drawing and descriptive geometry; Leicester F. Hamilton, instructor in analytical chemistry, and Ruth M. Thomas, research associate in organic chemistry.

Dr. Ward J. MacNeal has been appointed director of laboratories of the New York Post-graduate Medical School and Hospital, succeeding Dr. Jonathan Wright, resigned. Dr. Morris Fine has been promoted to adjunct professor of pathologic chemistry; Dr. Richard M. Taylor, to adjunct professor of pathology and Paul A. Schule, to a lectureship of bacteriology.

Mr. R. H. Bogue, for three years instructor in chemistry in the Massachusetts Agricultural College, has accepted a position as assistant professor of agricultural chemistry at the Montana Agricultural College. His place in the Massachusetts College has been filled by the appointment of Paul Serex.

Dr. F. D. Fromme, formerly of the botany department of the Indiana Agricultural Experiment Station, has been appointed to the professorship of plant pathology and bacteriology in the Virginia Polytechnic Institute.

Guy West Wilson, formerly agent, U. S. Laboratory of Forest Pathology, stationed at the Agricultural Experiment Station, New Brunswick, N. J., has been appointed assistant professor of mycology and plant pathology, State University of Iowa, Iowa City, Iowa.

DISCUSSION AND CORRESPONDENCE

NOTE ON THE MERIDIONAL DEVIATION OF A FALLING BODY

Introductory Remarks.—Various definitions have been given for the meridional deviation of a falling body and various potential functions have been assumed in the mathematical determination thereof. It is therefore perfectly natural that the results found by different writers on the subject do not agree. However, when once the equations of motion of the falling body, the definition of the deviation, and the potential function have been fixed, the solution of the problem is unique. In 1911[1] I published a general formula for the meridional deviation which included as special cases the apparently discordant formulæ of several other writers. This was possible because my formula could be broken up into parts which corresponded to different kinds of meridional deviation, and also because it was expressed in terms of the symbol representing the potential function, which symbol, when replaced by particular forms of this function, made it yield the results of the writers who had used these particular forms. In 1913[2] Dr. R. S. Woodward treated the problem using the equations of motion, the definition of the deviation and one of the potential functions which I had used. Therefore he should have obtained the result which I did for that potential function provided my solution was correct. But he got a different result. This lack of agreement was the means of interesting Professor F. R. Moulton in the problem. In June, 1914, Professor Moulton published an article[3] in which he solved the problem treated by Dr. Woodward and his result was the same as mine. Shortly after the appearance of Professor Moulton's article, I published a paper[4] (which, however, was prepared at the same time as Professor Moulton's, and independently of it) showing that Dr. Woodward's methods, when applied to his initial assumptions, should lead to my results. In reply to Professor Moulton's article and my last paper, Dr. Woodward has just published a note[5] in which he states that he did not solve the problem which Professor Moulton and I had solved. The present article is my reply to this note.

Granting that "two different problems have actually been solved," I will show that this is so because Dr. Woodward has not solved the

[1] *Transactions of the American Mathematical Society*, Vol. XII., pp. 335–53, *ibid.*, Vol. XIII., pp. 469–90.

[2] *Astronomical Journal* (Nos. 651–52), August 4, 1913.

[3] "The Deviations of Falling Bodies," *Annals of Mathematics* (Second Series), Vol. 15, pp. 184–94, June, 1914.

[4] "Deviations of Falling Bodies," *Astronomical Journal*, Nos. 670–72, pp. 177–201, January 22, 1915.

[5] "Note on the Orbits of Freely Falling Bodies," Science, New Series, Vol. XLI., No. 1057, pp. 492–95, April 2, 1915.

problem which he originally proposed. The solution of the problem which he now states in his note that he has solved corresponds to a meridional deviation different from that originally defined. This deviation is of the form $Ah + Bh^2$, while that originally defined was of the form Ch^2, in which h is the height of fall and A, B, C are constants. I will also show that a formula for this new meridional deviation may be obtained without integrating the equations of motion at all, and that this formula yields a result differing but slightly from the result given by Dr. Woodward, but given by him for the deviation originally defined. In this article I will also reply to certain criticisms made by Dr. Woodward concerning my work.

1. In the sixth paragraph of his note[6] Dr. Woodward says:

> 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.

In reply to this statement I should like to say that in order to determine the path (orbit) of the falling body a potential function is needed; a surface of reference is not enough. When once the potential function is chosen the geoid (or level surface) is determined. That the geoid, and not the spheroid, was originally contemplated by Dr. Woodward as the surface of reference, appears from the statement made below equations (2) of his paper in the *Astronomical Journal* (Nos. 651–52). For, of the points P_0 and P_1 from which, respectively, the body is let fall and the deviations measured, he says:

> It is important to specify how this point P_1 is located with reference to the initial point P_0. Imagine a basin of mercury at the point P_1. The surface of the mercury will be the level, or equipotential (or horizontal) surface through this point; and if it is located as here assumed the line joining the two points P_0 and P_1 will be normal to the surface of the mercury.

Now, the surface of the mercury is surely a portion of the geoid and *not* of the spheroid. The position of the point P_1 besides depending on that of P_0, depends on the potential function, and, furthermore, on the same potential function as that which is used in the differential equations of motion of the path of the falling body. Dr. Woodward now states that he used for his surface of reference the spheroid (of Clarke) instead of the geoid. If these two surfaces differ ever so slightly from one another—and they do differ according to equations (2) and (3) of his note[7]—the quantities which are determined by using the spheroid for reference are not the same as the quantities η, ξ (measured from P_1) which he originally defined as the easterly and meridional deviations of the falling body. Therefore, the problem which he now states that he has solved is not the one which he originally proposed.

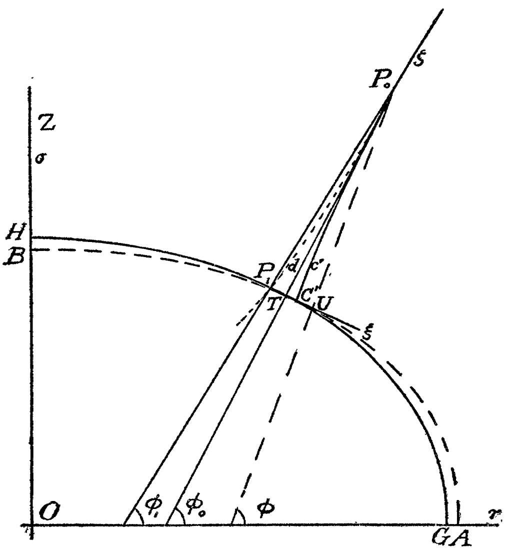

FIG. 1.

[6] SCIENCE, No. 1057, pp. 493.

[7] SCIENCE, No. 1057.

2. For the sake of simplicity let us assume (as Dr. Woodward did before he got very far into his solution) that the distribution of the earth's gravitating matter is such as to make the potential function independent of the longitude (*i. e.*, correspond to a *distribution of revolution*). Let P_0 denote the point (fixed with respect to the earth) from which the body falls. In Fig. 1 the plane of the drawing is assumed to be the meridian plane of P_0. This plane contains the axis of rotation OZ, and cuts from the geoid and the spheroid (both of which are surfaces of revolution of axis OZ) the meridian curves GH and AB, respectively, GH drawn full and AB dashed. The point P_1 is the foot of the perpendicular from P_0 to the geoid GH. The straight line P_1P_0 is then the vertical of PA and the angle ϕ, which it makes with the equatorial plane (perpendicular to the axis OZ) is the astronomic latitude of P_1. The straight line P_0T (not coincident with P_0P_1) is the vertical of P_0 (*i. e.*, the normal at P_0 of the level surface through P_0). The angle ϕ_0 which it makes with the equatorial plane is the astronomic latitude of P_0.[8] The path (with respect to the earth) of the falling body is a curve c which does not lie in the meridian plane of P_0, but is tangent at P_0 to the vertical P_0T of P_0. This curve c pierces the horizontal plane of P_1 (*i. e.*, the plane through P_1 perpendicular to P_1P_0) in a point C. Let us denote by c' and C' the orthographic projections of c and C, respectively, on the meridian plane of P_0. Then c' is also tangent to P_0T at P_0. According to the definitions originally adopted by Dr. Woodward,

$C'C$ is the easterly deviation of the falling body,
P_1C' is the meridional deviation of the falling body.

He now says, however, that he referred the motion of the falling body to the spheroid (AB, Fig. 1). By this he must mean that he measures the deviation of the falling body from the foot U of the normal drawn from P_0 to the spheroid. The angle ϕ which this normal (shown in Fig. 1 by the dashed line P_0U) makes with the equatorial plane is called the geodetic latitude of U. In other words, the statement that the spheroid is his surface of reference implies that UC' is the meridional deviation of the falling body. That this is the implication is also borne out by the fact that the value of this deviation agrees with the value which Dr. Woodward actually found. In order to show this let us first observe that

$$UC' = UT + TC', \tag{1}$$

the positive sense of each of these quantities being taken toward the equator. If ϕ and ϕ_0 be expressed in radians,

$$UT = (\phi_0 - \phi)h, \tag{2}$$

where $h = P_1P_0$ is the height of fall. Since the curve c' is tangent to P_0T at P_0, and has no cusp there,[9]

$$TC' = \tfrac{1}{2}(1/\rho_0)h^2 + \text{higher powers of } h,\text{[10]} \tag{3}$$

where ρ_0 is the radius of curvature of c' at P_0. By equations (2) and (3) of Dr. Woodward's note,[11] $\phi - \phi_0 = 12'' \sin 2\phi$, and hence in circular measure

$$\phi - \phi_0 = .00006 \sin 2\phi. \tag{4}$$

Hence for the data

$$h = 49024 \text{ cm.}, \ \phi = 45°, \tag{5}$$

assumed in his example in the *Astronomical Journal* (Nos. 651–652)

$$UT = -2.94 \text{ cm.}$$

[8] The difference between ϕ_0 and ϕ_1 is given by the formula

$$\phi_0 - \phi_1 = \frac{-(\partial g/\partial \xi)_1}{g_1} h + \text{higher powers in } h,$$

where h is the distance of P_0 above P_1, g_1 is the value of the acceleration due to weight at P_1, and $(\partial g/\partial \xi)_1$ is the value, at P_1, of the derivative of g with respect to ξ, where ξ represents distance measured to the south at P_1. For the potential function used by Dr. Woodward (*Astronomical Journal*, Nos. 651–52),

$$\frac{-(\partial g/\partial \xi)_1}{g_1} = 8.3 \times 10^{-12} \sin 2\phi_1.$$

[9] The curve c has a cusp at P_0 as has also its projection on a plane perpendicular to the meridian plane of P_0.

[10] See "Introduction to Infinite Series," by Osgood, p. 39.

[11] SCIENCE, No. 1057.

For the same data and for the potential function used by Dr. Woodward,[12]

$$TC' = .0010 \text{ cm.}$$

Therefore

$$UC' = UT + TC' = -2.94.$$

This result agrees very well with the value $\xi = -3.03$ obtained by Dr. Woodward for his originally defined meridional deviation. Thus I have shown that for the meridional deviation implied by the statement that the spheroid instead of the geoid is the surface of reference, it is possible to find a formula, namely

(6) $$UC' = -(\phi - \phi_0) \sin 2\phi \cdot h,$$

without integrating the equations of motion, and that, for the data given by equations (2) and (3) of Dr. Woodward's note, this formula yields values for the deviation UC' which do not differ much from those obtained by Dr. Woodward for his originally defined meridional deviation.

3. We have just seen that the expression (formula 6) for the newly defined meridional deviation UC' begins with the first power of h. Let us now show, with the aid of Fig. 1, that the originally defined meridional deviation P_1C' begins with the second power of h. For this purpose let us think of a series of level surfaces between the geoid GH and the level surface of P_0. The locus of the feet of the perpendiculars from P_0 to these level surfaces is a curve d passing, necessarily, through the points P_0 and P_1 and tangent at P_0 to the vertical P_0T of P_0 (see dotted curve in Fig. 1). Since the curve d is tangent to P_0T at P_0, we have for a reason given above,

(7) $$P_1T = \tfrac{1}{2}(1/\rho_d)h^2 + \text{higher powers of } h,$$

where ρ_d is the radius of curvature of the curve d at the point P_0. It is further evident from Fig. 1, that

(8) $$P_1C' = P_1T + TC',$$

the positive sense of each of these quantities being taken toward the equator. By relations (3), (7) and (8)

(9) $$P_1C' = \frac{1}{2}\left(\frac{1}{\rho_d} + \frac{1}{\rho_0}\right) h^2 + \text{higher powers of } h.$$[13]

Hence we see that while the originally defined meridional deviation P_1C' begins with the second power of h, the newly, implicitly, defined meridional deviation UC' begins with the first power of h.

4. In commenting on my work, Dr. Woodward, after speaking of a certain assumption,

[12] The quantity TC' is the negative of the quantity which I denoted by η_1 in my first paper (*Transactions of the American Mathematical Society*, Vol. XII., No. 3, pp. 335–53). It is the quantity which Comte De Sparre used for his meridional deviation of a falling body. I have shown this quantity to be expressible by the formula

$$TC' = \left\{ 2\omega^2 \sin 2\phi_0 + \left(\frac{\partial g}{\partial \xi}\right)_0 \right\} \frac{h^2}{6g_0},$$

where h and ϕ_0 have the meanings given above, ω is the angular velocity of the earth's rotation, and g_0 and $(\partial g/\partial\xi)_0$, are the values which the acceleration g due to weight and the derivative of g with respect to ξ have at the point P_0, ξ representing distance measured to the south. For the potential function used by Dr. Woodward (*Astronomical Journal*, Nos. 651–52), $(\partial g/\partial\xi)_0 = -8.14 \times 10^{-9}$ $\sin 2\phi_0$ and hence, since $\omega^2 = 5.3173 \times 10^{-9}$ we have for this potential function

$$TC' = 2.49 \times 10^{-9} \sin 2\phi_0 \cdot \frac{h^2}{6g_0},$$

which for the data (5) yields

$$TC' = +.0010 \text{ cm.}$$

[13] It is not difficult to show that

$$P_1T = -\left(\frac{\partial g}{\partial \xi}\right)_0 \cdot \frac{h^2}{g_0},$$

where the terms have the same meaning as in the preceding foot-note. Consequently

$$P_1C' = P_1T + TC' = \left\{ 2\omega^2 \sin 2\phi_0 - 5\left(\frac{\partial g}{\partial \xi}\right)_0 \right\} \cdot \frac{h^2}{6g_0}.$$

This formula I proved for the first time in the *Transactions of the American Mathematical Society*, Vol. XII., No. 3, pp. 335–53. See also Vol. XIII., pp. 469–90, *Astronomical Journal*, Nos. 670–72 and *Bulletin of the American Mathematical Society*, 2d series, Vol. XXI., No. 9, pp. 444–62. For the potential function used by Dr. Woodward,

$$(\partial g/\partial\xi)_0 = -8.14 \times 10^{-9} \sin 2\phi_0,$$

whence, for that potential function

$$P_1C' = 51.33 \times 10^{-9} \sin 2\phi_0 \; h^2/6g_0,$$

which for the data (5) gives

$$P_1C' = +.021 \text{ cm.}$$

now abandoned, which he made concerning my earlier paper, says:

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, "it is, of course, evident that this function corresponds to some distribution of revolution" in the earth's mass.

I shall reply first to the criticism concerning the "identification of astronomic with geocentric latitude." After having derived (in my first paper) a general formula for the meridional deviation of a falling body, I assigned various particular forms to the potential function and thus obtained the formulæ for the meridional deviations corresponding to these particular potential functions. Some of these potential functions were expressed in terms of astronomic latitude, and others in terms of geocentric. Consequently, the same thing was true of the corresponding formulæ for the meridional deviation. For instance, the formula of Gauss was expressed in terms of astronomic latitude and several others were expressed in terms of geocentric latitude. In order to compare the magnitudes given by the special formulæ I replaced, in the formula of Gauss, the symbol representing astronomic latitude by that representing geocentric, and in so doing I used the expression "with sufficient approximation" for which I am now criticized. It is of course evident that by this procedure a slight error was made in the formula of Gauss *after* its rigorous form had been derived. But none of the other work was thereby affected, the derivation of the general formula as well as that of each of the special formulæ being strictly rigorous. Concerning the criticism about my equations (j) I wish to say that the parameter ψ may be regarded as a geocentric latitude, since it is measured at the center of the spheroid and from the equatorial plane. I did not say that it was the geocentric latitude of the point (τ, σ). However, it would have been well to mention that it is called the reduced latitude of the point (τ, σ). But even if the reader interprets it as the geocentric latitude of the point (τ, σ), the argument in which it is used will not thereby be vitiated. For, as I pointed out, the relation (l) in which it is used is approximate, the relation (n) being the exact relation approximated. Now, the error made in using relation (l) instead of relation (n) is twice as great as the error made in relation (l) by calling ψ the geocentric instead of the reduced latitude of the point (τ, σ). As regards the "surprising statement," I should like to point out that on page 192[14] I defined a distribution of revolution as one for which $\partial V/\partial\lambda \equiv 0$, and surely my function (38) satisfies this condition since it does not contain the longitude λ. Then I was very particular to say—in the last foot-note on page 199—that for the assumption $B \equiv A$ made by Dr. Woodward in his relations (31), his potential function (26) is the same as my potential function (38). Concerning the potential function introduced on page 342 of my first paper, I stated that it had been taken from Poincaré, "Figures d'Equilibre d'une Masse Fluide" (1902), Chapt. V. Following Poincaré, I used the symbol M where Dr. Woodward used the symbol M_K. In other words, I suppressed the gravitation constant. But it was easy to see from the expressions and values of the constants that no error had been made in so doing.

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VEGETATIVE REGENERATION OF ALFALFA

When growing alfalfa plants in the greenhouse, for infection experiments with the crown-gall of alfalfa (*Urophlyctis alfalfæ*), the writer found it desirable to clip the shoots at intervals in order to secure a multiplication of the adventitious buds from the crown.

[14] *Astronomical Journal*, Nos. 670–72.