THE EXISTENCE OF A SUBELECTRON?

By R. A. Millikan.

1. The History of the Idea of a Unit Charge.

It was in the year 1833 that Faraday's discoveries in electrolysis first suggested the existence of an elementary electrical charge, and first made possible a rough estimate as to its value. This estimate was first carried through in 1874 by G. Johnstone Stoney,[1] who first used the word *electron*[2] as a name for the "natural unit of electricity" and who published as its value $.3 \times 10^{-10}$ E.S. units, a value obtained from electrolytic determinations of the product Ne and from kinetic theory estimates of N, the number of molecules in a gram molecule. These kinetic theory estimates vary ten fold, but if we take the value which was most current about 1900, namely that given in O. E. Meyer's well-known book[3] we obtain by Stoney's method $e = 2 \times 10^{-10}$.

The laws of Faraday were not regarded, however, even by the keenest of intellects as demonstrating in general the atomic structure of electricity, for it was entirely logical to attribute the exact multiple relations shown by the charges appearing in electrolysis to properties of the atoms carrying these charges rather than to an atomic property of electricity itself. This was the course actually taken by Maxwell,[4] who expressed himself positively as opposed to a general atomic theory of electricity. Furthermore, Faraday, Helmholtz[5] and Kelvin[6] all showed clearly that they did not regard the apparent ionic charges existing in electrolytes as necessarily existing in separate elements on charged metals. Indeed a sharp distinction was practically universally made up to 1900 between the phenomena of metallic and those of electrolytic conduction.

[1] Trans. Roy. Dublin Soc., 4, p. 582, 1891. Also Phil. Mag., 1881, p. 385.

[2] The most authoritative of modern writers such as Thomson, Rutherford, Richardson, Campbell, etc., have been careful to retain the original significance of the word electron instead of using it to denote solely the free negative electron or corpuscle of Sir J. J. Thomson. These writers all speak of positive as well as negative elections although the mass associated with the former is never less than that of the hydrogen atom.

[3] Die Kinetische Theorie der Gase, 1899. The number of gas molecules per c.c. is here given as 6×10^{19} which corresponds to $N = 1.34 \times 10^{24}$.

[4] Electricity and Magnetism, 1873, p. 380 and 381.

[5] Helmholtz's Wissenschaftliche Abhandlungen, Vol. 3, p. 69.

[6] Nature, Vol. LVI., p. 84, 1897.

The experiments made at the Cavendish Laboratory[1] about 1900 by Townsend, Thomson, Zeleny, Rutherford, H. A. and C. T. R. Wilson and others simply showed that gaseous conduction is of the same kind as electrolytic conduction, but did not throw any new light on the nature of metallic conduction. They did, however, give great stimulus to the atomic theory of electricity and caused it to become the prevalent mode of interpreting electrical phenomena. They brought to light the existence of a body, J. J. Thomson's corpuscle, for which the value of e/m was 1/1830 of that found on the hydrogen ion in electrolysis. Townsend,[2] J. J. Thomson,[3] H. A. Wilson,[4] Przibram,[5] Millikan and Begeman,[6] Ehrenhaft[7] and Broglie[8] in succession made rough determinations or estimates of the average charge appearing on gaseous ions and found it equal, within the limits of uncertainty (say one or two hundred per cent.), to the value estimated for the univalent ions in electrolysis.

2. Isolation of Individual Droplets and the Measurement of Their Charges.

None of the methods used by any of these observers were capable, however, of yielding anything more than the *mean* ionic charge. That the ionic charges in both solutions and in gases were all alike was commonly assumed but could not be proved. Ehrenhaft, in a paper read at the Naturforschersammlung zu Königsberg in Sept., 1910, made a very clear statement of this defect in his own attempted determination of e, and asserted that it inhered also in the work of all other observers. *As a matter of fact, however, I had had the good fortune to find a way of removing this limitation entirely more than a year earlier, viz., in the spring and summer of* 1909.[9] This had been done by isolating in a vertical electric field individual charged water droplets and determining the amount of electricity carried by each drop by measuring (1) the speed under gravity, (2) the speed under the combined action of the field and gravity. The following is a quotation from this article written October 9, 1909:[10] "It is an exceedingly interesting and instructive experiment to

[1] J. J. Thomson's Conduction of Electricity through Gases, 1906.
[2] Proc. Cambridge Phil. Soc., 9, p. 244, 1897.
[3] Phil. Mag., 46, p. 528, 1898.
[4] Phil. Mag., 5, p. 429, 1903.
[5] Phys. Zeit., Juli, 1907.
[6] Phys. Rev., XXVI., p. 198, 1908.
[7] Phys. Zeit., 10, p. 308, 1909.
[8] Le Radium, 6, p. 203, 1909.
[9] This method and the results were reported at the British Association meeting in Winnipeg in August, 1909, being placed upon the program as an additional paper. They were first published in brief in the Physical Review, Vol. 29, p. 260, 1909, and in full in the Phil. Mag. for Feb., 1910, Vol. 19, p. 209.
[10] Phil. Mag., 19, p. 219.

watch one of these drops start, and stop, or even reverse its direction of motion, as the field is thrown off and on. I have often caught a drop which was just too light to remain stationary and moved it back and forth in this way four or five times between the same two cross-hairs, watching it first fall under gravity when the field was thrown off, and then rise against gravity when the field was thrown on. *The accuracy and certainty with which the instants of passage of the drops across the cross-hairs can be determined is precisely the same as that obtainable in timing the passage of a star across the cross-hairs of a transit instrument.*

"Furthermore, since the observations upon the quantities occurring in equation (4) are all made *upon the same drop* all uncertainties as to whether conditions can be exactly duplicated in the formation of successive clouds obviously disappear. There is no theoretical uncertainty whatever left in the method unless it be an uncertainty as to whether or not Stokes' law applies to the rate of fall of these drops under gravity. The experimental uncertainties are reduced to the uncertainty in a time determination of from three to five seconds, when the object being timed is a single moving bright point."

A comparison of the charges obtained by this method showed that within the limits of experimental error, they were 2, 3, 4, 5 and 6 times a particular charge which obviously had to be the smallest charge which appeared in the gaseous ionization which I was studying. The value of this charge, according to the simple mean of my measurements was 4.70×10^{-10} electrostatic units.

Professor Ehrenhaft a year later published the above mentioned Königsberg paper[1] in which he discussed my 1909 work and claimed that I had not determined the charges on individual particles, but that he had now devised a method of doing so. His words are: "Bei dieser ersten Ausführung [that of 1909] war es mir nicht gelungen an ein und demselben Metallteilchen elektrische Beobachtung und Fallbeobachtung hintereinander zu machen. Erst die hier eingeschlagene Weg gestattet, bis on die Grenze der Ultramicroscopie, die Geschwindegkeit der Steigbewegung eines Einzelteilchens unter Einfluss einer greigneten Spannung und so dann *an eben und demselben Teilchen* die Fallgeschwindigkeit desselben partikels unter blossem Einflusse der Erdschwere bei Kurzgeschlossenem Kondensator zu messen."

Professor Ehrenhaft's new arrangement, then, and his new method of

[1] Its first appearance in print was in somewhat modified form in the Juli 15, 1910, number of the Phys. Zeit., p. 619, under the title "Ueber eine nene Methode zur Messung von Elektrizitätsmengen an Einzelteilchen deren Ladung die Ladung des Elektrons erheblich unterschreiten, etc."

observation were identical in every essential particular with those which I used in the paper which he was criticizing, but quite distinct, as he himself says, from anything which he had used in his earlier work. For, prior to the 1910 paper[1] Professor Ehrenhaft had never used a vertically directed electric field[2] and hence, as he correctly pointed out, he had not been able to find the charge on a particular particle. The reason which he assigned for contending that I too had not determined the charges on individual particles, was that in my summary of results, I had taken my values of $2e$, $3e$, etc., from observations in each case upon several drops which carried the charges $2e$, etc. I had done this, however, only for such drops as were obliged, because they were exactly balanced by exactly the same electric field, and fell at the same rate under gravity, to be of exactly the same size and to carry exactly the same charge. Balanced drops having different charges fell with totally different speeds when the field was thrown off, and therefore, never came into consideration. What I actually did was neither more nor less than is always done in obtaining an accurate measurement of any physical magnitude, for example, a length, namely to make exactly the same measurement several times over, and then take a mean solely *for the sake of diminishing the error in reading the measuring instrument.* This instrument was in my case a stopwatch. There was not the slightest reason for considering the fluctuations which Professor Ehrenhaft found in my measurement of e as arising from varying values of the ionic charge, since they were no larger than the necessary fluctuations in a stopwatch measurement of an interval from 2 to 5 seconds in length. Had I worked out e for each individual reading and then taken the mean my result would of necessity have come out exactly as it did. The point raised has to do, therefore, merely with the way in which I tabulated my data, not at all with the way in which I made my measurements, *which were in fact measurements upon the charge carried by individual particles.*

The reason for this effort to find variations in my measurements of the charges carried by different ions lay in the fact that, as soon as in 1910 Professor Ehrenhaft had changed from the de Broglie method of handling his metal particles to the method which I had used with the water drop, and which alone made possible the determination of the charge on a single particle, he found that these charges, as he determined

[1] This appears also in somewhat modified form in the Wiener Berichte of the 12th of May, Bd. CXIX., abt. II*a*, 1910, but this publication does not seem to have appeared till December, 1910, at least it is not noted in "Naturae Novitates" before this date.

[2] Indeed in the 1909 work referred to above he had used precisely the arrangement and method of observation described fully and used in 1908 by de Broglie in his study of charged metal particles coming from arcs or sparks between metal electrodes. See Compte Rendu, 1908, pp. 624 and 1010.

them, showed wide irregularities. These he interpreted as indicating that there is no elementary electrical charge, the smallest charge appearing in preceding work on solutions and gases being nothing but a statistical mean, made up of widely varying charges. The only experiment which had appeared which seemed to negative such a view were these of mine on the charges on water droplets, hence it was necessary to make it appear that these also showed fluctuations.

3. General Proof of the Atomic Structure of Electricity.

In April, 1910, three months before the first appearance in print in the Physikalische Zeitschrift of this paper of Ehrenhaft's, which seemed to undermine all the rapidly spreading views as to the granular structure of electricity, I read before the American Physical Society the paper[1] which seemed to me to establish in a perfectly general way the view that all electric charges, whether on ions or on large bodies, insulators or conductors, are simply an assemblage of elementary electrical specks or atoms all of which are exactly alike. The essential element in this proof lay in these three facts not brought out by any other experiments until 2 or 3 years later: (1) that the ionic charges obtained by capturing ions from gases on any kind of a body are all exactly alike or else small exact multiples of a definite charge; (2) that the static charges residing on all kinds of bodies from insulators up to conductors and put there by frictional or other processes are always exact multiples of this smallest ionic charge. (3) That the direct detachment of negative electrons from the drop by the incidence of X-rays upon it produces the same change in charge as the capture of an ion.[2]

So long as a charged droplet remains constant in shape and size the change in its speed in a given electrical field caused by the capture by the drop of one or more ions is a measure of the charge carried by the captured ion or ions. *These changes in speed were found to be all exactly alike or else exact small multiples.*

Again if the total speed produced in the charged drop by throwing on the given electrical field is found to be always an exact multiple of the smallest change in speed produced by the capture of ions, then the original charge, produced by friction or otherwise, must be built up out of these smallest ionic charges. *This relation was found to be in every case very exactly fulfilled.*

Finally, if the change in speed produced by letting X-rays or ultra-

[1] This paper appeared in print in abstract in Phys. Rev., 31, p. 92, July 15, 1910; also in Science, XXXII., p. 436–443, Sept., 1910; also in Phys. Zeit., XI., p. 1097–1109, 1910. For a more complete article see Phys. Rev., XXXII., pp. 351–397, April, 1911.

[2] See The Electron. Its Isolation and Measurement, etc. University of Chicago Press, 1917.

violet light fall upon the particle and detach negative corpuscles from it is the same as that produced by the capture of ions, then the ionic charge must be the same as the charge carried by the corpuscle or beta particle. Every one of these relations was carefully studied in the winter of 1909 and 1910 and in the paper read in April, 1910, the conclusion was announced that these experiments furnished indubitable proof of the most general and unimpeachable kind of the atomic structure of electricity. This conclusion was drawn only after four months of continuous experimenting from December, 1909, to April, 1910,[1] on drops of many kinds of materials, some of which were good conductors like mercury, some semi-conductors like glycerine, some very bad conductors like oil. Furthermore, these drops were often held under observation for four and five hours at a time, during which period scores of changes in charge in a given drop were produced both by the capture of ions and by the discharge of negative corpuscles from the drop by the direct incidence of X-rays. When it was desired to avoid this direct loss of electrons by the drop, lead screens were arranged so that the drop itself would not be illuminated by the rays although the gas underneath it was ionized by them.[2] We found, too, in 1910 that when we worked at very low gas pressures, changes in charge due to the capture of ions became very infrequent on account of the scarcity of ions, even when strong X-rays were passing between the plates, while changes due to the direct ejection of electrons from the drop by the direct incidence of the rays were as frequent as ever, the result being that at low pressures it is very easy to make the charge on the drop change toward greater positiveness, but next to impossible to make it change in the opposite direction. I did not in this first work, reported in 1910, discharge electrons from the drop by ultra-violet light, but I did discharge them by the direct incidence of both X-rays and gamma rays which were known from J. J. Thomson's and Lenard's work to discharge negative bodies having the same value of e/m as those discharged by ultra-violet light. Joffé[3] and Meyer and Gerlach[4] made in 1913 the first careful study of this case itself, using the balanced drop method precisely as I had used it in 1909 and 1911[5] and found that when they changed the charge on their drops by discharging corpuscles from them by ultra-violet light, they obtained the same sort of exact multiple relationships between the charges as I had found when I produced the changes either by capturing ions, or by discharging corpuscles from the drops with X-rays

[1] See PHYS. REV., 32, p. 360.
[2] See Phil. Mag., June, 1911, p. 757.
[3] Setz. Ber. d. Konig. Bayer, Akad. der Wiss., Feb., 1913.
[4] Arch. de Genève (*d*), 35, p. 398, 1913. See also Ann. der Phys., 45, p. 177, 1914.
[5] Phil. Mag., Feb., 1910, and June, 1911.

or gamma rays. Joffé's and Meyer and Gerlach's real contribution in these papers consisted in showing that the discharges of electrons by ultra-violet light come at irregular intervals and in making important measurements on the mean length of these intervals, called Aufladungszeiten by Joffé and Verzögerungszeiten by Meyer and Gerlach. In addition they check in every particular and very convincingly my conclusions as to the exactness of the multiple relationship which is found between the charges which can be placed upon a metallic droplet.[1]

Unfortunately even when Professor Ehrenhaft made his Königsberg address in Sept., 1910, he knew nothing whatever of this multiple relationship, for he had made no quantitative study of the phenomena of change of charge and he had not yet seen my paper. In all the work reported in 1910, and for that matter in all so far as I can discover which is reported prior to 1914, he records only the moving of a single particle up and back once, and holding it under observation at most a minute, precisely as I had described doing in 1909. The stage to which his experiments had progressed in 1910 can best be seen from the following note added in proof to the before mentioned fifty-two page paper in the Wiener Berichte which appeared in December, 1910, six months after I had read before the American Physical Society the paper in which I gave most elaborate proof of the multiple relationship between charges and described the keeping of a droplet moving up and down between my plates for five hours at a time, during which time I had forced it scores of times to change its charge by the capture of ions or the loss of negative electrons.

"Verfasser ist im Begriffe, mit Hilfe eines die Zeit registrierenden Dreihebelstiftschreibers von Siemens & Halske die beschriebene Messmethod an demselben Teilchen zu wiederholten Malen durchzuführen, um so den zeitlichen Ladungszustand eines und desselben Partikels zu verfolgen. Es gelang dies bis zu vermal and erwies eine ganz ausserordenliche Präzision der Methode. Es scheint vielfach ein kontinuierliches Entladen der Parikel stattzufinden, dies jedoch nach Bruchteilen des Elektronenwertes. Die Resultate, welche vorliegende Schlüsse erhalten, werde ich in diesen Berichten veröffentlichen."[2]

[1] For accurate measurement of the multiple relations between charges, it is best to measure the two speeds, v_1 and v_2, rather than to try to find the field strength at which the v_2 speed is about zero. Accordingly, for the sake of greater precision of measurement I very early discarded the balanced drop method which Ehrenhaft and Joffé and Meyer and Gerlach have so largely used. This accounts quite largely for the greater consistency of my data.

[2] In other words, Professor Ehrenhaft promises to do with his particles precisely what he had found in his review of my 1909 work, which he criticizes in this article, that I had done with my water drops. I had even in that work observed the change in charge, though I did not discuss it in the 1909 paper. De Broglie had observed it in 1908 (C. R., 146, pp. 624

This quotation is introduced merely to explain how it could happen that Professor Ehrenhaft placed such an interpretation as he did upon his observations of 1910. These showed very large irregularities (Schwankungen) in the values of the charges carried by different particles. He focused his attention upon these Schwankungen and concluded, first, that different ions had different charges and, second, that some of these charges were very much lower than 4.5×10^{-10}, the supposed value of the electron. Had Professor Ehrenhaft known of the exact multiple relationships brought to light by the above experiments he could scarcely have put such an interpretation as he did upon his own results, *for it is in this relation that the unimpeachable evidence that electrical charges are, in fact, all built up out of one and the same sort of unit charges, is found.* In the last six years of experimenting, though scores of persons have repeated my experiments with various modifications, no one, save Konstantinowsky (see below), not even Professor Ehrenhaft himself, has ever observed any departure from the exact multiple relationship which I first pointed out. And, until such a departure is found, it seems to me that there can be no scrap of evidence for the existence of electrical charges smaller than that of the electron, for some of the changes in any series of charges carried by a particle are always due to the capture of ions, and the multiple relationship means that all the other charges appearing in the experiment are multiples of this ionic charge.

4. The Essential Elements in the Droplet Method of Determining the Absolute Value of *e*.

It would, however, be very unfortunate if the charges on different drops could not be reduced to a common measure, *i. e.*, if an absolute determination of e could not be made. But my experiments[1] showed that this reduction can, in fact, be made with extraordinary consistency if we adopt the only procedure which it is legitimate to adopt in comparing the velocity of the charged body in a given electric field with its speed under a known force such as mg. For this comparison cannot be made unless we know both the mass m of the particle and the size of the surface S which it opposes to the resistance of the medium, and only then provided we know also the relation between the force acting, the

and 1010, 1908). In spite of the situation revealed in this note, Professor Ehrenhaft in 1914, in reviewing the history of this field of study writes as follows (Wien. Ber., CXXXIII., Feb., 1914, p. 73; also Ann. der Phys., 44, p. 670, 1914): "Die Umladung des Partikels erfolgt am besten durch Ionisierung der Luft im Kondensator. Solche Umladungen habe ich schon im April 1910 konstatiert und im Anhange an meine Abhandlung mitgeteilt. (This is the footnote given above.) Etwa ein halbes Jahr später hat Millikan ebenfalls auf diese Erscheinung bei seinen grössern Ölkugeln hingewiesen."

[1] Phys. Rev., 2, p. 109, 1913; 32, p. 393, 1911.

speed produced by it and the surface exposed to the drag of the medium. This relation we know exactly, both from the theory of Stokes and from the experiments of Arnold, Phil. Mag., 22, 755, 1911, *when and only when the inhomogeneities of the medium are negligibly small compared with the size of the particle*. There is but one possible way, then, to determine e, namely *to find the limit toward which the apparent value of e, on the assumption of Stokes's law, approaches as the size of the drop or the pressure of the gas is increased so as to approach the conditions assumed in Stokes's theory and realized in Arnold's experiments*. When this is done, absolutely all irregularities in the measurements disappear, and e comes out with extraordinary constancy as my results already published show,[1] and as results recently taken and given below show still more clearly even when the range of drop-radii is extended so as to overlap that in which Ehrenhaft works.

What he found was that when he computed e by the aid of the assumption of the validity of Stokes's law for the very minute drops with which he worked he got large irregularities, and these he interpreted as variations in the ionic charge. Mr. Fletcher and I pointed out that such irregularities were to be expected because of Brownian movements[1] when a very short distance of rise and fall was used and when the times of rise and fall were observed but once, as had been the case in Ehrenhaft's experiments. We further showed experimentally that the Brownian movements did account under conditions like those used by him, for just such irregularities as he observed. Furthermore, we suggested that the fact that his mean values fluctuated about a number which was too small for e when and only when he was working with very heavy metals, platinum, gold, and mercury, was probably due to the fact that his actual particles were of irregular shape, or else oxides or other compounds of much less mean density than that which he assumed. In any case since he neither got then, nor yet gets, any consistency in the value of e obtained with different drops *and yet in every case changes his charge by the capture of precisely the same ions* which my experiments show always carry exactly the same charge, it is obviously absurd to assume that these ionic charges take on one value when they are caught by one kind of a drop and another value when caught by another kind of a drop. *His difficulty had of necessity to be looked for in a wrong assumption as to the shape, or density, or size, or law of motion of his particles.*

5. The New Evidence.

Although Professor Ehrenhaft did not admit the validity of our evidence it was generally considered as settling the matter,[2] even Dr. Karl

[1] Phys. Zeit., 12, p. 161, 1911. See also Phys. Rev., 32, p. 393, 1911.

[2] See Pohl's "Bericht über e," Jahrbuck der Radioactivitat und Elektronik, VIII., p. 431.

Przibram who had collaborated with Professor Ehrenhaft and at first adopted his point of view writing me that although his work in this field had "commenced with such a grievous mistake it was now (1912) in good agreement" with mine.

Very recently, however, Professor Ehrenhaft[1] and two of his pupils, F. Zerner[2] and D. Konstantinowsky[3] have published new evidence for the existence of subelectrons and it therefore becomes necessary to consider the nature of this new evidence. The new results are precisely like the old save in the two following particulars.

1. Great precaution is now taken to remove oxygen from the gas so as to prevent the possibility of the formation of oxides. Because of these precautions and because of the fact that certain droplets of mercury of ten or more times larger diameter than those used in his experiments are shown by microphotography to be spherical and to have metallic luster Professor Ehrenhaft assumes that *all of his ultra-microscopic* particles must also be spherical and uncontaminated.

Now in my judgment this evidence is wholly inconclusive and Professor Ehrenhaft has not yet touched the criticism that his irregularities must be looked for in a wrong assumption as to the shape, or the density, or the size, or the law of fall of his particles. Indeed I shall presently show that the evidence furnished by Professor Ehrenhaft's own data is very strongly in favor of such an explanation. Because some big mercury droplets, which he photographs, are spherical, is no indication that all particles are spherical, particularly particles a tenth or a hundredth as large as those which it is possible to photograph. Because he carefully frees his gas from oxygen, is no reason at all for thinking that when he strikes an electric arc between metals all kinds of combinations may not occur between the metal and the gases occluded in the electrodes. For that matter, in electric arcs I know from my own experiments that an inert gas like nitrogen becomes extraordinarily active in forming higher nitrogen compounds.[4]

The fact that these low values are found only with substances like mercury whose density would be greatly diminished by the addition of an oxide, or any other foreign substance, is a very suspicious circumstance, for if the density of the drop is smaller than the assumed density the apparent value of e will be too low. It is too low values of e which Professor Ehrenhaft always obtains whenever he obtains any irregularities at all.

[1] Wien Sitz. Ber., CXXIII., pp. 53–155, 1914. Ann. Phys., 44, p. 657, 1914.

[2] Phys. Zeit., 16, p. 10, 1915.

[3] Ann der Phy., 46, p. 261, 1915.

[4] Since this was written a paper has appeared by Strutt showing that active nitrogen thus formed attacks mercury. See Proc. Roy. Soc. 92, 438, '16; also 85, 219, '11.

2. The second new evidence presented is as follows: In 1911 Mr. Harvey Fletcher and the author first combined, in the Ryerson Laboratory, the observations of the up-and-down motion through a gas, under electrical and gravitational forces, of very minute droplets, with observations on the Brownian movements in the same gas of the same drops[1] when gravity was balanced by an electrical field. We were thus able to eliminate entirely the density of the drop and the resistance factor of the medium, and, provided only Einstein's Brownian movement equation

$$\overline{\Delta x^2} = \frac{2RT}{NK}\tau \tag{1}$$

is correct, to get with certainty the product Ne in the form

$$Ne = \frac{4RT(v_1 + v_2)_0}{\pi KF(\overline{\Delta x})^2}\tau. \tag{2}$$

This equation represents merely the elimination of the resistance factor K from the characteristic equation of the oil drop method, namely

$$\frac{v_1}{v_2} = \frac{mg}{Fe_n - mg} \quad \text{or} \quad e = \frac{mg}{Fv_1}(v_1 + v_2)_0 = \frac{K}{F}(v_1 + v_2)_0 \tag{3}$$

by means of (1) which may also be written,[2] in view of the Maxwell distribution law,

$$(\overline{\Delta x})^2 = \frac{4}{\pi}\frac{RT}{NK}\tau. \tag{4}$$

Now we found Ne to come out by this method the same as in electrolysis.[3] But Professor Ehrenhaft has in 1914 made what he regards as the same test on mercury particles and found Ne to come out in the case of 6 or 7 of the 9 particles on which he has published data[4] somewhat smaller than in electrolysis, though in 2 or 3 of the 9 drops it does not come out smaller. *These two points constitute the sum and substance of Ehrenhaft's addition since 1910 to the question in hand.* It is interesting to observe however, that Professor Ehrenhaft is at last in position to announce the important conclusion that electricity is atomic in structure because he too now finds a multiple relation between the charges carried by a single particle.

[1] Professor Ehrenhaft credits this advance to Weiss and ignores entirely the preceding work which appeared from this laboratory.

[2] Phys. Rev., I, 218, 1913.

[3] A brief summary of this work was given by the author in Science, Feb. 17, 1911, a more complete abstract by Mr. Fletcher in Phys. Zeit., 12, March, 1911, pp. 202–208, and in Le Radium, 8, pp. 279, July 1, 1911. See also Harvey Fletcher, Phys. Rev., 33, pp. 81, 1911, and R. A. Millikan, Phys. Rev., 1, pp. 218, 1913. On July 6, 1911, Dr. Edmund Weiss presented similar observations on silver particles before the Vienna academy, Vol. CXX., abt. II*a*, pp. 1020, and also found Ne to come the same as in electrolysis. He does not treat his data precisely in this way but assumes N from other determinations and then solves for e.

[4] See Wien Ber., CXXIII., pp. 52, 1914.

Now I wish first to point out certain general conditions which must be satisfied if reliable results are to be obtained, either by the Brownian movement method or by the law of fall method of determining e and to show why in my judgment Professor Ehrenhaft and his pupils obtain such results as they do, and second, I wish to consider what bearing their results, whatever their reliability, can have on the question of the existence of a subelectron.

6. The Brownian Movement Method and the Law of Fall Method of Determining e.

The Brownian movement method of determining e actually consists simply in determining Ne as described above and then inserting an *assumed* value of N and solving for e. Although it is possible to make the test of Ne in just the way described and although it was so made in the case of one or two of our drops, Mr. Fletcher worked out a more convenient method which involves expressing the displacements Δx in terms of fluctuations in the time required by the particle to fall a given distance, and thus dispenses with the necessity of balancing the drop at all. I shall present another derivation of this relation which is very simple and yet of unquestionable validity.

In equation (2) let τ be the time required by the particle, if there were no Brownian movements, to fall between a series of equally spaced cross-hairs whose distance apart is d. In view of such movements the particle will have moved up or down a distance Δx in the time τ. Let us suppose this distance to be up. Then the actual time of fall will be $\tau + \Delta t$ in which Δt is now the time it takes the particle to fall the distance Δx. If now Δt is small in comparison with τ, that is, if Δx is small in comparison with d (say 1/10 or less) then we shall introduce a negligible error (of the order 1/100 at the most) if we assume that $\Delta x = v_1 \Delta t$, in which v_1 is the mean velocity under gravity. Replacing then in (2) $(\overline{\Delta x})^2$ by $v_1^2(\overline{\Delta t})^2$ in which $(\overline{\Delta t})^2$ is the square of the average difference, without regard to sign, between an observed time of fall and the mean time of fall t_g, that is, the square of the average fluctuation in the time of fall through the distance d we obtain, after replacing the ideal time τ by the mean time t_g[1]

$$Ne = \frac{4}{\pi} \frac{RT(v_1 + v_2)_0 t_g}{F v_1^2 (\overline{\Delta t})^2} \,. \tag{5}$$

In any actual work $(\overline{\Delta t})^2$ will be kept considerably less than 1/10 the mean time t_g if the irregularities due to the observer's errors are not to

[1] No error is introduced here if, as assumed, $\overline{\Delta t}$ is small in comparison with t_g. However, for more rigorous equations see Fletcher, Phys. Rev., 4, pp. 442, 1914; also Smoluchowski, Phys. Zeit., 16, p. 321, 1915.

mask the irregularities due to the Brownian movements so that (5) is sufficient for practically all working conditions. The work of Mr. Fletcher and of the author was done by both of the methods represented in equations 2 and 5. The difference is that in working with equation (2) one actually balances the drop, and then makes a long series of Δx measurements as in Brownian movement work in liquids. He then destroys the balance and, by producing changes in the number of electrons on the drop, takes a series of measurements of $(v_1 + v_2)$ using a large cross-hair distance and a constant field. The greatest common divisor of this series is the $(v_1 + v_2)_0$.

In working with equation (5) one does not balance the drop at all, but works out *Ne* from $(v_1 + v_2)_0$, taken just as before, and the square in the fluctuation $(\overline{\Delta t})^2$ in the times of fall between two cross-hairs between which the drops fall on the average in the time t_g. The first method involves simply the errors which are incidental to all Brownian movement work. It should yield reliable results if Δx is sufficiently large to be measured with some degree of certainty, and if enough displacements Δx have been taken to render the mean amenable to the law of averages. Hundreds or even thousands have usually been taken in all careful Brownian movement work. The method of equation (5), however, introduces the following two new errors which may be very large.

1. An observer's personal error in attempting to time a series of events happening at exactly equal time intervals will in general be one tenth or even two tenths of a second, depending somewhat on the observer. Now unless the fluctuations Δt due to this cause are *wholly negligible* in comparison with the mean fluctuation due to the Brownian movements in the time t_g, the observed $(\overline{\Delta t})$ in equation (5) will be too large and hence *Ne* too will come out too small.

2. If the drop is evaporating, or drifting out of the focal plane, or changing its speed regularly in any way while fluctuations due to Brownian movements are being taken, the changes in t_g due to this cause will be added to the Brownian movement fluctuations and will make the observed $(\overline{\Delta t})$ appear too large. It may even be several times larger than the value due to Brownian movements.

Finally it is altogether conceivable that if a body were of some shape other than spherical, for example spindle-shaped, it might always fall under gravity or rise in an electric field in such a way as to oppose a smaller surface to the resistance of the medium, than when it is knocking about irregularly under the influence of molecular bombardment. This cause too would make *Ne* come out too small. I have never found any evidence for such an effect as this, but the previously mentioned sources of error are very real and very serious. In the case then of equation (5) all sources of error tend to make Ne come out too small.

When on the other hand we obtain e from the law of fall we use these two equations[1]

$$e_1 = \frac{4}{3}\pi\left(\frac{9\eta}{2}\right)^{\frac{3}{2}}\left(\frac{1}{g(\sigma - p)}\right)^{\frac{1}{2}}\frac{(v_1 + v_2)v_1^{\frac{1}{2}}}{F}, \qquad (6)$$

$$e\left(1 + A\frac{l}{a}\right) = e_1. \qquad (7)$$

The correction to Stokes's law represented by the last equation (7) makes the value of e_1 necessarily larger than e but, since this is always allowed for in all computations which are here under discussion, the only question which concerns us is as to how sources of error are likely to affect e_1 (6). The only uncertain element in this equation is the density of the particle, and when one is working with a dense metal like mercury or gold any surface contamination, or impurity of any sort, will tend to make the particle less dense than the assumed value and thus make e_1 appear too small. With a dust particle of loose structure e_1 might well come out a dozen times too small.

While then the Brownian movement method is independent of density, and probably not much dependent upon shape, the law of fall method is markedly dependent upon both of them, so that if both methods are rightly carried out a wrong assumption as to density or shape would make e by the law of fall method appear to come lower than the value found by the Brownian movement method. Now as a matter of fact all of the nine particles studied by Mr. Fletcher and myself in 1910 and 1911 and computed by Mr. Fletcher[2] showed the correct value of Ne while only six of them as computed by me fell on, or close to, the $e_1^{2/3}$, l/a, {or $e_1^{2/3}$, $1/pa$} line which pictures the law of fall of an oil drop through air.[3] (See also Fig. 1.) This last fact was not published in 1911 because it took me until 1913 to determine with certainty the complete law of fall of a droplet through air, in other words, to extend curves of the sort given in Fig. 1 to as large values of l/a as correspond to particles small enough to show large Brownian movements. As soon as I had done this I computed all of the nine drops which had given correct values of Ne and found that two of them fell far below the $e_1^{2/3}$, l/a, line, one more fell somewhat below, while one fell considerably above it. This meant obviously that these four particles were not spheres of oil alone, two of them falling much too slowly to be so constituted and one considerably too rapidly. There was nothing at all surprising about this result since I had explained

[1] PHYS. REV., XXXII, pp. 354, 378, 1911.
[2] Le Radium, 8, pp. 279, 1911; PHYS. REV., 33, pp. 107, 1911.
[3] PHYS. REV., 2, pp. 136, 1913.

fully in my first paper on oil drops[1] that until I had taken great precaution to obtain dust free air "the values of e_1 came out sometimes differently even for drops showing the same velocity under gravity." In the Brownian movement work no such precautions to obtain dust-free air had been taken because we wished to test the *general* validity of equations (2) and (5). That we actually used in this test two particles which had a mean density very much smaller than that of oil and one which was considerably too heavy was fortunate, since it indicated that our result was indeed independent of the material used.

It is worthy of remark that in general, even with oil drops, almost all of those behaving abnormally fall too slowly, that is they fall below the line of Fig. 1 and only rarely does one fall above it. This is because the dust particles which one is likely to observe, that is, those which remain long in suspension in the air, are either, in general, lighter than oil or else expose more surface than does an oil drop and hence act as though they were lighter. When one works with particles made of dense metals this behavior will be still more marked since all surface impurities of whatever sort will diminish the density. The possibility, however, of freeing oil-drop experiments from all such sources of error is shown by the fact that during the past year, although I have studied altogether as many as two or three hundred drops I do not recall that there has been a single one which did not fall within less than one per cent. of the line of Fig. 1.

7. Ehrenhaft's and Zerner's Analysis of Our Oil Drop Data.

Now, to return to the contention of Ehrenhaft and his pupils, they find, as stated above, first that in some instances Ne by the Brownian movement method comes out too small, and second that in general e comes out smaller by the law of fall method than by the Ne method and increasingly smaller the smaller the velocity of fall and hence the smaller the apparent size. So they contend that they have found subelectrons, but that these escaped my notice because I worked with so large droplets. *These extraordinarily minute charges of electricity can appear, so they assert, only on extraordinarily minute particles.*

Ehrenhaft and Zerner even analyze our reports on oil droplets and find that these also show in certain instances indications of subelectrons, for they yield, in these observers' hands, too low values of e whether computed from the Brownian movements or from the law of fall, and when the computations are made in the latter way e is found, according to them, to decrease with decreasing radius, as is the case in their experiments on mercury and gold particles.

[1] Phys. Rev., 33, pp. 366 and 367, 1911.

Now the single low value of *Ne* which they find in our work, is obtained by computing *Ne* from some twenty-five observations on the times of fall, and an equal number on the times of rise, of a particle which, before we had made any *Ne* computations at all, we reported upon[1] for the sake of showing that the Brownian movements would produce just such fluctuations as Ehrenhaft had observed when the conditions were those under which he worked.

Let us consider first this oil-drop evidence. Although it is obvious that in general very little significance can attach to attempts to test a statistical theorem on so few observations as we recorded in the case of this drop, yet it so happens that according to Mr. Fletcher's computation[2] of the data which he and I published in the Phys. Zeit., 12, pp. 162, 1911, on this drop Ne does come out from it within 2 per cent. of the correct value, namely 9,650 instead of 30 per cent. less as Ehrenhaft and Zerner find it to do. When, however, I compute *Ne* by equation (5) using merely the 25 times of fall, I find that the value of *Ne* comes out 26 per cent. low, which is about as Zerner finds it to do. If, however, I omit the first reading it comes out but 11 per cent. low. In other words the omission of one single reading changes the result by 15 per cent. and different groupings of the same observations make the 30 per cent. difference between Fletcher's and Zerner's results. This brings out clearly the futility of attempting to test a statistical theorem by so few observations as 25, *which is nevertheless more than Ehrenhaft usually uses on his drops.* Furthermore I have just shown that unless one observes under carefully chosen conditions his own errors of observation and the slow evaporation of the drop tend to make *Ne* obtained from equation (5) come out too low, and these errors may easily be enough to entirely vitiate the result. *There is then not the slightest indication in any work which we have thus far done on oil drops that Ne comes out too small.*

Next consider the apparent variation in e when it is computed from the law of fall. Zerner computes e from my law of fall in the case of the nine drops published by Fletcher, in which *Ne* came out the same as in electrolysis and finds, that one of them yields $e = 6.66 \times 10^{-10}$, one $e = 3.97 \times 10^{-10}$, one $e = 1.32 \times 10^{-10}$, one $e = 1.7 \times 10^{-10}$, while the other five yield about the right value, namely, 4.8×10^{-10}. In other words, as I had found before was the case (see above), five of these drops fall exactly on my curve (Fig. 1), one falls somewhat above it, one somewhat below, while two are entirely off and very much too low. These two, therefore, I concluded, were not oil at all but dust particles.

[1] Phys. Zeit., Vol. 12, pp. 162, 1911.

[2] Harvey Fletcher, Phys. Zeit., Sept., 1915.

Since Zerner computes the radius from the rate of fall, these two dust particles, which fall much too slowly, and therefore yield too low values of e must, of course, yield correspondingly low values of a. Since they are found to do so *Zerner concludes that our oil drops as well as Ehrenhaft's mercury particles yield decreasing values of e with decreasing radius.* His own tabulation does not show this. It merely shows three erratic values of e, two of which are very low and one rather high. But a glance at all the other data which I have published on oil drops shows the complete falsity of this position,[1] for this data shows that *after I had eliminated dust all of my particles yielded exactly the same value of e whatever their size.*[2] The only possible interpretation then which could be put on these two particles which yielded correct values of Ne but too slow rates of fall was that which I put upon them, namely, that they were not spheres of oil.

8. The Vienna Data on Mercury and Gold.

As to the Vienna data on mercury and gold, Ehrenhaft publishes, all told, data on just 16 particles and takes, for his Brownian movement calculations, on the average, only *15 times of fall and 15 times of rise on each, the smallest number being 6 and the largest 27.* He then computes by equation (5) his statistical average $(\overline{\Delta t})^2$, from such absurdly inadequate numbers of observations. Next he assumes Perrin's value of N, namely 70×10^{22}, which corresponds to $e = 4.1$, and obtains instead by the Brownian movement method, *i. e.*, the Ne method, the following values of e, the exponential term being for brevity omitted: 1.43, 2.13, 1.38, 3.04, 3.5, 6.92, 4.42, 3.28, .84. Barring the first three and the last of these the mean value of e is just about what it should be, as a matter of fact a little too high instead of too low, namely, 4.22 instead of 4.1. Further, the first three particles are the heaviest ones, the first one falling between his cross-hairs in 3.6 seconds and its fluctuations in time of fall are from 3.2 to 3.85 seconds, that is, only three tenths of a second on either side of the mean value. *Now these fluctuations are only slightly greater than those which the average observer will make in timing the passage of a uniformly moving body across equally spaced cross-hairs.* This means that in these observations, two nearly equally potent causes were operating to produce fluctuations. The observed Δts were, of course, then, larger than those due to Brownian movements alone, and might easily, with the few observations which were taken, be two or three times as large which would make Ne come out from four to nine

[1] Phys. Rev., II., pp. 138, 1913.
[2] See Phys. Rev., 2, pp. 134 and 135, 1913.

times too small. *It is only when the observer's mean error is wholly negligible in comparison with the Brownian movement fluctuations that this method will not yield too low values of e.* The overlooking of this fact is, in my judgment, the cause of some of the low values of *e* recorded by Ehrenhaft.

Again in the original work on mercury droplets, which I produced both by atomizing liquid mercury and by condensing the vapor from boiling mercury,[1] it was noticed that such droplets evaporated for a time even more rapidly than oil, and other observers who have since worked with mercury have reported the same behavior.[2] The amount of this effect may be judged from the fact that one particular droplet of mercury recently under observation in this laboratory had at first a speed under gravity of 1 cm. in 20 seconds, which changed in half an hour to 1 cm. in 56 seconds. The slow cessation, however, of this evaporation indicates that the drop slowly becomes coated with some sort of protecting film. Now if any evaporation whatever is going on while successive times of fall are being observed, the apparent $(\overline{\Delta t})^2$ may easily be several times as large as that due to Brownian movements even though these movements are large enough to prevent the observer from noticing, in taking twenty or thirty readings, that the drop is continually changing. One is disposed then from the established behavior of mercury drops to draw one or the other of two conclusions regarding Ehrenhaft's drops (1) either that they were not pure mercury or else (2) that his $(\overline{\Delta t})^2$ measurements were too large because of evaporation, and it is altogether conceivable that in the latter case they might be ten times too large. *There is, then, so far as I can see, no evidence at all in any of the data published to date by Ehrenhaft that the Brownian movement method actually does yield too low values of e.*

Konstantinowsky's data is very much like Ehrenhaft's in the possibility which it permits of too low values of *Ne* due to observational error and evaporation, but it emphasizes one further source of error which apparently leads the author entirely astray. He publishes *Ne* observations on just 11 particles,[3] five of which yield values of *e* between 3.3 and 4.2×10^{-10} or roughly correct values when the fact is considered that his chosen value of N is 70×10^{22}, three of the others yield about 2×10^{-10}, two more about 1×10^{-10}, while one yields $.5 \times 10^{-10}$. *His determination of the series of multiple relationships by which he gets the greatest common divisor* $(v_1 + v_2)_0$ *(see equation 6), is however so unreliable that he raises a question as to whether there is any greatest common divisor at all in spite of the fact that all other observers, a dozen of us now*

[1] PHYS. REV., 32, pp. 389, 1911.

[2] See Schidlof et Karpowicz, C. R., 158, pp. 1992.

[3] Ann. d. Phys., 46, pp. 292, 1915.

at least, including Ehrenhaft himself, now find these exact multiple relations invariably to hold. But an uncertainty in $(v_1 + v_2)_0$ (see equation 6) means an equal uncertainty in Ne. Konstantinowsky's very low values of Ne are then, in my judgment, due to the fact that he chooses the wrong value of $(v_1 + v_2)_0$. But with apparatus of his dimensions and particles as minute as he uses, it is not at all surprising that he cannot find the greatest common divisor of the series of speeds. *It would take more observations than he usually makes on a particle to locate it with certainty where the Brownian movements are as large as those which his particles ought to show, and where the field strengths are as small as those which he uses (nine volts only in some cases on condenser plates 2 mm. apart)* and hence where the drops are relatively heavily charged.

Let us next see why it is that Ehrenhaft's data show that, as we take smaller and smaller drops, the values of both a and e computed from the law of fall become farther and farther removed from the value of a and e computed from Brownian movements. To do this it is only necessary to consider how the density of the drop enters into the two methods.

The Brownian movement method, as used by him, consists first in solving equation (4) for K after inserting Perrin's value of N. (My K is his $1/B$ and to adapt (4) to his type of observation $(\overline{\Delta x})^2$ must be replaced by $v_1^2(\overline{\Delta t})^2$ and τ by t_g.) Knowing K correctly we can obtain m and e correctly from the equations which I experimentally verified in my first oil drop work, namely,

$$mg = Kv_1 \tag{8}$$

and

$$e = \frac{K}{F}(v_1 + v_2)_0. \tag{9}$$

So far no assumption has been needed as to the density and sphericity of the drop. If we wish to obtain a, however, we must get it from the assumption of a spherical drop, namely, from

$$m = \tfrac{4}{3}\pi a^3(\sigma - \rho)$$

which gives with (8)

$$a = \sqrt[3]{\frac{3}{4}\frac{Kv_1}{\pi(\sigma - \rho)g}}. \tag{10}$$

These equations show that any wrong assumption as to the density σ of the drop will not affect e, (9) but it will affect a, (10) a 9 per cent. error in $(\sigma - \rho)$ for example, appearing, in view of the cube root sign, only as a 3 per cent. error in a.

When now we compute e and a from the law of fall we see from equation (6) since e is proportional to $1/(\sigma - \rho)^{1/2}$ that a 9 per cent. error in $(\sigma - \rho)$ appears, in view here of the square root sign, as a $4\frac{1}{2}$ per cent. error in e. Again, from $mg = 6\pi\eta av$ or

$$\frac{4}{3}\pi a^3(\sigma - \rho)g = 6\pi\eta a v_1 \tag{11}$$

we see that

$$a = \sqrt{\frac{9}{2}\frac{\eta v_1}{(\sigma - \rho)g}}, \tag{12}$$

so that a 9 per cent. error in $\sigma - \rho$ involves also a $4\frac{1}{2}$ per cent. error in a. Suppose now there is a film of some foreign material which collects upon droplets of mercury thus making them a trifle lighter than pure mercury. This will not affect the e obtained from Brownian movements but it will make the e obtained from the resistance formula too small, since we have inserted too large a density in the denominator of equation (6). Again, this wrong assumption as to σ will affect a from (12) more than a from (10) the latter therefore appearing the larger. Further, the smaller the drop becomes the more will the two computations of e differ, the e from the law of fall becoming smaller and smaller with respect to the e from the Brownian movements. Similarly the a computed by the two methods will grow apart, the value obtained from the law of fall being influenced more by the wrong assumption as to density than the other. *Now this is precisely the behavior shown by all of Ehrenhaft's and Konstantinowsky's series of measurements on particles of mercury and gold.* All of these results follow in just the same way if the trouble is not with the density, but with the shape of the particles; for then the v_1 of equations (10) and (12) is too small, and hence the particle seems to be smaller than it is, but smaller from (12) than from (10).

If we then consider the Einstein Brownian movement equation established, there is only one possible conclusion to draw from Ehrenhaft's and Konstantinowsky's data, namely, that the few particles on which these authors publish observations have surface impurities, or non-spherical shapes, or else are not mercury at all. I have already commented on the illuminating fact that this data is all taken when they are working with the exceedingly dense substances, mercury and gold, and when, therefore, any thing not mercury but assumed to be, would yield very much too low values of e and a. The further fact that Ehrenhaft implies that normal values of e very frequently appear in their work[1] and that the low, erratic drops represent only a part of the data

[1] Wien Ber., CXXIII., pp. 59. "Die bei grösseren Partikeln unter gewissen Umständen bei gleicher Art der erzeugung haufig wiederkehrenden höheren Quanten waren dann etwa als stabilere raumliche Gleichgewichtsverteilungen dieser Sub-elecktron anzusehen, die sich unter gewissen Umstanden ergeben."

taken, is very suggestive. *When one considers too that in place of the beautiful consistency and duplicability shown in our oil drop work Ehrenhaft and his pupils never publish data on any two particles which yield the same value of e, but instead find only irregularities and erratic behavior*[1] just as they would expect to do with non-uniform particles, or with particles having dust specks attached to them, one wonders why any other explanation than the foreign material one, which explains all the difficulties, has ever been thought of. As a matter of fact in our work with mercury droplets at the Ryerson laboratory we have found that the initial rapid evaporation gradually ceases just as though the droplets had become coated with some foreign film which prevents further loss. Schidlof and Karpawicz find the same behavior of mercury drops and they further find that *this behavior as regards evaporation is the same in the purest nitrogen as it is in air.*[2] Ehrenhaft himself, in speaking of Brownian movements of his metal particles, comments on the fact that they seem at first to show large movements which grow smaller with time.[3] This is just what should happen if the radius is increased by the growth of a foreign film. In addition to all this we have very definite proof which will be presented later that mercury drops in the presence of oil vapor such as might come from stopcock grease do become coated with a film of oil.

Now what does Ehrenhaft say to these very obvious suggestions as to the cause of his troubles. Merely that he has avoided all oxygen and hence an oxide film is impossible and that he has photographed some big droplets and found them spherical. *Yet he makes his metal particles by striking an electric arc between metal electrodes, and the particles in question are not those which he photographs, for these are far below the limit of resolving power of any optical instrument.* In a word then Ehrenhaft's tests as to sphericity and purity are all quite worthless as applied to the particles in question, which according to him, have radii of the order 10^{-6} cm., a figure a hundred times smaller than the limit of sharp resolution.

[1] Their whole case is summarized in the tables in Ann. der Phys., 44, p. 693, 1914, and 46, p. 292, 1915, and it is recommended that all interested in this discussion take the time to glance at the data on these pages, for the data itself is so erratic as to render discussion of it needless.

[2] C. R., July, 1914.

[3] Phys. Zeit., 12, pp. 98. "Wie ich in meinen früheren Publikationen erwahnt habe zeigen die ultra mikroskopischen metall Partikel, unmittelbar nach der Erzeugung beobachtet eine viel lebhaftere Brownsche Bewegung als nach einer halben Stunde."

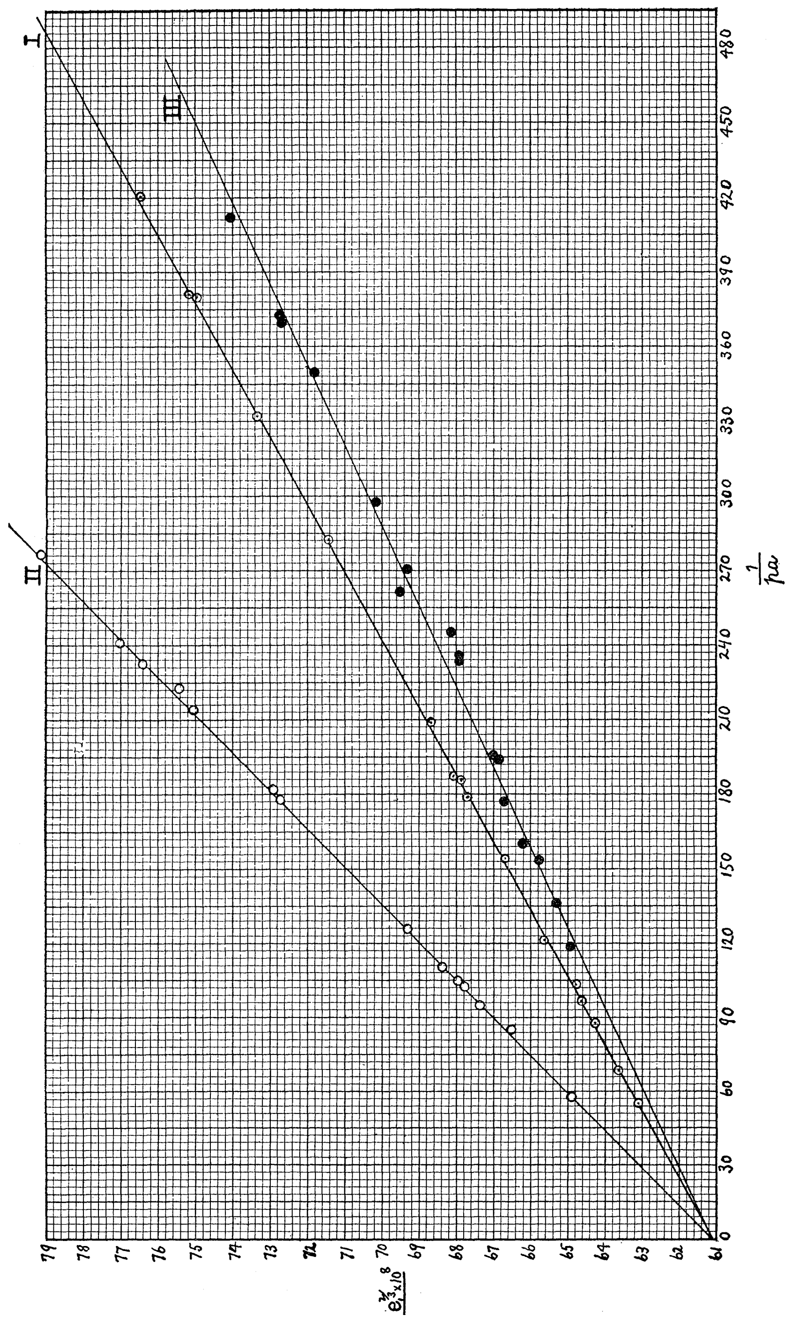
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Fig. 1.

9. The Bearing of the Vienna Work on the Question of the Existence of a Subelectron.

But let us suppose that Ehrenhaft and Konstantinowsky do actually work with spherical particles of pure mercury and gold as they think they do, and that the observational and evaporational errors do not account for the low values of *Ne*, then what conclusion could be legitimately drawn from their data? Merely this and nothing more: (1) that Einstein's Brownian movement equation is not universally applicable, and (2) that the law of motion of their very minute charged particles through air is not yet fully known. So long as they find exact multiple relationships, as Ehrenhaft now does, between the charges carried by a given particle when its charge is changed by the capture of ions or by the direct loss of electrons, the charges on these ions must be the same as the ionic charges which I have accurately and consistently measured and found equal to 4.77×10^{-10} E.S. units. For they, in their experiments, capture exactly the same sort of ions, produced in exactly the same way as those which I capture and measure in my experiments. That these same ions have one sort of a charge when captured by a big drop and another sort when captured by a little drop is obviously absurd. If they are not the same ions which are caught in the two cases, then, *in order to reconcile the results with the existence of the exact multiple relationship found by Ehrenhaft as well as ourselves, it would be necessary to assume that there exist in the air an infinite number of different kinds of ionic charges corresponding to the infinite number of possible radii of drops, and that, when a powerful electric field drives all of these ions toward a given drop, this drop selects in each instance just the charge which corresponds to its particular radius.* Such an assumption is not only too grotesque for serious consideration but it is directly contradicted by my experiments, for I have repeatedly pointed out that with a given value of l/a I obtain exactly the same value of e_1 whether I work with big drops or with little ones.

10. New Proof of the Constancy of *e*.

For the sake of subjecting the constancy of *e* to the most searching test, I have recently made new measurements of the same kind as those heretofore reported, but using now a range of sizes which overlaps that in which Ehrenhaft works. I have also varied through wide limits the nature and density of both the gas and the drops. Fig. 1 (I.) contains new oil drop data taken in air; Fig. 1 (II.) similar data taken in hydrogen; Fig. 1 (III.) similar data on droplets of mercury in air. The radii of these drops, computed by the very exact method given in the Physical Review (2), p. 117, 1913, vary tenfold, namely, from .000025 cm. to .00023 cm. Ehrenhaft's range is from .000008 cm. to

.000025 cm. It will be seen that these drops fall in every instance on the lines of Fig. 1 (I.) and (II.), and hence that they all yield exactly the same value of $e^{2/3}$, namely, 61.1×10^{-8}. The details of the measurements in air, which are just like those previously published, will be entirely omitted, but sample data on four of the drops in hydrogen are given in Tables I., II., III. and IV., and in Table V. is found a summary of the results on all the drops. The drop in Table I. will be seen to yield a value of $e^{2/3}$ which is a per cent. too high, because l/a is slightly above the limit at which I found the linear relation between $e_1^{2/3}$ and l/a to begin to break down[1] *but there is not a trace of an indication that the value of e becomes smaller as a decreases.* The coefficient of viscosity of hydrogen, namely $\eta_{23} = .0000884$, is in agreement with that obtained by multiplying the absolute value[2] of η for air at 23° C., namely, .0001824, by the value found by two recent observers Markowsky[3] and Gille[4] for the ratio between the viscosities of hydrogen and air. The

TABLE I.

Oil Drop in Hydrogen Falling 1.03 cm. in 8 Minutes.

t_g.	t_F.	n.	$\frac{1}{n}\left(\frac{1}{t_g}+\frac{1}{t_F}\right)$.	
	10.626	3	.03209	
	32.678			$V_i = 809.5$
	32.848			$V_f = 808.0$
	33.060			$t = 23.0°$ C.
				$p = 74.78$ cm.
460.0	15.830	2	.03262	$v_1 = .03249$
				$\eta_{23} = .00008841$
441.8	32.892			$a = .0000252$ cm.
471.8	33.192			$\frac{1}{pa} = 530.6$
455.4	33.802			
452.8	33.046			$\frac{l}{a} = 0.695$
442.4	32.740	1	.03166	
436.2	33.996			$e_1^{2/3} = 96.92$
445.0	33.104			
434.0	33.194			
448.8			.03170	$e^{2/3} = 61.9 \times 10^{-8}$

points on these two curves represent consecutive series of observations, not a single drop being omitted in the case of either the air or the hydrogen. This shows the complete uniformity and consistency which we

[1] PHYS. REV., 2, 138, 1913.
[2] Ann. der Phys., 1913.
[3] Ann. der Physik, p. 749, 1904.
[4] Ann. der Physik, Vol. 48, p. 825, 1916.

Table II.

Oil Drop in Hydrogen Falling 1.02 *cm.*

t_g.	t_F.	n.	$\frac{1}{n}\left(\frac{1}{t_g}+\frac{1}{t_F}\right)$.	
101.0	30.5			$V_i = 3252.8$
101.9	30.6	1		$V_f = 3244.5$
102.9	30.6			$t = 23.0° C.$
				$p = 75.25$ cm.
103.4	13.6			$v_1 = .04303$
103.0	13.2	2	.04218	$\eta_{23} = .00008841$
				$a = .0000596$
103.1	30.5			$\frac{1}{pa} = 223.1$
103.1	30.8			
101.9	30.8	1	.04219	$\frac{l}{a} = .292$
102.7	30.1			
102.6	30.8			$e_1^{2/3} = 75.40 \times 10^{-8}$
102.6			.04219	$e^{2/3} = 60.9 \times 10^{-8}$

Table III.

Oil Drop in Hydrogen Falling 1.02 *cm.*

t_g.	t_F.	n'.	$\frac{1}{n'}\left(\frac{1}{t_{F'}}-\frac{1}{t_F}\right)$.	n.	$\frac{1}{n}\left(\frac{1}{t_g}+\frac{1}{t_F}\right)$.	
18.638	42.122					
18.720	42.466					
18.616	41.916					
18.505	41.519			6	.01292	
18.656	42.070					
18.579	41.780					$V_i = 2456.2$
18.596	41.922					$V_f = 2452.6$
		1	.01283			$t = 22.88° C.$
18.588	91.712					$p = 39.40$
18.588	93.012					$v_1 = 0.01313$
18.569	93.228			5	.01289	$\eta = 0.00008841$
18.692	95.416					$a = 0.000143$
18.648	94.628					$\frac{1}{pa} = 178.1$
		1	.01275			
18.497	42.828					$\frac{l}{a} = 0.233$
18.459	43.016					
18.376	43.141					$e_1^{2/3} = 72.71 \times 10^{-8}$
18.586	43.190			6	.01282	
18.514	43.200					
18.456	42.926					
		1	.01310	5	.01276	
18.426	98.590					
18.472	99.246					
18.560			.01289		.01287	$e^{2/3} = 61.11 \times 10^{-8}$

TABLE IV.

Oil Drop in Hydrogen Falling 1.02 *cm.*

t_g.	t_F.	n'.	$\frac{1}{n'}\left(\frac{1}{t_F}-\frac{1}{t'_F}\right)$	n.	$\frac{1}{n}\left(\frac{1}{t_g}+\frac{1}{t_F}\right)$.	
7.726	65.146					
7.742	64.102					
7.632	63.240					
7.742	62.624			13	.01112	
7.728	61.782					
7.760	61.308					
7.750	61.394					
		1	.01132			
7.808	36.176					
7.742	35.822			14	.01118	
7.838	35.754					
		1	.01109			V_i = 3928.0
7.778	59.340					V_f = 3920.6
7.750	59.320			13	.01119	t = 23.02° C.
		3	.01114			p = 74.27 cm.
7.734	19.926					v_1 = 0.011434
7.828	19.566					η = 0.00008841
7.716	19.732			16	.01121	a = 0.000233
7.734	19.600					$\frac{1}{pa}$ = 57.7
		3	.01113			
7.774	57.900					$\frac{l}{a}$ = 0.0756
7.810	57.296			13	.01123	
7.856	57.354					$e_1^{2/3} = 64.89 \times 10^{-8}$
		1	.01128			
7.848	34.880			14	.01124	
7.816	34.708					
		5	.01119			
7.846	11.784					
7.738	11.836			19	.01122	
7.732	11.774					
		6	.01119			
7.780	57.120					
7.814	56.632					
7.754	57.340			13	.01124	
7.794	57.186					
7.769			.01119		.01121	$e^{2/3} = 61.10 \times 10^{-8}$

have succeeded in obtaining in the work with oil drops. That mercury drops show a similar behavior was somewhat imperfectly shown in the original observations which I published on mercury.[1] I have since fully confirmed the conclusions there reached. That mercury drops can with suitable precautions be made to behave practically as consistently as oil is shown in Fig. 1, III, which represents data obtained by blowing into

[1] PHYS. REV., pp. 389–390, 1911.

Table V.

No.	$t°$ C.	p Cm. Hg.	PD Volts.	t_g Sec.	n.	$a \times 10^5$ Cm.	$\frac{1}{pa}$	$e_1^{2/3} \times 10^8$.	$e^{2/3} \times 10^8$.
1	23.0	74.78	808.5	44.88	1–3	2.52	530.5	96.92	61.9
2	23.0	19.05	1,644.2	17.05	7–11	13.64	385.0	86.46	61.22
3	23.0	30.99	1,638.8	34.78	3–4	9.74	331.4	83.05	61.00
4	23.0	40.64	2,445.0	53.23	1–2	7.94	309.8	81.65	60.92
5	23.0	30.74	1,639.5	25.03	5–12	11.76	276.6	79.14	61.24
6	23.0	30.46	1,640.3	19.20	7–8	13.65	240.5	77.04	61.06
7	23.0	40.57	2,460.5	32.56	2–3	10.59	232.6	76.39	61.01
8	23.0	75.40	3,248.5	10.26	1–2	5.96	223.1	75.40	60.9
9	23.0	40.41	2,448.8	27.47	3–8	11.55	214.2	75.02	60.90
10	23.0	40.19	2,451.0	20.16	5–8	13.67	182.0	72.88	61.27
11	22.9	39.40	2,454.4	18.61	5–6	14.25	178.1	72.71	61.12
12	23.0	73.59	3,280.0	33.56	2–3	10.78	126.0	69.31	61.26
13	23.0	74.78	3,266.5	27.58	2–5	12.07	110.7	68.36	61.08
14	23.0	74.46	3,915.5	24.55	2–5	12.84	104.6	67.94	61.06
15	22.9	74.40	3,288.0	23.63	3–6	13.12	102.4	67.74	61.00
16	23.0	73.31	3,287.8	19.76	4–7	14.37	94.9	67.36	61.02
17	23.0	75.88	3,278.1	17.54	5–8	15.34	85.8	66.56	60.88
18	23.0	74.27	3,924.3	7.78	12–19	23.34	57.7	64.89	61.11
								Mean.	61.11

the observing chamber above the pinhole in the upper plate a cloud of mercury droplets formed by the condensation of the vapor arising from boiling mercury. This data has just been taken in the Ryerson Laboratory with my apparatus by Mr. and Mrs. John B. Derieux. Since the pressure was here always atmospheric, the drops progress in the order of size from left to right, the largest having a diameter about three times that of the smallest, the radius of which will be seen from Table V. to have been .00003244 cm. It will be seen that a way has here been found to largely eliminate the evaporation of the mercury drops, so marked in most preceding work of this kind on mercury.

It is not claimed that this work constitutes a determination of e which compares in precision with that attained in the oil drop work previously published, but it does show that the line through the point $e^{2/3} = 61.1 \times 10^{-8}$ lies very close to all the points observed and hence that my original determination of e at 4.774×10^{-10} cannot be much in error.

A glance at the value of $e^{2/3}$ in the lower right hand corner of each of these tables is enough to establish with absolute conclusiveness the correctness of the assertation that the apparent value of the electron is not in general a function of the radius of the drop on which it is caught, even when that drop is of mercury, and even when it is as small as some of those with which Ehrenhaft obtained his erratic results. If it appears to be so with his drops, the cause cannot possibly be found in actual fluctuations in the charge

TABLE V.

Mercury Drop No. 1 Falling 1 cm. in Air.[1]

t_g.	t_F.	n'.	$\frac{1}{n'}\left(\frac{1}{t_{F'}}+\frac{1}{t_F}\right)$.	n.	$\frac{1}{n}\left(\frac{1}{t_g}+\frac{1}{t_F}\right)$.	
39.89	8.13					
39.91	8.03					
39.82	7.90			4	.0376	
39.79	7.91					
		1	.0372			
	11.28					
	11.30					
	11.54			3	.0377	
	11.51					
	11.22					
		1	.0380			$V_i = 3935$
	20.16					$V_t = 3875$
	0.12					$t = 23.05$
	19.95			2	.0373	$p = 75.98$
	20.05					$v_1 = .02464$
	19.95					$\eta = .0001824$
		1	.0372			$a = .0000362$
	78.4					
	79.2			1	.0373	$\frac{1}{pa} = 369.4$
	78.0					$\frac{l}{a} = .2666$
		1	.0380			
	19.81					$e_1^{2/3} = 72.63 \times 10^{-8}$
	19.61					
	19.79			2	.0374	
	19.75					
	19.69					
41.41		1	.0382			
41.21	11.19					
41.38	11.30					
41.20	11.31			3	.0377	
41.21	11.25					
41.46	11.26					
40.58			.0377		.0375	$e^{2/3} = 61.3 \times 10^{-8}$

of the electron without denying completely the validity of my results. But these results have now been checked in their essential aspects by scores of observers, including Professor Ehrenhaft himself. Furthermore, my results are not the only ones with which Ehrenhaft's contention clashes; it is at variance also with all experiments like those of Rutherford and Geiger and Regener on the measurement of the charges carried by α and β particles, for these are infinitely smaller than any particles used by Ehrenhaft and if, as he contends, the value of the unit out of which a charge is built up is smaller and smaller the smaller the capacity of the body on which it is found, then these alpha-particle charges ought

TABLE VI.

Mercury Drop No. 3 Falling 1 cm. in Air.

t_g.	t_F.	n'.	$\frac{1}{n'}\left(\frac{1}{t_F} - \frac{1}{t_F}\right)$.	n.	$\frac{1}{n}\left(\frac{1}{t_g} + \frac{1}{t_F}\right)$.	
48.09	50.8					$V_i = 3800$
47.48	50.4					$V_t = 3745$
47.97	50.9			1	.0405	$t = 22.90$
47.89	50.1					$p = 74.84$
47.71		1	.0409			$v_1 = .02082$
48.48	16.50					$\eta_{23} = .0001824$
48.64	16.52			2	.0407	$a = .00003244$
						$\frac{1}{pa} = 411.8$
						$\frac{l}{a} = .2928$
						$e_1^{2/3} = 74.05 \times 10^{-8}$
47.92			.0409		.0406	$e^{2/3} = 61.3 \times 10^{-8}$

to be extraordinarily minute in comparison with the charges upon our oil drops. Instead of this, the charge on the alpha-particle comes out nearly twice the charge which I measure in my oil-drop experiments.

While then it would not be in keeping either with the spirit or with the method of modern science to make any dogmatic assertion about the existence or non-existence of a subelectron it can be asserted with entire confidence that there is not in Ehrenhaft's experiments a scrap of evidence for the existence of charges smaller than the electron. If all of his assumptions as to the nature of his particles are correct then his experiments mean simply that Einstein's Brownian movement equation is not of universal validity and that the law of motion of minute charged particles is quite different from that which he has assumed. It is very unlikely that either of these results can be drawn from his experiments for Nordlund[1] and Westgren[2] have apparently verified the Einstein equation in liquids with very much smaller particles than Ehrenhaft uses and on the other hand, while I have worked with particles as small as 2×10^{-5} cm. and with values of l/a as large as 100, which is very much larger than any which appear in the work of Ehrenhaft and his pupils, I have thus far found no evidence of a law of motion essentially different from that which I published in 1913.

[1] Zeitschr. für Phys. Chem., 87, 40, 1914.

[2] Untersuchungen über Die Brownsche Bewegung u. s. w. Inaugural Dissertation von Arne Westgren, Uppsala & Stockholm, 1915.

Table VII.

Mercury Drop No. 2 Falling 1 cm. in Air.

t_g.	t_F.	n'.	$\frac{1}{n'}\left(\frac{1}{t_{F'}} - \frac{1}{t_F}\right)$.	n.	$\frac{1}{n}\left(\frac{1}{t_g} + \frac{1}{t_E}\right)$.	
9.92	6.00					
9.83	5.90					
9.85	5.80			14	.0194	
	5.94					
		4	.0197			
	11.02					
	10.92					
9.76	10.97					
	11.01					
	10.98			10	.0193	
	11.04					
	10.94					
9.76	11.01					
	11.15					
		1	.0195			
	14.05					
	13.96			9	.0193	
9.66	14.06					
		4	.0194			V_i = 4685 volts
	6.68					V_f = 4615 volts
	6.67			13	.0193	t = 23° C.
9.67	6.79					p = 74.61 cm.
	6.75					v_1 = .1024 cm./sec.
		4	.0193			η = .0001824
	14.07					a = .00007575
	13.93					$\frac{1}{pa}$ = 177.4
9.72	13.92			9	.0193	
	13.98					$\frac{l}{a}$ = .1253
		1	.0199			$e_1^{2/3} = 66.75 \times 10^{-8}$
	19.26					
	19.29			8	.0193	
9.55	19.39					
	19.52					
		2	.0194			
	76.8					
	77.4			6	.0192	
	77.3					
	77.8					
		1	.0188			
	31.54					
	31.36			7	.0191	
9.82	31.47					
	31.84					
		1	.0192			
	19.7			8	.0192	
	19.6					
9.72	19.51					

t_g.	t_F.	n'.	$\frac{1}{n'}\left(\frac{1}{t_{F'}}-\frac{1}{t_F}\right)$.	n.	$\frac{1}{n}\left(\frac{1}{t_g}+\frac{1}{t_F}\right)$.	
9.72	19.67	2	.0192			
	8.0					
	8.0			6	.0192	
	7.92					
9.79	7.90	1	.0187			
9.97	3.17			7	.0191	
9.79	3.23					
9.77			.01932		.01925	$e^{2/3} = 61.1 \times 10^{-8}$

11. Summary.

In conclusion then:

1. Professor Ehrenhaft has published no adequate test of whether Ne by the Einstein Brownian movement method comes out for his particles the same as in electrolysis. I have tested it very roughly for Hg particles of size about $a = 2.5 \times 10^{-5}$ and Fletcher and Eyring have tested it very accurately for oil particles of this size and found it to hold. It will probably hold for Ehrenhaft's particles when given a reliable test.

2. If it should not be found to hold his result will have no bearing whatever upon the question of the existence of a subelectron. It will mean merely that the Einstein equation is not applicable in gases to charged particles of all sizes.

3. All of Professor Ehrenhaft's results are easily explained on the assumption of incorrect assumptions as to the density and sphericity of his particles, but even if these assumptions are correct, yet his results have no bearing on the question of the existence of a subelectron. They mean simply that he has assumed an incorrect law of movement of his minute charged particles through a gas.

4. The non-appearance of a subelectron in Professor Ehrenhaft's experiments is demonstrated by the existence of a multiple relationship between the charges carried by a given particle taken in conjunction with my direct proof extended in this paper that the apparent value of e is not in general even with mercury droplets a function of the radius of the drop on which it is found. The opposite assumption invoked by Professor Ehrenhaft not only involves a grotesque assumption as to the nature of ionization but is flatly contradicted by my experiments.

5. There has appeared up to the present time, I think, no evidence whatever for the existence of a subelectron.

I have to thank Mr. Wm. Gaertner for the loan of the magnificent printing chronograph with which the above time determinations have been made, and Dr. Yoshin Ishida for able assistance in the experimental work as well as in computing the observations.