THE MOST PROBABLE 1930 VALUES OF THE ELECTRON AND RELATED CONSTANTS

By Robert A. Millikan

Norman Bridge Laboratory of Physics, California Institute, Pasadena

Abstract

The conclusions are reached that: (1) No such empirical oil-drop formula as that suggested by H. A. Wilson can be valid, (2) It is highly improbable that the spectroscopic fine structure constant can be a whole number—either 136 or 137, (3) The most probable 1930 values of e, N, and h, are the same as in 1917, corrections due merely to new determinations of the velocity of light and the absolute value of the ohm being alone needed. These values so corrected are

$$e = (4.770 \pm 0.005) \times 10^{-10} \quad h = (6.547\ 0.011) \times 10^{-27} \quad N = (6.064\ 0.01) \times 10^{23}.$$

(4) If Lewis and Adam's theoretical equation is considered valid the probable errors are much smaller than the foregoing estimates.

THE first purpose of this paper is to comment on Professor H. A. Wilson's "Note on the Value of the Electric Charge,"[1] in which he concludes that "the oil-drop experiments do not definitely exclude" a value of e one percent higher than 4.774×10^{-10}; the second purpose is to express an individual 1930 judgment, with the reasons therefor, as to the best values of these constants in view of all that has been done to date in the way of their exact evaluation; the third purpose is briefly to discuss the mutual restrictions which the theorists and the experimentalists impose upon each other; and the fourth is to present evidence bearing on Eddington's contention that the spectroscopic fine-structure constant is a whole number.

I. Oil-Drop Method's Precision

If there were nothing more to the oil-drop work than the finding of some empirical equation that would reproduce the experimental data published in my 1917 paper,[2] then the statement quoted above might have some justification; but the experimental facts are, first, that Professor Wilson's empirical equation merely fits the relatively *short portion* of the oil-drop curve which corresponds to these data, and shoots above a considerable number of *observed points* farther down on the curve, that is, corresponding to smaller values of $1/pa$, then runs into the axis at a point much higher than these observed points permit; second, that it departs very largely from the *observed curve* at its upper end, that is, for large values for $1/pa$. For not only has the complete oil-drop curve from very low to very high values of $1/pa$ been worked out empirically so that we know by experiment

[1] H. A. Wilson, Phys. Rev. **34**, 1493 (1929).

[2] R. A. Millikan, Phil. Mag. **34**, 1 (1917).

that no such equation as that given by Professor Wilson is valid, but also—
and this is more important—in view of a group of papers, some by myself,[3]
partly experimental and partly theoretical, and a masterly one by Epstein,[4]
we now know precisely why Stoke's law of fall breaks down, and why the law of
fall embodied in the oil-drop equation takes its place.

I shall later in the paper have reason to insist that the theorist is bound to
build his theories so that they do not collide head on with experimental
facts; but at this point my insistence is that the experimentalist is not justi-
fied in setting up an empirical equation which makes Nature behave in a way
which established and well verified theory tells him that she cannot behave.
With this my comment upon Professor Wilson's paper is ended, and possibly
no further remarks on e, h, and N would be needed had not Professor Zwicky's
recent work on the secondary or block-structure of crystals gone far toward
clearing the atmosphere surrounding these constants since Birge's masterly
review[5] of last July appeared. This may perhaps justify the following brief
historical review and new appraisal of where these constants now seem to
me to stand.

II. Changes in e, h, and N in Fourteen Years

Fourteen years ago, at the conclusion of seven years of work on the ex-
perimental evaluation of these three constants, the electron, the Avogadro
number, and Planck's quantum of action, the following were published as
the most reliable values then attainable:[2]

$$e = 4.774 \pm .005 \times 10^{-10}$$
$$h = 6.547 \pm .006 \times 10^{-27} \tag{1}$$
$$N = 6.062 \pm .006 \times 10^{-23}$$

and these values have been incorporated into most of the tables and texts
compiled since that time; but quite recently some uncertainty has arisen,
and there has been a widespread discussion of these constants, stimulated,
in the first instance, by Eddington's theoretical suggestion as to what ought
to be the value of the spectroscopic fine-structure constant, and in the sec-
ond place by the more recent experimental work of Bäcklin,[6] Wadlund,[7] and
Bearden[8] on the comparison of x-ray wave-lengths as determined by ruled
gratings and by crystals. After this discussion, the following is where the
matter now stands from the viewpoint of an experimentalist,—that is, the
man who is engaged in making the measurements themselves and who has
the feeling and the judgment that possibly only the observer can have about
the relative reliability of the readings involved.

[3] R. A. Millikan, Phys. Rev. **21**, 67 (January 1923); also **21**, 217 (March 1923); also **22**,
1 (July 1923).

[4] P. S. Epstein, Phys. Rev. **23**, 710 (June 1924).

[5] Raymond T. Birge, Phys. Rev. Supplement **1**, 1 (1929).

[6] E. Bäcklin, "Absolute Wellenlängenbestimmungen der Röntgenstrahlen," Uppsala Dis-
sertation, 1928.

[7] A. P. R. Wadlund, Proc. Nat. Acad. Sci. **14**, 588 (1928), and Phys. Rev. **32**, 841 (1928).

As to the electron, I think it is generally agreed that the oil-drop method still remains the chief, if not the sole, reliance of the physicist, so far as directness, reliability, and precision are concerned, in arriving at its value. I had myself hoped, and expected, that the comparison of wave-lengths as determined by ruled gratings and by crystals would lead to greater precision in the fixing of both N and e, but I now feel sure that both (1) the experimental results obtained by this method, and (2) Zwicky's theoretical work (Proc. Nat. Acad. March, 1930) on the secondary structure of crystals, show that this method cannot have anything like the reliability which we had hoped that it would show, and therefore that all of the methods which depend upon crystal grating-space, as computed from density and N, had better be left out of the accounting.

No new work has been done by the oil-drop method that is comparable in elaborateness nor precision with that published in 1917, but the fundamental constants entering into that method, such as the velocity of light and the value of the ohm, have been redetermined within that period, so that *the 1917 data, treated precisely as it was then treated, but combined with Michelson's new value of the velocity of light, namely, 2.99796 instead of 2.999, and the absolute value of the ohm* (my international volts thus are raised 1 part in 2000 to reduce them to absolute volts) *yields*

$$e = (\mathbf{4.770 \pm .005}) \times 10^{-10} \text{ absolute electrostatic units}^9 \qquad (2)$$

in place of $(4.774 \pm .005) \times 10^{-10}$. *The foregoing is, then, merely the old oil-drop value of e* brought up to date by inserting new values of the velocity of light and the value of the ohm.

This value of the electron is also that at which Birge finally arrives as a result of his survey of the whole field of fundamental constants. It is true that he reanalyzes for himself my individual oil-drop readings and weights them so that he gets from them the value 4.768 ± 0.005 in place of my value

[8] J. A. Bearden, Proc. Nat. Acad. Sci. **15**, 528 (1920).

[9] The following quotation is made from an article published in Science on May 10, 1929:

"The reason I have not heretofore made the foregoing readjustment in my value of e is, first, that it is of no particular significance any way, since it is in any case within the limits of my estimated uncertainty; and, second, that I have until recently doubted its legitimacy.

"In the presentation of the best values of widely used physical constants I have heretofore questioned the wisdom, or even the correctness, of making a differentiation between so-called international units and absolute units before a suitably authorized international commission had recognized that difference, since otherwise such differentiation would rest merely upon some individual's *estimate* of the superior reliability of some particular new determination or determinations over the weighted mean of the whole series of determinations used by the international commission which in 1908 and 1911 fixed upon the international units. However, Professor Raymond T. Birge has called my attention to the fact that in view primarily of the close agreement between new determinations of the absolute value of the ohm by F. E. Smith (*Phil. Trans.*, 1914) and Grüneisen and Giebe (*Ann. d. Physik*, 1920), the compilers of tables have actually recently begun to make the foregoing differentiation. It is because of this fact and because of Michelson's undoubted new precision in the measurement of the velocity of light that I have thought it worth while to begin herewith to recognize the effect of these changes upon the value of e."

4.770 ± 0.005, a result that is so much nearer mine than my experimental uncertainty that I am quite content—indeed gratified—but I may perhaps be pardoned for still preferring my own graphical weightings, since I thought at the time, and still think, that I got the best obtainable results in that way from my data. The person who makes the measurements certainly has a slight advantage in weighting over the person who does not, and the *graph-ical method by which I got at my final estimated uncertainty* is, I think, in the hands of the experimenter himself more dependable than least squares. A glance at the 1917 paper will show that I used least squares only to exhibit how well my observed points were distributed about my final graphically chosen *line*. Birge actually chooses for his final value my own foregoing value, 4.770, instead of 4.768, because he attaches enough weight to Wad-lund's work to induce him to push the latter value up by 2 parts in 5000. Wadlund's work, however, must now, I think, be thrown entirely out of account, both because Bearden's repetition of it yielded a full percent of divergence from it and because Zwicky's theoretical block-structure work indicates that the method itself involves up to a percent of uncertainty.

The photoelectric evaluation of h, as reported in this same 1917 article, was $(6.56 \pm 0.03)10^{-27}$, but *it was definitely stated in that article* that the value of this constant derived from the Bohr equation

$$R_\infty = \frac{2\pi^2 e^5}{h^3 c^2 e/m} \tag{3}$$

was in my judgment the most reliable value then obtainable, the value then resulting from inserting in the foregoing equation $e = 4.774$ and $e/m = 1.767$ was as given in that paper

$$h = (6.547 \pm 0.01) \times 10^{-27} \tag{4}$$

Birge, from his recently published survey, comes to precisely this 1917 value with practically the same estimated error, namely, 0.008, so that these fourteen years have not changed at all the final result as to h. The reason that h remains unchanged while my e suffers a slight decrease is that the new work done by Babcock[10] and Houston[11] changes e/m as spectro-scopically determined to 1.761, while the value used in the 1917 computations was 1.767, and this change just balances the foregoing change in e. The pres-ent numerical equation is

$$h = \left[\frac{2\pi^2(4.770 \times 10^{-10})^5}{109737.42(2.99796 \times 10^{10})^2(1.761 \times 10^7)} \right]^{1/3} = 6.5471 \times 10^{-27} \text{erg. sec.} \tag{5}$$

The Avogadro number Birge estimates as 6.064 ± 0.006 in place of the 1917 value, namely, 6.062 ± 0.006. In view of the estimated error in both cases the difference of course has no significance whatever. The reason the agreement is so good is that new work on the electro-chemical equivalent

[10] Babcock, Phys. Rev. **33**, 268 (1929).

[11] Houston, Phys. Rev. **30**, 608 (1927).

leads Birge to choose to modify the Faraday constant in such a way as nearly to offset the influence of the slight change in e on the value of N.

So far, then, as the foregoing estimates by both Dr. Birge and myself are to be depended upon, the last fourteen years have introduced no change into the values of e, h, and N as experimentally determined, save the insignificant one in e due to new precision in the velocity of light and the value of the ohm.

III. Theoretical Relationships

What, then is to be said of the demands that the theorists have made upon the relations of these constants? This is a very important and a very interesting question. There are two such relations to be considered, one brought forward by Lewis and Adams[12] in 1914 and one by Eddington[13] in 1929. With respect to the first, I have nothing to add to Birge's comments, except to emphasize them. Lewis and Adams, from their theory of ultimate rational units arrive at the following relation between h, c, and e:

$$\frac{hc}{2\pi e^2} = 8\pi\left(\frac{8\pi^5}{15}\right)^{1/3}. \tag{6}$$

It will be seen that the right side of this equation involves no physical measurements whatever. It has the value 137.348. The left side, which is Eddington's spectroscopic fine-structure constant $1/\alpha$, contains three physically measured quantities, and if the values $e = 4.770 \times 10^{-10}$, $h = 6.547 \times 10^{-27}$, and $c = 2.99796 \times 10^{10}$ be inserted, *it yields 137.29 in exceedingly close agreement with the right side.*

This Lewis and Adams theoretical relationship is introduced here merely to emphasize the fact that if it is correct, then without recourse to any experiment at all the spectroscopic fine-structure constant—the left side of the foregoing equation—cannot possibly be a whole number, as contended by Eddington. Also, the remarkable agreement of the left side with the demands of the right side lends rather extraordinary experimental credentials to the Lewis and Adams relationship, although I believe that in other particulars it has not behaved itself so well.

But, turning next to experiment alone, what has it to say as to Eddington's suggestion that the spectroscopic constant $1/\alpha$ ought to be a whole number? The number he originally suggested was 136, but, as Birge pointed out, this suggestion was in irreconcilable conflict, first, with the oil-drop results on e; second, independently of e, it was in conflict with Houston's and Babcock's determinations of the spectroscopic value of e/m; and, third, it collided with Lewis and Adams' equation. The combined weight of this attack was so great that Eddington's suggestion could not possibly be entertained, provided the principle be admitted that the theorist cannot be permitted to ignore the facts in building his theories.

Quite recently, however, Eddington has changed the theoretical value of his fine-structure constant, still keeping it a whole number, but now

[12] G. N. Lewis and E. Q. Adams, Phys. Rev. 3, 92 (1914).
[13] A. S. Eddington, Proc. Roy. Soc. A122, 358 (1929).

making it 137^{14} instead of 136. This might perhaps remove the clash with my experimental oil-drop work, for if h were kept at 6.547 and e pushed up to 4.775—a change only a trifle outside the limits of my estimated experimental uncertainty—$1/\alpha$ would come down from 137.29 to 137. But quite independently of the oil-drop work on e, Eddington would still be in what appears to be an irreconcilable clash with the spectroscopic measurements on e/m and the following series of measurements on the ratio of h/e. To see this, write with Birge the spectroscopic equation in the following form:

$$R_\infty = \frac{\alpha^2}{2}\left(\frac{e}{h}\right)\frac{1}{e/m}.$$

This fits perfectly with $1/\alpha = 137.29$, $e = 4.770$, $h = 6.547$, and $e/m = 1.761$.

If, then, α^2 is to be pushed up the 0.42% necessary to make $1/\alpha$ equal to 137, then if e/h remains constant, e/m, spectroscopically determined, must go up 0.42%. But e/m, spectroscopically determined, and it is this mode of evaluation which obviously must be used for substitution in a spectroscopic equation, is now known to about one part in a thousand, for not only are both Babcock's and Houston's published determinations extraordinarily precise, but Houston and his associates inform me that the new independent determination that they are now making appears to check with the other two. If, then, there is no flexibility in e/m, the factor e/h must go down 0.42% or h/e up 0.42% to match the 0.42% rise in α^2.

Now h/e has been determined by four different methods, none of which involve the properties of crystals: (1) the photoelectric method; (2) the ionizing potential method; (3) the c_2, or Wien displacement-law method; and (4) the σ, or Stefan-Boltzmann-law method. *Not one of these methods will permit h/e to go up 0.42%.* The best result to date by the photoelectric method—which involves simply the equation $h\nu = eV$—is probably my 1916 value of h/e for sodium; for the length of the line, the slope of which gives h/e, was several times greater than that used by other observers, and in addition the currents were large and stopping potentials accurately determined. The value of h as published both in my 1916[15] and 1917[16] papers, and in "The Electron"[17] (1917) was $(6.56 \pm 0.03) \times 10^{-27}$, and this remains the value when absolute units, Michelson's new c, and $e = 4.770$ are used. Lukirsky and Prilezaev's recent determination[18] made with the metals Al, Zn, Sn, Ni, Cd, Cu, and Pt yields a mean 6.543×10^{-27}, and there has just appeared a paper by Olpin[19] which yields $h = 6.541 \times 10^{-27}$ "significant to three figures." I shall take the mean of these three, 6.560 and 6.543 and 6.541 namely 6.548, as the most probable value of h which combines with $e = 4.770$ to make the best photoelectric value of h/e.

[14] A. S. Eddington, Proc. Roy. Soc. A126, 696 (1930).

[15] Millikan, Phys. Rev. 7, 373 (1916).

[16] Millikan, Phil. Mag. 34, 14 (1917).

[17] Millikan, "The Electron" (1917), p. 227.

[18] Lukirsky and Prilezaev, Zeits. f. Physik 49, 236(1928).

[19] A. R. Olpin, Phys. Rev. 35, 670 (1930).

The ionizing potential method, also involving merely $h\nu = eV$ is most accurately used by Lawrence,[20] who gets, corresponding to $e = 4.770$, the value $h = 6.560 \pm 0.015$.

I have nothing to add to Birge's estimates of h/e from the Wien Displacement Law method, which corresponds to $h = 6.548 \pm 0.015$. The equation here is $c_2 = hc/k$ where k is determined from, and is proportional to e, so that h/e has here the same error as c_2 and $c_2 = 1.432 \pm 0.003$ cm deg.

The Stefan-Boltzmann Law method does not yield h/e directly, but it does give, through Planck's equation, $h/k^{4/3}1 \propto /\sigma^{1/3}$ and although the variation in the experimental values of σ is large, since the error in $h/k^{4/3}$, or $h/e^{4/3}$, is only one-third that in σ, I am willing to accept Birge's estimate that the probable error in the value of h corresponding to $e = 4.770$ as given by this method is $h = (6.539 \pm 0.010) \times 10^{-27}$.

The mean of the values 6.548, 6.560, 6.548, and 6.539 by these four methods is 6.549, which is very close to my 1917 value 6.547, and *the foregoing analysis shows how extraordinarily unlikely it is that h/e can go up the 0.42% necessary to permit $1/\alpha$ to be 137*, as Eddington wishes it to be, so that even when the theoretical evidence from Lewis and Adam's equation is entirely discarded the *experimental situation alone* renders it highly improbable that Eddington's conclusion can be correct.

If, however, the experimental credentials which have thus far developed for Lewis and Adam's relation are considered significant, then the values of e, h, and N are probably considerably more accurately known than the foregoing estimates of probable experimental error indicate. If both Eqs. (5) and (6) are to be considered correct, then, since (5) is of the form $h/e^{5/8} \propto 1/e/m$, and (6) of the form $h/e^2 = $ constant, when I compare them with the experimental limits of error of h/e I find that the possible range of variation in e, h, and e/m is very small indeed, and I should then write the most probable values of e, h, and e/m as follows:

$$e = (4.769 \pm 0.001) \times 10^{-10}$$
$$h = (6.547 \pm 0.001) \times 10^{-27}$$
$$e/m = (1.7595 \pm 0.001) \times 10^{7}.$$

It will be interesting to see whether new experimental work can check this prediction as to the value of e/m.

However, in view of the theoretical uncertainty in Lewis and Adams' relation, I think it safer not to depend at all upon it, and hence to continue to write, as representing the best 1930 experimental situation, the 1917 values merely corrected for volts and for speed of light, namely,

$$e = (4.770 \pm 0.005) \times 10^{-10}$$
$$h = (6.547 \pm 0.010) \times 10^{-27}$$
$$N = (6.064 \pm 0.006) \times 10^{-23}.$$

[20] E. O. Lawrence, Phys. Rev. **28**, 947 (1926).