THE NATURE OF THE EVIDENCE FOR THE DIVISIBILITY OF THE ELECTRON

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Abstract

The purpose of this note is to remove misconceptions due to erroneous statements regarding the determination of the elementary electronic charge by the oil-drop method. It is emphasized that the existence of an elemental charge is directly proved by the multiple relationship shown to exist between successive charges on each test particle; that the unitary character of electricity is not presupposed even in determining the absolute value; and that the method used by Sexl in determining the radius of the droplet is not essentially new. Recent photo-electric work of Hake and of Wasser is discussed briefly.

FOR the past eight years I have taken no part in the discussion of the nature of the evidence for the divisibility of the electron, since it has seemed to me that the published data told the story quite plainly to any one who would take the trouble to examine them critically. Further, I stated the case as clearly as I could in 1916[1] and no evidence has appeared since to modify the conclusions then drawn, or, indeed even to need further discussion, save only the very recent photo-electric work of Hake[2] and of Wasser,[3] which will be touched upon below.

I believe that every one of the observers outside of Vienna who has repeated my work has both checked my experimental results and reasserted my conclusions upon all the essential points in dispute,[4] while even in Professor Ehrenhaft's laboratory itself one observer, Schmid,[5] has found, like all the rest of us, that measurements upon the Brownian movements of minute suspended particles in air do not lead, as formerly asserted in Vienna, to too low a value of Ne, while another observer, Mattuck,[6] has found by using my oil-drop method without any essential modification, that my results, both as to the complete law of fall of a spherical particle and as to the uniform appearance upon all particles, little and big, of the charge of about 4.7×10^{-10} electrostatic units, were correct within the limits of his rather large experimental error.

[1] Millikan, Phys. Rev. **8,** 595-625 (1916).

[2] Hake, Zeits. f. Phys. **15,** 110 (1923).

[3] Wasser, Zeits. f. Phys. **27,** 226 (1924).

[4] See Bär, Ann. der Phys. **67,** 157 (1922); also Die Naturwissenschaften **14** and **15,** 1922.

[5] Schmid, Zeits. f. Phys. **5,** 31 (1921).

[6] Mattuck, Phys. Zeits. Dec. 1, 1924, p. 620.

Scarcely any further extensive discussion of the sub-electron problem then seems necessary now, but it is perhaps worth while to correct, for the benefit of those who have not the time to study the original data, some fundamental misconceptions which may have been gained from erroneous statements which have been repeatedly made in the course of this discussion and which are reasserted by Sexl.

1. *Nothing could be more fundamentally incorrect than the statement that my method presupposes an equality of charge on all test particles, i. e., the existence of an elemental charge.* It is altogether obvious that no assumption whatever regarding the character of the charge e on the droplet is involved in my fundamental equation

$$\frac{v_1}{v_2}=\frac{mg}{Fe-mg}$$

or

$$e=(mg/Fv_1)(v_1+v_2)=K(v_1+v_2).$$

This equation shows that the charge e on the drop, whatever its value, is proportional to the velocity communicated to the drop, namely (v_1+v_2) by the constant field F acting on the charge e. In other words, this charge e is measured in my method in terms of the speed (v_1+v_2). The essence of the method consists in *changing* the charge and hence v_2 by capturing ions, by photo-electric discharge, by x-rays, α, β and γ rays, etc., and in finding by such change all the values of (v_1+v_2) that can be obtained. The atomic nature of electricity is revealed conclusively in the experimental fact that this series of possible speeds is actually found to bear the relations 1, 2, 3, 4, 5. *It is in this purely experimental multiple relationship in the speeds of drops all of which capture the same sort of ions from the air that the proof of the atomic theory of electricity rests, and it is at this point that the attack on the atomic theory of electricity must be made if it is to be worthy of any serious consideration whatever.*

Now, so far as I know, no one has ever published any data that are susceptible of careful analysis for this multiple relationship and that yet fail to reveal it. Further, no data should be considered as evidence in which the charge upon a given drop is not changed enough times to test thoroughly the existence or non-existence of this multiple relationship. In so far as I can see from the rather meager data that Hake and Wasser publish in their recent photo-electric work, their drops do show this unitary progression of charge, and if they do then it may be taken as practically certain that these gentlemen are not dealing with sub-electrons, for I have demonstrated conclusively that the charges caught from ionized air, such as some of the observed charges will always be if the observations are long continued, are uniformally electrons, and I think it is not too much to assert that they must be electrons in Vienna if they are such in Pasadena and Chicago.

It is true that I do not yet understand fully what Wasser calls his "inverse photo-electric effect," but it seems to me likely that he obtains this effect by catching electrons released from the surrounding walls by ultraviolet light instead of by detaching positive charges from his droplet by ultraviolet light, as he thinks that he does. But this point can be tested easily, and we are in the process of making this test in this laboratory. By changing the charge on a given drop, first with the aid of ultraviolet light and then by throwing upon it ions produced by the passage of a beam of x-rays underneath the drop, it will be possible to observe directly whether these two changes in charge produce the same change in speed. The question of the appearance of the sub-electron in these experiments can then be settled definitely without making any assumptions whatsoever about the densities of the drops worked with.

2. *It is fundamentally erroneous to suppose that even in the reduction of the value of the electron from velocity units to absolute electrostatic units—an operation which does presuppose the density and sphericity of the droplet—any presupposition whatever as to the unitary character of electricity is involved.* A glance at any of my early papers upon this subject will show that the radius a of the drop was determined entirely from Stokes' law—or from a slightly corrected form of Stokes' law—and involves no suppositions of any kind as to the nature of the electrical charge. The two-thirds' power of the smallest charge (measured in terms of a speed) *that the drop actually carried*—this smallest charge could be unerringly computed from the observed multiple relationship in speeds, but not even this computation was necessary since plenty of drops could be obtained upon which it was *directly observed*—was plotted against $1/pa$, a being computed from Stokes' law, viz: $mg=6\pi\mu av_1$. This plotting yielded a single straight line. It is quite true that it could not have done this unless all of the drops, when most lightly charged, carried one and the same unit charge, as the multiple relationship had already shown that they did, *but the straight line, like the multiple relationship, is an experimental fact* from which the unitary character of the charge follows. *It is not an assumption.* The absolute value, too, of the electron was now approximately determined by the intercept of the straight line on the $e_1^{2/3}$ axis. This line also yielded, through its slope, the approximately correct value of the first correction term, A, to Stokes' law, which thus became $mg=6\pi\mu av_1(1+Al/a)^{-1}$. This new equation, with A now approximately known, was then solved to obtain a final value for a for use in the more

exact computation of e. All this was carefully explained in all my early papers.[7] It is true that in my later papers,[8] after the existence of the electron had been demonstrated and its value accurately determined, I found it in the interests of both convenience and accuracy to use the the value of e for working back to a, and this I often did, but it was in no way essential to my method and was actually never used until the value of e had been accurately found without it. *In other words, my method of determining the size of the carrier of the electric charge is in fact completely independent of every assumption about the value of this charge.*

3. It is fundamentally erroneous to suppose that the method used by several observers, including Sexl, of determining the radius of the drop by assuming its sphericity and density and then measuring its speed of fall at two different pressures is in any important respect an *essential* modification of my method, for *there is no relationship here involved that is not included in my linear relationship between* $e_1^{2/3}$ *and* l/a, *or* $1/pa$. I exhibited my results in this single-line form because it is by far the most comprehensive and by far the most elegant mode of treatment of oil-drop data. It is simply Stokes' law, with a first correction term added, that requires the linear relation between v_1 and l, mentioned by Sexl, and it is the same law plus the unitary character of electricity out of which grows the linear relation between $e_1^{2/3}$ and l/a. The straight line between v and l is a different one for each drop because the drop-radius has not yet been eliminated from it. By its elimination in the $e_1^{2/3}$, l/a line all of these v, l lines have been reduced to a single one, as is beautifully shown in my published data.

Again, after I had changed both pressure (l) and drop-radius a and found that 99 percent of my drops fell upon the single $e^{2/3}$, l/a line it obviously became superfluous to change l alone as these other observers do, for if a point cannot get off the line by varying at random both l and a it clearly cannot do so by varying l alone and holding a constant.

Further, the reason that in my early experiments I did not change l alone in this way—an operation which takes a relatively long interval of time—was that on account of the slow change in size of all drops, especially of mercury drops, this method was less accurate than the one I did use of changing both l and a and thus getting a group of velocity-measurements close together in time and hence practically free from evaporation errors. If an occasional one of my points fell entirely off this line it meant, as I pointed out fifteen years ago, simply that this

[7] Millikan, Phys. Rev. (1) **32,** 379 (1911)
[8] Millikan, Phil. Mag. **34,** 3 (1917)

drop had not the assumed density or sphericity. It might, indeed, conceivably have meant, had the observation stood by itself, that the charge carried was a fraction of an electron while the assumed density and sphericity were correct. This was the way Ehrenhaft chose to interpret his irregularities, *but this alternative was barred out by the observed multiple relationship in speeds* and also by the fact that *Ne* when computed from Brownian movements came out as in electrolysis.[9] It became possible at once in the case of a drop that fell off my line, to obtain the correct equivalent density of a spherical drop by inserting such a density-value as would make the drop fall on that line. This is precisely what I did as early as 1911, and it is essentially what these other more recent observers, including Sexl, do when they work back to density or drop-radius from their v, l line, except that they ignore evaporation and in the case of mercury at least introduce large errors thereby.

It is true that Meyer and Gerlach[10] and Bär[11] performed an important service by computing the densities of particles produced by Ehrenhaft's method from the linear v, l relation instead of the linear $e_1^{2/3}$, l/a relation, such as I used, for though the two methods, in view of my multiple relationships, must yield the same results, barring evaporation, Meyer and Bär's result is, as they point out, entirely independent of the existence upon their particles of any charge at all, so that when they find by their procedure, as I had done from mine, that Ehrenhaft's assumed densities are entirely wrong they remove from him the possibility of calling upon any electrical assumption whatever, for explaining his irregularities. In other words, Meyer and Bär added a third demonstration, independent of the two that I used as mentioned above, that Ehrenhaft's irregularities are due to the assumption of incorrect drop densities. It may be added, too, that there is no apriori reason why some of Hake and Wasser's droplets may not be made up of clusters of particles of little more than molecular dimensions even though they are formed from the condensation of pure mercury, and such clusters may conceivably have a different long wave-length limit from that of mercury in mass. For it will be remembered that it requires light of wave-length about 1200 angstroms to detach electrons from mercury molecules, while the long wave-length limit of liquid mercury is 2635 angstroms. This point, however, will be settled by experiments now in progress.

In Derieux's work upon mercury, it was found impossible—as he clearly stated—to prevent evaporation, but by taking a series of consecutive

[9] Millikan, Phys. Rev. **8,** pp. 610-11 (1916).

[10] Meyer and Gerlach, Ann. d. Phys., **47,** 227, 1915.

[11] Bär, Ann. der Phys. **59,** 393 (1919) and Ann. der Phys. **67,** 157 (1922).

observations in rapid succession he was able to render its influence small and he then found, by assuming his drops spherical and of density 13.56, that all of his points fell close to a straight line of slope $A = .708$ and having an intercept at the correct value for the electron. It then follows, as a matter of necessity, that if there had been no evaporation, and if the points had all been exactly on the .708 line, Sexl could not get by using Bär's method of computing drop-radius and density as he does (working back from the observed v, l line) different values of a and σ from those used by Derieux, unless one or the other of them made numerical blunders. It is not important to search for such blunders since Sexl's critique of Derieux's results is completely without significance because of his astonishing disregard of evaporation in spite of Derieux's full discussion of it.

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