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An Appraisal of the Controversial Nature of the Oil Drop Experiment: Is Closure Possible?

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ABSTRACT

Acceptance of the quantization of the elementary electrical charge (*e*) was preceded by a bitter dispute between Robert Millikan (1868–1953) and Felix Ehrenhaft (1879–1952), which lasted for many years (1910–25). Both Millikan and Ehrenhaft obtained very similar experimental results and yet Millikan was led to formulate the elementary electrical charge (electron) and Ehrenhaft to fractional charges (subelectron). There have been four major attempts to reconstruct the historical events that led to the controversy: Holton ([1978]); Franklin ([1981]); Barnes et al. ([1996]); Goodstein ([2001]). So we have the controversy not only among the original protagonists but also among those who have interpreted the experiment. The objective of this study is a critical appraisal of the four interpretations and an attempt to provide closure to the controversy. It is plausible to suggest that Ehrenhaft's methodology approximated the traditional scientific method, which did not allow him to discard anomalous data. Millikan, on the other hand, in his publications espoused the scientific method but in private (handwritten notebooks) was fully aware of the dilemma faced and was forced to select data to uphold his presuppositions. A closure to the controversy is possible if we recognize that Millikan's data selection procedure depended primarily on his commitment to his presuppositions (existence of *e*). Franklin's ([1981]) finding that the selection of the drops did not change the value of *e* but only its statistical error carries little weight as Millikan did not perform Franklin-style analyses that could have justified the exclusion of drops. It is plausible to suggest that had Millikan performed such analyses, he would have included them in his publication in order to provide support for his data selection procedures. In the absence of his presuppositions, Millikan could not tell which was the 'expected correct' value of *e* and the degree of statistical error. Finally, if we try to understand Millikan's handling of data with no reference to his presuppositions, then some degree of 'misconduct' can be perceived.

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1 Introduction

Most chemistry and physics textbooks consider the oil drop experiment to be a simple, classic and beautiful experiment, in which Robert A. Millikan (1868–1953) by an exact experimental technique determined the elementary electrical charge. Polanyi ([1964]) has emphasized the degree to which established knowledge in textbooks departs from the events associated with the original discovery:

> Yet as we pursue scientific discoveries through their consecutive publication on their way to the textbooks, which eventually assures their reception as part of established knowledge by successive generations of students, and through these by the general public, we observe that the intellectual passions aroused by them appear gradually toned down to a faint echo of their discoverer's first excitement at the moment of Illumination… A transition takes place here from a heuristic act to the routine teaching and learning of its results, and eventually to the mere holding of these as known and true, in the course of which the personal participation of the knower is altogether transformed. (pp. 171–2)

Analyses of chemistry and physics textbooks shows that Polanyi ([1964]) had indeed foreseen the dilemma with much acumen (Matthews [1994]; Niaz [2000]; Rodríguez and Niaz [2004]). A historical reconstruction of the events that led to the determination of the elementary electrical charge (*e*) shows the controversial nature of the oil drop experiment then (1910–25) and that the experiment is difficult to perform even today (Jones [1995]).

Acceptance of the quantization of the elementary electrical charge was preceded by a bitter dispute between Millikan (University of Chicago) and Felix Ehrenhaft (University of Vienna, 1879–1952), which lasted for many years (1910–25). Both Millikan and Ehrenhaft obtained very similar experimental results and yet Millikan was led to formulate the elementary electrical charge (electron) and Ehrenhaft to fractional charges (subelectron). Interestingly, however, after almost 90 years historians and philosophers of science do not seem to agree as to what really happened. A review of the literature shows that there have been at least four major attempts to reconstruct the historical details that led to the Millikan–Ehrenhaft controversy:

1. Holton ([1978])
2. Franklin ([1981])
3. Barnes et al. ([1996])
4. Goodstein ([2001])

The interpretations of Holton ([1978]), Franklin ([1981]), and Goodstein ([2001]) were from the additional vantage point of having consulted Millikan's handwritten notebooks available at the Millikan Archives, California Institute of Technology, Pasadena. This shows that with respect to the oil drop experiment we have the controversy not only among the original protagonists but also among those who have tried to understand and interpret the experiment. According to Machamer et al. ([2000]), although most achievements in scientific progress have involved controversies, it is paradoxical that there is a dissociation between science as actually practised and science as perceived by both scientists and philosophers:

> While nobody would deny that science in the making has been replete with controversies, the same people often depict its essence or end product as free from disputes, as the uncontroversial rational human endeavor par excellence. (p. 3)

The objective of this study is a critical appraisal of the four interpretations with respect to the controversial nature of the oil drop experiment and an attempt to provide closure to the controversy.

2 An appraisal of Holton's interpretation

One of the most important aspects of Holton's ([1978]) interpretation is that he attributes Millikan's data selection and reduction procedures to his presuppositions with respect to the atomic nature of electricity. The importance of presuppositions in a research programme has also been recognized by others (e.g. 'hard-core' Lakatos [1970]; 'guiding assumptions' Laudan et al. [1988]). Holton highlights the importance of presuppositions by drawing attention to how Millikan in his first major publication (Millikan [1910], manuscript submitted on 9 October 1909) started with the following words:

> Among all physical constants there are two which will be universally admitted to be of predominant importance; the one is the velocity of light... and the other is the ultimate, or elementary, electrical charge, a knowledge of which makes possible a determination of the absolute values of all atomic and molecular weights, the absolute number of molecules in a given weight of any substance, the kinetic energy of agitation of any molecule at a given temperature, and a considerable number of other important physical quantities. (Millikan [1910], p. 209)

This shows that Millikan had a fairly good idea of what he was looking for, even before the controversy with Ehrenhaft started in 1910. In later publications, Millikan ([1916]) explicitly outlined 'The History of the Idea of a Unit Charge'. Besides Benjamin Franklin, the origin of the idea is attributed to Faraday's discoveries in electrolysis (1833), Stoney's use of the word 'electron' in 1874, Thomson's ([1897]) work at the Cavendish Laboratory; finally Millikan concluded:

> They did . . . 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 [1897], J.J. Thomson [1898], H.A. Wilson [1903], Przibram [1907], Millikan and Begeman [1908], Ehrenhaft [1909] and [De] Broglie [1909] in succession made rough determinations or estimates of the average charge appearing on gaseous ions and found it equal, within the limits of uncertainty . . . to the value estimated for the univalent ions in electrolysis. (Millikan [1916], p. 596)

The inclusion of Ehrenhaft and his colleague Przibram in this list of researchers supporting the atomic theory of electricity may sound strange. Holton ([1978]) shows how in his earlier work Ehrenhaft ([1909]) did subscribe to what he referred to as the 'Elementary Quantum of Electricity' (p. 185).

The second important aspect of Holton's ([1978]) interpretation is that Millikan was convinced not only about the atomic theory of electricity but also about the magnitude of the elementary electrical charge. In Holton's opinion, given the pioneering work of Thomson and colleagues, there was enough evidence to be convinced 'of the particle theory of unitary electrical charge, even prior to Millikan's work' (p. 180). Millikan could foresee the next logical step—determination of the unitary charge. Millikan and Begeman ([1908]), based on work done in 1907 using the cloud method, reported a value of 4.03×10^{-10} esu for the unitary charge. This value was quoted by Rutherford and Geiger ([1908]) and they considered it to be one of the best experimental values available. Rutherford and Geiger had determined the magnitude of the alpha particle charge as 9.3×10^{-10} esu and assumed that it should be equal to $|2e|$ and hence that e (unitary charge) should be 4.65×10^{-10} esu. Millikan and Begeman's ([1908]) value of 4.03×10^{-10} esu was 15% lower, and from then on the value suggested by Rutherford and Geiger was a sort of guidepost for Millikan and many other researchers.

The third important aspect of Holton's ([1978]) interpretation is to have studied Millikan's handwritten notebooks at CALTECH, Pasadena. The notebooks have data from the period 28 October 1911 to 16 April 1912 consisting of about 175 pages. Data from these notebooks were used to publish Millikan ([1913]), a major publication in defence of his methodology. According to Holton's account there were 140 experiments on an equal number of oil drops. In the actual publication complete data on 58 drops are meticulously presented and it is emphasized that

> *[i]t will be seen from Figs. 2 and 3 that there is but one drop in the 58 whose departure from the line amounts to as much as 0.5 percent. It is to be*

> *remarked, too, that this is not a selected group of drops but represents all of the drops experimented upon during 60 consecutive days*. (Millikan [1913], p. 138, original italics)

How do we interpret this information? The laboratory notebooks tell us that there were 140 drops and the published results are emphatic that there were 58 drops. What happened to the other 82 (59%) drops? Herein lies the crux of the difference in the methodologies of Ehrenhaft and Millikan. In other words, Millikan perhaps excluded drops that did not have charge equal to an integral multiple of the elementary electrical charge *e*, as suggested by the work of Rutherford and Geiger ([1908]). Holton ([1978]) wondered what Ehrenhaft's response might have been if he had had access to Millikan's notebooks:

> If Ehrenhaft had obtained such data, he would probably not have neglected the second observations [that did not give the expected value of *e*] and many others like it in these two notebooks that shared the same fate; he would very likely have used them all. (pp. 209–10)

At this stage, it is important to note that Ehrenhaft, too, obtained data that he interpreted as an integral multiple of the elementary electrical charge (*e*). Nevertheless, his argument was precisely that there were many drops that did not lead to an integral multiple of *e*. The crux of the issue is what the warrant was under which Millikan discarded more than half of his observations. The answer is simple if we are willing to give up the so-called scientific method, according to which experimental data inevitably lead to theoretical formulations. In other words, Millikan's presuppositions (existence of an elementary electrical charge and the idea that this charge must have a value within a certain range) provided the warrant. Indeed, according to Holton, Millikan would perhaps have liked to warn Ehrenhaft that all their data could not be used as their experiments were constantly faced with difficulties such as evaporation, changes in the sphericity, radius and density of droplets, validity of Stokes's law and other experimental variables (battery voltages, stopwatch errors, temperature, pressure, convection and so on).

Finally, Holton ([1978]) makes the following important observation with respect to Millikan's handling of the data: 'It appears likely that after almost every run Millikan made some rough calculations of *e* on the spot, and often he appended a summary judgment' (p. 211). As we see in the next section, this procedure makes the decision with respect to how and why Millikan selected some drops and not others extremely difficult. In other words, Millikan could have decided to exclude a drop as the first observations provided sufficient indication that it might not come close to the 'expected correct' value of *e*.

3 An appraisal of Franklin's interpretation

Franklin ([1981]) also consulted Millikan's handwritten notebooks and his report was published in the same journal as that of Holton ([1978]). Some of the salient features of his interpretation are discussed in this section. Franklin reports (p. 187) that the notebooks covering the period from 28 October 1911 to 16 April 1912 contain data on 175 drops. There is no explanation as to why this number differs from that (140 drops) provided by Holton ([1978]).

According to Franklin ([1981]): 'Millikan had a good idea of the results he expected although his expectations seem to have changed as he went along . . . For the early events . . . he was comparing his observations to the value of $e = 4.891 \times 10^{-10}$ esu given in his paper of 1911' (p. 192). Franklin ignores the fact that besides the value of e from his 1911 paper, Millikan was guided by the value of e suggested by Rutherford and Geiger ([1908]). Furthermore, it is not clear what Franklin means by 'his expectations seem to have changed as he went along'. In fact, if there was one thing that could characterize Millikan's approach it was his continued belief and perseverance in his original expectations (presuppositions) regarding the atomic nature of electricity and a value of e that approximated that of Rutherford and Geiger ([1908]). Interestingly, Franklin makes no mention of this reference.

Franklin tries to convince the reader that Millikan's data before 13 February 1912 should not be considered as he was still trying to get his apparatus to work properly—perhaps a warm-up period. Based on Franklin ([1981]) the oil drops could be classified as follows:

i.	Total number of drops in the notebooks	175
ii.	Number of drops studied before 13 February 1912	68
iii.	Number of valid drops	107
iv.	Of the valid drops, number published by Millikan	58
v.	Of the valid drops, number excluded by Millikan	49
vi.	Of the valid drops, number excluded with no calculation	22
vii.	Of the valid drops, number excluded after calculating e	27

Reasons for excluding 27 valid drops even after having calculated the value of e:

- 12 drops: their values of pressure and radius were such as to require second-order correction to Stokes's law
- 2 drops: on experimental grounds
- 5 drops: they had three or fewer reliable measured values of t_{f} (represents the time of rising of the drop under the field)

- 2 drops: for no apparent reason, probably because Millikan did not need them for calculating *e*
- 1 drop: anomalous (Franklin provides no further information)
- 5 drops: according to Franklin, 'His only evident reason for rejecting these five events is that their values did not agree with his expectations' (p. 195)

Exclusion of 68 drops (studied before 13 February 1912) on the grounds that presumably the apparatus was not working well is questionable. Apparently, Millikan made no explicit qualification in the notebooks and nor does Holton allude to this aspect. Furthermore, given Millikan's general procedure of making 'rough calculations of *e* on the spot' (Holton [1978], p. 211) it is extremely doubtful that all 68 drops were excluded owing to difficulties with the apparatus. It is plausible to suggest that Millikan may have excluded a drop as initial observations could have given him a rough estimate of the value of *e*.

The last five drops make an interesting case. Franklin attributes their exclusion to values that did not agree with Millikan's expectations. So in the ultimate analysis, Millikan's expectations (perhaps presuppositions) were after all important. Nevertheless, Franklin does not tell us anything explicitly about Millikan's expectations. Of the last 27 drops excluded after having calculated the value of *e*, one could accept Franklin's advice with respect to the first 12 (required second-order correction to Stokes's law). For the other 15 drops there was apparently no plausible reason, except that the value of *e* was not the expected one.

Does Franklin's selective analysis of oil drops convince the reader? I am afraid not. Let us summarize all the information. There were 175 drops in the notebooks; 68 were excluded as the apparatus was presumably not working well; another 49 were excluded even when the warm-up period was over; of these, 22 were excluded for reasons that are not very clear and, of the other 27, at least 15 were excluded owing to an unexpected value of *e*. The reader may now be in a greater quandary than after having read Holton ([1978]). It appears that Millikan excluded not 59% (82 out of 140, as per Holton) of the drops, but rather 67% (117 out of 175, as per Franklin) of the drops. In order to convince the reader, Franklin also plotted (his figure 4) the value of *e* against the number of the drops studied (order of event). Franklin used the recalculated value of *e* for all the published (58) and unpublished (25) drops. The reader, however, will recall that there were 49 unpublished drops after 13 February 1912, and of these Millikan did not calculate the value of *e* for 22 drops—apparently for experimental reasons. One could ask why all the 49 unpublished drops were not included in figure 4. This could have helped to make Millikan's (and also Franklin's) case stronger. What is even more troublesome is the fact that 13 of the 25 drops included in figure 4 were among the 22 drops for which Millikan did not calculate the value of *e*. Franklin calculated a mean value of *e* based on his recalculation of the 58 published drops to be 4.777×10^{-10} esu (statistical error $= 0.003$) and for the 25 unpublished drops a mean value of 4.789×10^{-10} esu $\pm$ 0.007. Although Franklin's selection of drops helped to decrease the statistical error, one could argue that given the controversy with respect to the selection of drops by Millikan, readers would have liked to see the scatter in figure 4, if it had been based on the 49 unpublished drops.

According to Franklin ([1981]): 'Millikan's cosmetic surgery touched 30 of the 58 published events, from which he excluded one or more (usually less than three) observations. For example, in the case of drop No. 15, Millikan used only eight of the twelve measurements of t_f in calculating *e*' (p. 197). Franklin plots, in his figure 5, Millikan's calculated values of *e* (with cosmetic surgery) and his own recalculated values for all 58 published drops and concludes, 'the result of his [Millikan's] tinkering is to reduce the statistical error rather than to change the mean value of *e* (Fig. 5)' (p. 198). Thus, Millikan not only excluded drops (Holton [1978]) but also resorted to cosmetic surgery and selective analysis of data (Franklin [1981]).

Franklin refers to three anomalous events (drops), and of these the second drop of 16 April 1912 worries him the most:

> This event is the most worrisome of the three since it is among Millikan's very best observations, as shown by internal consistency of the values... With a second-order correction to Stokes' law, $e = 2.810 \times 10^{-10}$ esu... both the charge on the drop, as well as the changes in charge, must be fractional, a highly unlikely occurrence. Once again neither dust nor voltage problems can explain the anomaly. Millikan remarked, 'something wrong w[ith] therm[ometer],' but there is no temperature effect that could by any stretch of the imagination explain a discrepancy of this magnitude. Millikan may have excluded this event to avoid giving Ehrenhaft ammunition in the controversy over the quantization of charge. (Franklin [1981], pp. 200–1)

This is indeed quite revealing and shows how a discrepant event could appear even on the last day of the notebook readings, which weakens Franklin's argument about Millikan having excluded 68 drops studied before 13 February 1912. Furthermore, this is the only explicit reference (besides a brief mention on pp. 191–2) to the controversy with Ehrenhaft.

One of the least convincing aspect of Franklin's ([1981]) study is his repeated defence of exclusion of drops by Millikan on the grounds that he had more data than he needed for calculating *e*. He uses this argument explicitly on at least five different occasions: (1) p. 192, ll. 8–10; (2) p. 192, ll. 20–22; (3) p. 194, ll. 13–15; (4) p. 194, last two lines; (5) p. 195, ll. 19–20, where he refers to 2 of the 27 drops that Millikan excluded after 13 February 1912 and states: 'and [excluded] two for no apparent reason, probably because he did not need them for calculating *e*'. Given the controversy with Ehrenhaft it is difficult to imagine how Millikan could throw away data.

Finally, it is important to note that Franklin ignored (except for a brief mention) not only the fact that the Millikan–Ehrenhaft controversy played an important part in the determination of the elementary electrical charge but also that Ehrenhaft, too, was implicitly guided by his presuppositions (an anti-atomist empiricist framework).

4 An appraisal of Barnes, Bloor and Henry's interpretation

Barnes et al. ([1996]) outline their plan of action by recognizing the complexity of the experiment in the following terms:

> Was there a smooth, automatic, unproblematic path joining the readings entered into the laboratory notebook and the data in the published papers that were used by Rutherford, Bohr and the rest of the scientific community? As Holton makes clear, the answer to this question is negative. The route from the notebook to the published paper was complex and interesting. (p. 22)

Next, the authors recognize that given some latitude for experimental error, charges on a series of drops can be shown to be an integral multiple of a supposed unit value, '*provided the unit is made small enough*' (p. 23, original italics). In other words, Millikan was using a prior belief (presupposition) that the unit of charge was in the region of 4.7×10^{-10} esu. At this stage, Barnes et al. ([1996]) ask a very pertinent question: 'Were *all* of the cases in which Millikan discarded readings ones where he had grounds for suspecting the apparatus, and grounds that were independent of the readings it was producing' (p. 24, original italics). Based on the work of Holton the authors respond to this question in the negative.

With this introduction, the authors present their framework (based on Mannheim [1952]; Garfinkel [1967]) for understanding the oil drop experiment in cogent terms:

> Mannheim asked us to reflect on the predicament of someone trying to understand some fragments of a document. If they knew the import of the whole document, then they could make sense of the fragments . . . We can understand Millikan's work by seeing him in an analogous hermeneutic predicament, and as adopting an analogous method. His experimental data are his fragments. The whole document is the unknown reality that underlies and produces them. His guiding theory is the meaning he imputes to the document, a theory that guides his response to the evidential fragments, deciding which are reliable, which have undergone corruption or alteration or decay or misunderstanding. (Barnes et al. [1996], p. 25)

In view of the framework outlined above, the next section, entitled 'A sociological reading', is problematic. According to the authors:

> Millikan's apparatus—as even the minimally physically informed observer can see—merely permitted electricity to interact with drops of oil. It therefore dealt with the *interface* between electricity and gross matter. To make the move from this interface to conclusions about the nature of electricity *as such* involves a special inference. What is more, this inference is one that cannot be sanctioned by the general principles of deductive reasoning nor by common sense. Not even the most unsophisticated person is inclined to think that because he draws water from a well by the bucketful, that means that water exists naturally in bucket-sized units. Water is a continuous fluid, and it is we who divide it when we scoop it up. The man-water interface, as we might call it, doesn't sanction inferences to the nature of water as such. Why then should we be so impressed by Millikan when he makes the corresponding move from the oil-electricity interface to the nature of electricity as such? . . . The ultimate cause of our acceptance is that a sufficient number of trusted authorities can be found who are prepared to let such an inference pass muster, or even encourage it, even though in other circumstances (e.g., standing round the village well) they would laugh at it. (Barnes et al. [1996], pp. 26–7, original italics, underlining added)

This quotation represents an interesting piece of thinking about scientific methodology. I would agree with the first part and would like to endorse it by citing from a methodologist, Campbell ([1988]):

> the objectivity of physical science does *not* come from the fact that single experiments are done by reputable scientists according to scientific standards. It comes instead from a social process which can be called competitive cross-validation . . . and from the fact that there are many independent decision makers capable of rerunning an experiment, at least in a theoretically essential form. The resulting dependability of reports . . . comes from a social process rather than from dependence upon the honesty and competence of any single experimenter. (pp. 302–3, original italics)

Furthermore, Campbell conceptualizes the social aspect of science as a systematic norm of distrust (organized scepticism), which facilitates validity by peer monitoring. It is plausible to suggest that 'trusted authorities' (Barnes et al. [1996]) and 'independent decision makers' (Campbell [1988]) perhaps play the same role in scientific methodology.

In the second part, Barnes et al. ([1996]) state that 'water is a continuous fluid' and that the 'man-water interface' does not sanction inference with respect to the nature of water. To be precise, it is the atomic theory that shows that based on the particulate nature of matter, 'water is not a continuous fluid'. Understanding the particulate nature of matter is counterintuitive, and it is no wonder that high school and even college students have considerable difficulty (Gabel and Bunce [1994]). As a thought experiment, one could even argue that water could be scooped from a well as bucket-sized units, if the size of the bucket was adjusted to contain exactly 2.988×10^{-23} grams (one molecule) of water. However, the important point is that both the particulate nature of matter and the elementary electrical charge are sanctioned by the 'village elders' meeting around the well. Generally, the decisions taken at the village well, both in the past and at present, are well corroborated by experience over a period of time. My contention is that the decisions taken by the village elders are validated by peer monitoring. In the case of the elementary electrical charge the village well meeting could have been the 79th Meeting of the British Association for Advancement of Science held in Winnipeg, Canada, from 25 August to 1 September 1909. Holton ([1978]) has already drawn attention to the importance of this meeting, at which the presidential address was delivered by J. J. Thomson, and E. Rutherford addressed the Mathematical and Physical Science Section. Rutherford emphasizes the role played by atomic theory and traces its origin to Dalton's work in 1805, followed by the contributions of J. C. Maxwell and R. Clausius, 'Brownian movement', and Perrin; finally he sets the stage for what at that time was paramount:

> We have referred earlier in the paper to the work of Ehrenhaft on the Brownian movement in air shown by ultra-microscopic dust of silver. In a recent paper ([1909]) he has shown that each of these dust particles carries a positive or negative charge. The size of each particle was measured by ultra-microscope, and also by the rate of fall under gravity. The charge carried by each particle was deduced from the measured mass of the particle, and its rate of movement in an electric field. The mean value of e was found to be 4.6×10^{-10}. (Rutherford [1909], p. 380)

In the next paragraph, Rutherford compares Ehrenhaft's value of e to his own (4.65×10^{-10}) and considers it to be of 'considerable confidence'. I wonder whether the village elders could provide greater insight into what was at stake. Robert Millikan was of course at the meeting and imbibed the lesson completely. Ehrenhaft was not at the meeting, and nor did he see the writing on the wall.

The role played by Ehrenhaft was, however, complex. After 1909, he interpreted his data to show that there was little evidence for the elementary electrical charge. Holton ([1978]) has shown how Ehrenhaft was greatly influenced by the anti-atomist and empiricist philosophy of E. Mach and W. Ostwald. Anti-atomists' ideas were still strong in 1910 and it is no surprise that both Millikan and Ehrenhaft played their part in this ongoing debate. Brush ([1976]) has presented this conflict succinctly:

> The leaders of this reaction [against the atomic theory], in the physical sciences were Ernst Mach, Wilhelm Ostwald, Pierre Duhem, and Georg

> Helm. Mach recognized that atomic hypotheses could be useful in science but insisted, even as late as 1912, that atoms must not be considered to have a real existence. Ostwald, Duhem, and Helm, on the other hand, wanted to replace atomic theories by 'Energetics' (a generalized thermodynamics); they denied that kinetic theories had any value at all, even as hypotheses. (p. 245)

Wilhelm Ostwald wielded considerable influence in scientific circles and in 1904 was invited by the Royal Society itself to deliver the Faraday Lecture. This is how a historian has summarized Ostwald's (enfant terrible of physical chemistry) presentation:

> Ostwald came to London and gave a stunning performance. It was not a talk aimed at convincing his audience. It was a talk aimed at crushing his audience. Ostensibly, his purpose was to show that all the laws of chemistry that could be deduced from the atomic hypothesis could equally well be deduced from the theory of chemical dynamics, which he told them was the most significant achievement of modern chemistry... One could almost feel the audience fuming, prevented by etiquette from shouting against the sacrilege so unashamedly perpetrated by Ostwald at the very heart of London, at the Lecture Theatre, in fact, of the Royal Institution, where so much had been said about the real atoms all these past years. (Gavroglu [2000], pp. 184–5)

We have been witness to two village well conversations—one in Winnipeg, Canada, in 1909 and the other in London in 1904—and in both leading village elders played an important role. This was the philosophical milieu that pervaded the first decade of the twentieth century. Based on this scenario it is plausible to suggest that Millikan and Ehrenhaft were strongly committed to the two leading philosophical currents of the time and, on this basis, presented plausible hypotheses. Ehrenhaft's data could be explained by an alternative rival hypothesis which was equally plausible, namely, the anti-atomist research programme of Mach and Ostwald. In a sense, the Millikan–Ehrenhaft controversy perhaps even represents an integral part of scientific development. Campbell ([1988]) emphasizes the rivalry and even proliferation of plausible hypotheses in both the social and the natural sciences. Similarly, Lakatos ([1970]) is quite emphatic: 'proliferation of theories cannot wait until the accepted theories are "refuted" (or until their protagonists get into a Kuhnian crisis of confidence)' (p. 122).

Barnes et al. ([1996]) have raised another important issue, which I would like to develop further: 'we should avoid the inference to the rightness of Millikan's theory from the fact that it works' (p. 30). Based on Millikan's notebooks and data reduction procedures it is quite clear that his theory was not right even for some of his own data. Scientific theories need be evaluated not on the basis of rightness or wrongness, but instead on their heuristic power. Furthermore, according to Lakatos ([1970]), scientific theories are tentative. For example, was Thomson's ([1897]) theory right at the time and later wrong after Rutherford ([1911]) published his model of the atom? Rutherford's model increased the heuristic power of the theory, just as Millikan's determination of the elementary electrical charge increased the heuristic power of the atomic theory even further. Lakatos ([1970], p. 123) attributes this conflation between 'proven truth' and 'heuristic power' to both W. Whewell and P. Duhem.

On the whole, Barnes et al. ([1996]) have facilitated our understanding of the experiment and thus played a role envisaged by Fuller ([2000]): 'sociologists can step into the breach when philosophers [Holton and Franklin in this case] cannot decide among themselves which methodology best explains a certain historical episode of scientific theory choice' (p. 141). Furthermore, Kitcher ([2000]) has cautioned: 'Philosophical attempts to make the ultimately triumphant position rationally preferable even at early stages in the controversy seem to be doubly unfortunate' (p. 27). Similarly, Machamer et al. ([2000], p. 16) have recommended that it would be advisable that philosophers, historians and sociologists do not neglect each other's work.

5 An appraisal of Goodstein's interpretation

Of the four interpretations included in this study, the one by Goodstein ([2001]), who also had access to the Millikan's archives, was explicitly conducted to defend Millikan's methodology:

> What scientist familiar with the vagaries of *cutting-edge experimental work* would fault Millikan for picking out what he considered to be his most dependable measurements in order to arrive at the most accurate possible result? (Goodstein [2001], p. 57, italics added)

Goodstein rightly recognizes that in 'cutting-edge experimental work' scientists do resort to picking out data in order to arrive at the most accurate possible result. This raises two questions: how did Millikan know in 1913 what was 'the most accurate possible result' and did Millikan's 'picking out' of data alter the right or the most accurate possible result? Goodstein leaves the first question unanswered and for the second presents Franklin's treatment of the original data, which shows that had Millikan published data from all the drops available, this would not have changed the final value of e. This leads us to yet another question: did Millikan perform the sort of analyses conducted by Franklin ([1981]) on published and unpublished drops in order to justify the picking out of data? Apparently, based on the interpretations of Holton ([1978]), Franklin ([1981]) and Goodstein ([2001])—all three of whom consulted/checked Millikan's original notebooks—we have no evidence to the effect that Millikan could have performed Franklin-style analyses before deciding which drops to discard.

Interestingly, Franklin ([1997]) implicitly responds to this question by acknowledging that

> I speculate that this exclusion [drop of 16 April 1912 that gave a value of *e* 40% too low] was simply to avoid giving Felix Ehrenhaft ammunition in the charge-quantization controversy. Later analysis has shown that the data for this drop were indeed unreliable. (p. 28; for later analysis Franklin refers the reader to Fairbank and Franklin [1982])

Given the importance of this drop of 16 April 1912 (Franklin [1981], p. 200, considered it to be the most worrisome), one would have expected a detailed treatment in later analyses. Surprisingly, however, Fairbank and Franklin ([1982]) make no mention of this drop. Similarly, a later publication (Franklin [1986]) provides no further information with respect to this drop. In other words, given the controversy with Ehrenhaft in 1913, Millikan discarded data without performing the sort of analyses conducted later by Franklin ([1981]) and Fairbank and Franklin ([1982]). One could go further and speculate that if Millikan had conducted such analyses, it could have provided him with a good defence for excluding drops and he most probably would have included it in his publication (Millikan [1913]). This, of course, leads to the big issue: in 1913, there was no way for Millikan to have known with certainty what the 'expected correct' value for *e* was. His best guides were his presuppositions and the advice of the village elders (Rutherford and Geiger [1908]).

Goodstein ([2001]) is at pains to justify/explain the following problematic statement from Millikan ([1913]): 'It is to be remarked, too, that this is not a selected group of drops but represents all of the drops experimented upon during 60 consecutive days' (p. 138). Goodstein ([2001]) cites this statement three times in his paper with the following comments: 'The question is why did Millikan mar his masterpiece [Millikan [1913]] with a statement that is clearly false' (p. 57); 'still, Millikan's paper contains that nagging and blatantly false . . .' (p. 58); '[that] damning remark' (p. 59). Finally, in an attempt to diminish the significance of the statement, Goodstein ([2001]) resorts to a truly tautological argument:

> So the damning remark is made, not about whether charge comes in units or what the value of *e* is, but in regard to getting the correction to Stokes's law right. Millikan is merely saying here that all of the 58 drops he just discussed confirm his presumed formula for amending Stokes's law. (p. 59)

Based on this presumption, Goodstein ([2001]) goes on to conclude: 'Thus, a careful reading of the context of Millikan's words greatly diminishes their apparent significance as evidence of misconduct' (p. 59). This raises an important issue: Millikan could easily have reported that based on his correction of Stokes's law, only 58 drops qualified for the study. He did not have to mention that these 58 drops formed part of a bigger group of drops. On the contrary, in order to make his case stronger (given the challenge from Ehrenhaft) he stated emphatically: '*this is not a selected group of drops but represents all of the drops experimented upon*' (p. 138, original italics).

In order to diminish the significance of Millikan's 'misconduct', Goodstein ([2001]) reproduces the following passage from Millikan ([1913]):

> Table XX, contains a *complete* summary of the results obtained on all of the 58 different drops upon which complete series of observations like the above were made during a period of 60 consecutive days. (Millikan [1913], p. 133; Goodstein [2001], p. 58; italics added)

Goodstein comments on this assertion by Millikan ([1913]) in the following terms: 'Millikan didn't detail why he had not considered his evaluation of some drops to be sufficiently complete . . . The clear implication of this statement in his paper is that there *were* drops for which the data were not complete enough to be included in the analysis' (Goodstein [2001], pp. 58–9, italics in original, underlining added).

Many years later, Millikan ([1947]) recounted his experiments reported in Millikan ([1913]) in much the same way as in the original. There are, however, some significant changes, as can be observed from the following (cf. the quote from Millikan [1913], p. 133, cited above): 'The numerical data from which these curves are plotted are given *fairly fully* in Table IX (Table XX in Millikan [1913]). It will be seen that this series of observations embraces a study of 58 drops. These drops represent all of those studied for 60 consecutive days, *no single one being omitted*' (Millikan [1947], pp. 108–11, italics added. The quotation starts on p. 108 and finishes on p. 111, pp. 109–10 being devoted to Table IX, reproducing exactly Table XX of Millikan [1913]).

It can be observed that the word 'complete' (important for Goodstein) has been replaced by 'fairly fully' and the phrase 'no single one being omitted' has been added in Millikan ([1947]). How do we interpret these changes? Apparently, Millikan is alluding to the fact that the data presented in Table XX/IX are after all not complete but 'fairly fully [complete]'. Furthermore, the reference to 'no single one being omitted' is obviously more categorical. This, in the sense that the data presented are based on all the drops studied, and hence makes the explanation for having excluded drops even more problematical and thus the evidence for 'misconduct'. Indeed, if we try to understand Millikan's handling of data with no reference to his presuppositions (Holton [1978]) or the 'hard core' of his research programme (Lakatos [1970]), then some degree of 'misconduct' can be perceived.

6 A crucial test: the second drop (reading) of 15 March 1912

This drop can provide further insight into Millikan's handling of the data as it formed part of the handwritten notebooks and was studied within the period in which Franklin considered Millikan's apparatus to be working well. Holton ([1978]) devotes almost a page (pp. 209–10) to analysing Millikan's handling of this drop and also reproduces the corresponding page from the handwritten notebooks (his figure 4, p. 207). This was a heavy drop, and it did not give the results expected by Millikan, which led him to note in his notebook, '*Error high* will not use' (italics by Millikan, reproduced in Holton [1978], p. 209). About three cm below this Millikan added: 'Can work this up & probably is ok but point is [?] not important. Will work if have time Aug. 22' (reproduced in Holton [1978], p. 209). Holton considered that the latter part was added 'probably later' (p. 209). It is plausible to suggest that guided by his presuppositions, Millikan did not waste time in investigating the reason for the high error and instead went on to study another drop.

Now let us see how Franklin ([1981]), who also reproduced the corresponding page from the notebooks (his Figure 1, p. 188) handled this drop. Franklin pointed out that the exclusion of this drop had 'bothered Holton' (p. 195). After providing some details about the results, Franklin ([1981]) concluded:

> Although there is a substantial difference between the results of the two methods of calculation, larger than any for which Millikan gave data in his published paper, it is no larger than the difference in some events that Millikan published, as shown in the laboratory notebooks. There is no reason to assume, as Holton seems to do, there were unstated and perhaps unknown experimental reasons for the exclusion of this event. (p. 195)

No further arguments or analyses are presented of why the drop was excluded or of what could have bothered Holton.

Let us go back to Holton ([1978]) to see how he interpreted the data with respect to this drop:

> the entries on the right-hand page [figure 4, p. 207], which Millikan abandoned, make excellent sense if one assumes that the smallest charge involved is not *e* but, say, one tenth *e* . . . From Ehrenhaft's point of view, it is the assumption of integral multiples of *e* that forces one to assume further, without proof, a high 'error' to be present and thus leads one to the silent dismissal of such readings and hence of the possibility that the quantum of electric charge may be 0.1*e*. (p. 210)

This clearly shows the role of presuppositions in the interpretation of data. For Millikan this drop simply represented a case of 'high error' and, according to Franklin, there were other drops with similar characteristics. On the other hand, for Ehrenhaft this drop provides evidence for the existence of fractional charges (0.1*e*). Readers would have liked to see Franklin's detailed analysis with respect to this dilemma, and thus the provision of a rebuttal to Holton's interpretation.

Surprisingly, Goodstein ([2001]) makes no attempt to analyse Holton's ([1978]) interpretation of this drop. Barnes et al. ([1996]) have rightly recognized the importance of this drop:

> The point at which one would expect the import of Franklin's criticism to stand out with maximum clarity would be in his handling of the very example on which Holton's case crucially depends—the discarded second reading of 15 March 1912, where Millikan appeared to have captured a subelectron charge *e*/10. What did Franklin make of this ?... Nothing Franklin says shows Holton is wrong. (pp. 44–5)

Finally, it is instructive to go back once again to Holton ([1978]) to understand the dilemma: 'In Millikan's terms, on the contrary, such an interpretation of the raw readings would force one to turn one's back on *a basic fact of nature*—the integral character of *e*—which clearly beckoned' (p. 210, italics added). Could 'a basic fact of nature' here represent the 'presuppositions'. The crux of the issue is that Holton's interpretation based on Millikan's methodology of defending his 'presuppositions' has not been rebutted in subsequent interpretations. In other words, there was an alternative framework (Ehrenhaft's) based on a different set of suppositions, namely, the quantum of electric charge could have been 0.1*e*, that could have explained the data.

7 Conclusion: Is closure possible?

It is almost 90 years since most of the experimental work that led to the determination of the elementary electrical charge was conducted. Since then there has been considerable controversy among physicists, historians, philosophers and sociologists over how data were collected and the reduction procedures employed. Three of the researchers have had access to Millikan's handwritten notebooks. Given these circumstances, it is plausible to attempt closure. In order to facilitate that closure, let us consider some hypothetical questions that are discussed in this section.

If Millikan's handwritten notebooks had been lost (or for that matter if Holton, Franklin and Goodstein had not consulted them), the oil drop experiment would have remained an enigma for some, whereas for others it would have been a classical test case of how experiments unambiguously lead to theoretical formulations. The availability of Ehrenhaft's notebooks would also have helped further our understanding of the experiment.

What would have been the consequences if the scientific community had recognized Ehrenhaft's findings and not Millikan's? This leads to a corollary, namely, what would have been the consequences if Millikan's formulation of the elementary electrical charge could not have been sustained by further experimental evidence? This issue is crucial as there is evidence to show that the choice between Millikan and Ehrenhaft was not automatic. Release of the Nobel Prize archives shows that

> although Millkan was nominated for the prize in physics regularly from 1916 on, Svante Arrhenius, in the report he prepared on Millikan's work for the deliberations of the committee, noted as late as 1920 that even though most physicists had come to agree with Millikan in the dispute with Ehrenhaft, the matter was not yet resolved, and that Millikan should therefore not then be recommended for the prize. (Holton [1988], p. 196)

Millikan finally got the Nobel Prize in 1923. This contrasts with Franklin's ([1981], p. 191) claim that the question of charge quantization was already settled even before Millikan ([1913]) was published. A plausible course of events if Millikan's findings in the long run wrong had been proven untenable is provided by Olenick et al. ([1985]):

> He [Millikan] had a pretty clear idea of what the result ought to be—scientists almost always think they do when they set out to measure something . . . it's actually a powerful bias to get the result he wants, because you can be sure that when he got a result he liked, he didn't search as hard to see what went right. *But experiments must be done in that way. Without that kind of judgment, the journals would be full of mistakes, and we'd never get anywhere. So, then, what protects us from being misled by somebody whose 'judgment' leads to a wrong result? Mainly, it's the fact that someone else with a different prejudice can make another measurement . . . Dispassionate, unbiased observation is supposed to be the hallmark of the scientific method. Don't believe everything you read. Science is a difficult and subtle business, and there is no method that assures success*. (p. 244, italics added; David Goodstein, one of the co-authors apparently changed his mind in Goodstein [2001])

Millikan and Ehrenhaft, would never have discussed their experimental findings in the context of 'a different prejudice' and the problematical nature of 'dispassionate, unbiased observation'. However, during the controversy, at one stage Millikan came quite close: 'That these same ions have one sort of charge when captured by a big drop and another sort when captured by a little drop is obviously *absurd* . . . Such an assumption [existence of a whole range of fractional charges, as suggested by Ehrenhaft] is not only too *grotesque* for serious consideration but is directly contradicted by my experiments' (Millikan [1916], p. 617, italics added). In other words, Millikan was trying to convince the reader by recognizing that experimental observations are important but there is something even more important, namely, presuppositions, and any data that go against them would appear to be 'absurd' and even 'grotesque'; hence subelectrons could not exist.

Even philosophers of science would now recommend that in cutting-edge scientific research, the collection of experimental data (Ehrenhaft's strategy) in itself may not lead to new insights:

> Our vision of reality, to which our sense of scientific beauty responds, must suggest to us the kind of questions that it should be reasonable and interesting to explore. It should recommend the kind of conceptions and empirical relations that are intrinsically plausible and which should therefore be upheld, *even when some evidence seems to contradict them*, and tell us also . . . what empirical connections to reject as specious, even though there is evidence for them—evidence that we may as yet be unable to account for on any other assumption. (Polanyi [1964], p. 135, italics added)

It is plausible to suggest that Ehrenhaft's methodology approximated the traditional scientific method, which did not allow him to discard 'specious drops'. Millikan, on the other hand, in his publications espoused the scientific method, but in private (see his handwritten notebooks) was fully aware of the dilemma faced and was forced to select data in order to uphold his presuppositions. No wonder, according to Fuller ([2000]), that 'something called "the scientific method" is at best a philosophical caricature of what science is really about' (p. 212). As an indicator of how scientific methodology has progressed it is remarkable that even physicists now recognize in public that

> [c]hoices in the design of speculative experiments [cutting-edge] usually cannot be made simply on the basis of reason. The experimenter usually has to base her or his decision partly on what feels right, partly on what technology they like, and partly on what aspects of the speculations [presuppositions] they like. (Perl and Lee [1997], p. 699)

Interestingly, Martin Perl, recipient of the 1995 Nobel Prize for Physics, is at present working on Millikan-type experiments in order to isolate fractional charges (quarks).

A closure to the controversy with respect to the oil drop experiment is possible if we recognize that Millikan's data selection procedure depended primarily on his perseverance with his presuppositions, namely, the existence of the elementary electrical charge and its magnitude, based on previous studies. Franklin's ([1981]) finding that the selection of the drops did not change the value of the elementary electrical charge (e) but only its statistical error carries little weight as Millikan did not perform Franklin-style analyses that could have justified the exclusion of drops. Furthermore, acceptance of Franklin and Goodstein's arguments approximates quite closely what Kitcher ([2000]) has critiqued: 'The most primitive type of rationalism proposes that scientific controversies are resolved by designing and carrying out crucial experiments' (p. 21).

Finally, Millikan himself, in his Nobel Prize speech, provided a clue to his methodology, which has been generally ignored by historians, philosophers and physicists:

> Indeed, *Nature* here was very kind. She left only a narrow range of field strengths within which such experiments as these are all possible. They demand that *the droplets be large enough so that Brownian movements are nearly negligible, that they be round and homogeneous, light and non-evaporable, that the distance be long enough to make the timing accurate, and that the field be strong enough to more than balance gravity by its pull on a drop carrying but one or two electrons*. Scarcely any other combination of dimensions, field strengths and materials, could have yielded the results obtained. (Millikan [1965], pp. 57–8, italics added)

It is not far-fetched to suggest that Millikan's mention of 'Nature' could very well mean 'presuppositions' and the stringent experimental variables mentioned (given the difficulty of getting the right combination) inevitably leads to selection of drops. A recent attempt to study the structure of scientific controversies shows that the closure point cannot be fixed by logical or methodological rules and finally recognizes the role of 'presuppositions': 'What science says the world is like at a certain time is affected by the human ideas, choices, expectations, prejudices, beliefs, and assumptions holding at that time' (Machamer et al. [2000], p. 6).

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