractical it was to rely on his patent rights for protection. That inventors should have to proceed in this way is certainly not a recommendation for our patent system; it kills the very purpose for which our fundamental patent law was created, namely, *the prompt publication of new and useful inventions.*

L. H. Baekeland

NOTE ON THE ORBITS OF FREELY FALLING BODIES

In No. 975, Vol. XXXVIII., N.S. (September 5, 1913), of this journal, I gave a semi-popular account of an investigation on "The orbits of freely falling bodies" published in Nos. 651, 652 of the *Astronomical Journal,* August 4, 1913. Soon after the appearance of these papers several correspondents challenged the result I derived for the meridional deviation of the falling body, all of them maintaining that this deviation is toward the equator instead of away from it, as I had concluded. Being preoccupied with affairs somewhat remote from the fields of mathematical physics, I have not been able to give this apparent discrepancy adequate attention, although its origin was indicated in an informal communication to the Philosophical Society of Washington in April, 1914.

In the meantime, two noteworthy contributions to the already extensive literature of this subject have been published by Professor F. R. Moulton[1] and by Professor Wm. H. Roever,[2] respectively. These contributions are not only important for originality of methods and for painstaking attention, especially to mathematical details, but they may seem to the casual reader to have exhausted the subject by demonstrating in the most approved mathematical fashion of our day that the postulates

[1] "The Deviations of Falling Bodies," *Annals of Mathematics,* Second Series, Vol. 15, No. 4, pp. 184–94, June, 1914. This investigation is specially remarkable in that but one kind of latitude is used. It is likewise remarkable in that no explicit statement is made as to which of the various latitudes (astronomic, geocentric, geodetic or reduced) is used.

[2] "Deviations of Falling Bodies," *Astronomical Journal,* Nos. 670–672, pp. 177–201, January 22, 1915.

adopted and the results derived are at once unique, "necessary" and "sufficient." Both authors insist with much particularity that the discrepancy between us is due to superior methods of approximation followed by them in integrating the fundamental equations of motion, since we all agree on the forms of these equations.

But the subject is not thus easily disposed of. A sense of humor should lead us to inquire whether the parties concerned have all solved the same problem. The answer to such an inquiry in this case is that while all have ostensibly treated the same problem, two different problems have actually been solved. We have thus developed a fresh illustration of a common danger in mathematical physics, namely, that of fixing attention on mathematical perfection before adequate regard has been given to physical requirements.

It would be out of place in the columns of this journal to enter into a review of the details of the investigations of Professor Moulton and Professor Roever. Such a review is, in fact, neither desirable here nor essential in a technical publication. The source of the discrepancy referred to is so evident that it needs only to be stated to be appreciated; and once stated there is no ground for controversy in this part of the subject. It appears desirable, however, to refer in some detail to the general considerations involved in deriving the orbits of falling bodies as well as to those special considerations which determine meridional deviations. For this purpose it will be essential in a limited degree to use the abridged language of analysis.

But before adducing these considerations I wish to plead guilty to an oversight in reading Professor Roever's earlier papers[3] and to submit a brief of extenuating circumstances. At first reading of these papers it appeared to me that he had neglected terms involving the square of the angular velocity of the earth in his equations of motion of the falling body. These terms do not appear explicitly in those equations, but only implicitly through a special potential function used by him for the first time, apparently, in this connection. Not being able to follow his derivation of these equations (if, indeed, he may be said to have derived them in the mechanical sense), I assumed them to be identical in meaning, as they are in form, with those published by several earlier writers. This assumption was supported by uncertainty as to meaning and by lack of homogeneity of his expression for the potential function introduced on page 342 of his first paper; and still more by his identification of astronomic with geocentric latitude (on p. 339, same paper) by means of the loose phrase "with sufficient approximation." A similar lack of "accuracy and precision" will be found in several parts of his latest paper cited above. See, for example, his equations (j), wherein he confounds geocentric with reduced latitude; also p. 199, where he identifies his equations (38) and (41) with my equation (26) and makes with respect to them the surprising statement that "it is, of course, evident that this function corresponds to some distribution of revolution" in the earth's mass. Concerning the absence of validity for this latter statement some remarks are made below.

Now, to account for the discrepancy in question, namely, our differing values for the meridional deviation of the falling body, it is only essential to observe that two different surfaces of reference have been used. Professors Moulton and Roever have referred the motion to a geoid specified by a certain approximate potential function, while I have referred the same motion to Clarke's spheroid of revolution (of 1866), which is determined by certain axes (a, b) dependent on geodetic measurements. These surfaces are not coincident to the order of approximation adopted by either party, and the discrepancy developed appears to be both "necessary" and "sufficient" to restore confidence in the mathematical mills of all concerned.[4]

[3] "The Southerly Deviation of Falling Bodies," *Transactions of the American Mathematical Society*, Vol. XII., pp. 335–53. "The Southerly and Easterly Deviations of Falling Bodies for an Unsymmetrical Gravitational Field of Force," *Ibid.*, Vol. XIII., pp. 469–490.

[4] It has been known since the earlier writings of Airy that the geoid and the spheroid are not coincident, but I was not aware that their inclination

To put this statement in a clearer form for the mathematical reader, let V denote the gravitational potential per unit mass at a point outside, or on, the earth, and let r and ψ denote, respectively, the radius vector and the geocentric latitude of that point. Then, if ω denote the angular velocity of the earth and if the point (r, ψ) is attached to and rotates with the earth, the expression

$$V + \tfrac{1}{2}\,\omega^2 r^2 \cos^2\psi$$

is the potential per unit mass at that point due to the attraction and to the rotation of the earth. Calling this expression U,

$$U = V + \tfrac{1}{2}\,\omega^2 r^2 \cos^2\psi = cons't \qquad (1)$$

specifies a family of equipotential surfaces about the earth. Thus, for example, $U = constant$ specifies the sea surface, provided V, r, ψ have appropriate values, and this surface, which may be imagined to extend through the continents, is called the geoid. Similarly, corresponding surfaces above and below the sea surface are geoidal and may be used, like the sea level, as surfaces of reference.

Adopting for the moment the simpler hypothesis that the shape of the geoid does not depend on longitude, the divergence from parallelism of the geoid (1) and the spheroid (a, b) may be defined in the following manner. Since the linear acceleration components along and perpendicular to the radius vector r at the point (r, ψ) of the geoid $U = constant$ are, respectively,

$$\frac{\partial U}{\partial r} \quad \text{and} \quad \frac{\partial U}{r\partial\psi},$$

the tangent of the angle between r and the normal to the geoid at the same point is given by the quotient of the second by the first of these partial derivatives.[5]

The angle thus derived is the difference between the astronomical latitude, ϕ_0, say, and the geocentric latitude ψ of the point (r, ψ). Using the data for V and r adopted in my paper cited above, it is found that the general value of this difference is to the first order of approximation, and in seconds of arc,

$$\phi_0 - \psi = 688'' \sin 2\phi_0. \qquad (2)$$

On the other hand, the difference between the geodetic latitudes ϕ, say (determined by the normal to the spheroid (a, b)), and the geocentric latitude of the same point, is to the same order of approximation

$$\phi - \psi = 700'' \sin 2\phi. \qquad (3)$$

There is thus a systematic difference between these two quantities, since the residuals $(\phi_0 - \phi)$, or the so-called plumb-line deflection in the meridian, are assumed to be of compensating plus and minus magnitudes in determining the spheroid (a, b). Otherwise expressed, this systematic difference is such as to make the value of the meridional deviation of the falling body vanish to terms of the order of ω^2 inclusive, adopted in my investigation, if reference is made to the geoid instead of to the spheroid; and to this order of approximation the discrepancy is completely accounted for.

It is evident that we may not discard either in favor of the other, of the two surfaces of reference giving rise to this discrepancy, since their departure from coincidence is an index of our ignorance of the geoid especially and to a less extent also of the spheroid used. The geoid specified by equation (1) is obviously less well known than the spheroid, since an assumption must be made concerning the distribution of density in the earth before the moments of inertia which determine the geoid can be computed. Thus the relation (2) is known with less precision than the relation (3); but it is now clear that a complete treatment of the problem in question requires that both of these relations be taken into account along with the additional relations $(\phi_0 - \phi)$ and $(\lambda_0 - \lambda)$, say, or the plumb-line deflections in latitude and longitude, respectively, at the point (r, ψ, λ). That considerable uncertainty attaches still to the relation (3) is indicated by the range in the following values for the coefficient of sin 2ϕ derived by some earlier and by some more recent writers in geodesy.

could figure sensibly in the orbits of falling bodies when my first investigation of these orbits was published.

[5] To terms of the order of ω^2 inclusive this tangent, using the notation of my paper cited above, is

$$\frac{\tfrac{1}{2}r\left(\omega^2 + \dfrac{3\beta}{r^5}\right)\sin 2\psi}{\dfrac{\alpha}{r^2} + \dfrac{3\beta}{2r^4}(1 - 3\sin^2\varphi) - \omega^2 r\cos^2\psi}.$$

Bessel, 1841	690.6″
Clarke, 1866	700.4″
Harkness, 1891	688.2″
Hayford, 1909	695.8″

It appears essential in this connection to call attention to a common misapprehension with respect to the earth which Professors Moulton and Roever have helped to disseminate by their able contributions to the subject before us. The potential function V which appears in equation (1) above, may be developed in a series of spherical harmonics whose first three terms are given in the second member of the following equation:

$$V = \frac{Mk}{r} + \frac{k}{2r^3}\{C - \tfrac{1}{2}(B + A)\}(1 - 3\sin^2\psi) + \frac{3k}{4r^3}(B - A)\cos^2\psi\cos 2\lambda. \quad (4)$$

In this r, ψ, λ are, respectively, the radius vector, geocentric latitude and longitude of the point, outside the earth, to which V applies. M is the mass of the earth, k is the gravitation constant and A, B, C are in order of increasing magnitude the moments of inertia of the earth with respect to a set of principal axes originating at its centroid. C is commonly said to be the moment with respect to the axis of rotation of the earth, but in these days of "variation of latitudes" and of "mathematical rigor," it should be said to apply to the axis of figure nearest the axis of rotation. A and B are then the moments with respect to the principal axes in a plane through the centroid and normal to the axis of C, or in the plane of the equator as we commonly say.

The expression (4) has very remarkable properties. It is equation (26) of my paper cited above. The value of V is the same whether the latitude ψ is positive or negative; and dependence on longitude vanishes if $B = A$. With respect to this equation Professor Moulton remarks "If the rotating body is a figure of revolution about the axis of rotation whose density does not depend upon the longitude, the function V can be developed as a series of zonal harmonics in the form

$$V = \frac{\alpha}{r} + \frac{\beta}{r^3}(1 - 3\sin^2\varphi)."$$

A similar remark with regard to this expression has been quoted above from Professor Roever, the inference being, apparently, that in some manner the expression (4) limits the distribution of the earth's mass to one of revolution. As a matter of fact, however, the expression (4) implies no such restriction; on the contrary, it applies equally to a body of any form and of any distribution of density, the sole requirement being that the point (r, ψ, λ) lie at a distance from the centroid of the body equal to or greater than the greatest distance of any element of mass in the body from the same point. The considerations which permit us to assume $(B - A)$ small, or possibly negligible, in this and other problems of geodesy, must depend, unfortunately, on other sources of information than the expression (4). Some attention to these considerations was given in each of my papers referred to in the first paragraph of this note.

Without going further into the subject at this time it may suffice to remark that it now appears illusory except as a mathematical exercise to push the solution of the differential equations of motion of a falling body to terms involving the second derivatives of V without including the third term in the right-hand member of (4), without taking account of the known relation between these derivatives, and without taking account of plumb-line deflections, which often exceed the discrepancy shown by equations (2) and (3).

R. S. WOODWARD

February 22, 1915

ARTHUR VON AUWERS

THE problems that confront the astronomer differ from those with which workers in other departments of science are engaged in many important particulars, but in none more than in the magnitude of the data involved. So great is the number of the stars, so vast, both in space and in time, the scale of their motions, that in general it transcends the powers of an individual, or even of a single observatory, to collect, within the span of a lifetime, the materials for comprehensive studies, or to collate and discuss them. Cooperation is probably