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# Historical Studies in the Physical Sciences

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# Subelectrons, Presuppositions, and the Millikan–Ehrenhaft Dispute

By Gerald Holton\*

## 1. INTRODUCTION

Peter Medawar is one of the few first-rank research scientists still concerned with the problem of knowledge—the sources, warrants, and degrees of certainty of scientific findings, the interplay between fact and belief and between perception and understanding. In *The Art of the Soluble* he asks: “What sort of person is a scientist, and what kind of act of reasoning leads to scientific discovery and the enlargement of the understanding?”<sup>1</sup> He finds the usual approaches too limited: “What scientists *do* has never been the subject of a scientific, that is, an ethological inquiry. . . . It is no use looking to scientific ‘papers,’ for they not merely conceal but actively misrepresent the reasoning that goes into the work they describe. . . . Only unstudied evidence will do—and that means listening at a keyhole.”<sup>2</sup>

Medawar proposes that to study scientific activity one should live in the laboratory or in the theoretician’s workroom and observe the work as it is carried out. To approach Medawar’s aim when dealing with historical problems, historians and sociologists regularly make use of unselfconscious evidence such as letters, autobiographical reports crosschecked by other documents, oral history interviews conducted by trained historians, transcripts of conversations that took place in the heat of battle at scientific meetings, and, above all, laboratory notebooks—first-hand documents directly rooted in the act of doing science, with all the smudges, thumbprints, and bloodstains of the personal struggle of ideas.

These sources can help us in understanding the beliefs and activity of some scientists and how they dealt with new ideas at times when systematic tests of these ideas, if available at all, were difficult to trust or apply. In this study I treat that period following the earliest phase of discovery, when the stirrings of a new conception are difficult to document and before the new work has been absorbed into the

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<sup>1</sup>P. B. Medawar, *The Art of the Soluble* (London, 1967), p. 7.

<sup>2</sup>*Ibid.*, pp. 151, 155.

mainstream of science through the mechanisms of justification. In this period one may hope to find evidence of the fragile and obscure process of science in the making which has been explicitly avoided by Hans Reichenbach,<sup>3</sup> K. R. Popper,<sup>4</sup> and others.

## 2. "THE MOST FUNDAMENTAL QUESTION OF MODERN PHYSICS"

This study centers on the events around 1910 that led two physicists into opposite directions—one to "success" and the Nobel prize, the other to "failure" and eventually a broken spirit. Initially, the protagonists of this study seemed not well matched. Robert A. Millikan was a practically unknown professor at the new University of Chicago, a man over forty years old, with few scientific publications. Felix Ehrenhaft, at the venerable University of Vienna, was regarded as an accomplished physicist, was eleven years younger than Millikan, and had a dozen publications.<sup>5</sup> Their disagreement was about the value of the smallest electric charge found in nature. Both men recognized that the subject of their experimental research, as well as the import of their controversy, went to the foundations of science. Yet today this controversy is virtually forgotten. Failures and the disputes they caused are not remembered in science, and they are rarely analyzed in histories of science.

Millikan's first major paper begins:

Among all physical constants there are two which will be universally admitted to be of predominant importance; the one is the velocity of light, which now appears in many of the fundamental equations of theoretical physics, and the other is the ultimate, or elementary, electrical charge, a knowledge of which makes pos-

<sup>3</sup>Hans Reichenbach, "The Philosophical Significance of the Theory of Relativity," *Albert Einstein: Philosopher-Scientist*, ed. Paul Arthur Schilpp (Evanston, Ill., 1949), p. 292.

<sup>4</sup>K. R. Popper, *The Logic of Scientific Discovery* (New York, 1959), p. 31.

<sup>5</sup>The best available bibliographies are, for Millikan, *Biographical Memoirs of the National Academy of Sciences*, 33 (1959), 270–282; and for Ehrenhaft, Lotte Bittner, *Geschichte des Studienfaches Physik an der Wiener Universität in den letzten hundert Jahren* (diss., University of Vienna, 1950). While useful, both items have similar gaps: each omits a key paper by each man, Millikan's "A New Modification of the Cloud Method of Measuring the Elementary Electrical Charge, and the Most Probable Value of That Charge," *Physical Review*, 29 (1909), 560–561, and Ehrenhaft's "Über eine neue Methode zur Messung von Elektrizitätsmengen, die kleiner zu sein scheinen als die Ladung des einwertigen Wasserstoffions oder Elektrons und von dessen Vielfachen abweichen," *Physikalische Zeitschrift*, 11 (1910), 940–952.

sible 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.

While the velocity of light is now known with a precision of one part in twenty thousands [thanks largely to R. A. Millikan's patron and colleague at Chicago, Albert A. Michelson], the value of the elementary electrical charge has until very recently been exceedingly uncertain.<sup>6</sup>

Since Michael Faraday's time it was known that during electrolysis one gram-atomic weight of univalent material would be released at the electrode if about  $10^5$  coulombs of charge pass through the electrolyte. If one assumed that this quantity of charge was carried by  $N$  ions of charge  $e$  each (where  $N$  is Avogadro's number), then  $Ne \doteq 10^5$  coul. If  $e$  is now measured independently with accuracy,  $N$ , the number of atoms per gram-atomic weight of any substance, is also known with accuracy, and as a result many other fundamental physical constants may be calculated. At the beginning of the twentieth century,  $e$  was identified by many physicists with the magnitude of the charge of the electron. Poor values for  $e$  put into doubt the value of  $N$  and all that followed from it.

The controversy between Ehrenhaft and Millikan, often called "the battle over the electron," erupted in the spring of 1910. Only a year earlier Ehrenhaft had published measurements of the "elementary quantum of electricity" obtained by methods rather similar to Millikan's. But now in 1910 he suddenly announced his finding of electric charges much smaller than the charge on the electron. Millikan wrote later that Ehrenhaft's new claim "raises what may properly be called the most fundamental question of modern physics."<sup>7</sup> In a series of increasingly long and detailed articles, Ehrenhaft and his students claimed to find "subelectrons." That is, they found droplets of liquid, metal particles, and other small objects having charges with a value much smaller than that of the electron. In the course of time Ehrenhaft found charges a half, a fifth, a tenth, a hundredth, a thousandth that of the electron. As his work progressed there seemed

<sup>6</sup>R. A. Millikan, "A New Modification of the Cloud Method of Determining the Elementary Electrical Charge and the Most Probable Value of That Charge," *Philosophical Magazine*, 19 (1910), 209–228 (submitted 9 October 1909).

<sup>7</sup>R. A. Millikan, *The Electron: Its Isolation and Measurement and the Determination of Some of Its Properties* (Chicago, 1917), p. 158. In the revised edition of 1924, p. 161, the statement is repeated unmodified.

to be no reason to assume that Ehrenhaft would find any lower limit to exist for the electric charge associated with matter. On the other hand, only in the laboratories of the Vienna group were these results obtained. At the same time that Ehrenhaft was advancing his claims, Millikan and his students were assiduously refining and publishing evidence for the unitary electron.

The Millikan–Ehrenhaft controversy reverberated for years in the scientific community. Articles devoted to it increased in number. Max Planck, Jean Perrin, Albert Einstein, Arnold Sommerfeld, Max Born, and Erwin Schrödinger, among others, discussed it at scientific meetings. Periodically the evidence was reviewed in depth.<sup>8</sup> In 1927, three years after Millikan received the Nobel prize (in part for his work on the charge of the electron), the respected physicist O. D. Chwolson still called the fight a “delicate case”; and he added: “It has already lasted 17 years, and up to now it cannot be claimed that it has finally been decided in favor of one side or the other, i.e., that all researchers have adopted one or the other of the two possible solutions to this problem. The state of affairs is rather strange.”<sup>9</sup>

To appreciate the seriousness of Ehrenhaft’s claims today, we must guard against some ahistorical impressions. First, anyone familiar with the beautiful “Millikan Oil Drop Experiment,” now routinely assigned in elementary physics classes, may be inclined to dismiss contrary findings. Nevertheless, such pedagogical exercises are really designed to bolster belief in the electron, not to evaluate evidence for its existence. Even so, it is quite difficult to obtain good data during such experiments. One instructor recently said of his class experience: “In spite of the improvements in the Millikan oil-drop apparatus . . . the experiment remains perhaps the most frustrating of all the exercises in the undergraduate laboratory.”<sup>10</sup>

Second, the existence of a kind of subelectronic charge has been postulated in recent years in the quark model of elementary particle physics. In that model objects are assumed to exist having one third or two thirds the magnitude of the charge of an electron; but current theory and experiments concur that it is improbable for fractional

<sup>8</sup>For example, by R. Pohl, 1911; by R. Bär, 1922; and by O. D. Chwolson, 1927; in addition to the two protagonists themselves.

<sup>9</sup>O. D. Chwolson, *Die Physik, 1914–1926*, trans. Georg Kluge (Braunschweig, 1927), pp. 17–18.

<sup>10</sup>H. Kruglak, “Another Look at the Pasco–Millikan Oil Drop Apparatus,” *American Journal of Physics*, 40 (1972), 768–769. See also M. A. Heald, “Millikan Oil-Drop Experiment in the Introductory Laboratory,” *American Journal of Physics*, 42 (1974), 244–246; and J. L. Kapusta, “Best Measuring Time for a Millikan Oil Drop Experiment,” *American Journal of Physics*, 43 (1975), 799–800.

charges due to quarks to appear outside a nuclear particle, for example, on the droplets and similar objects that Ehrenhaft and Millikan were watching. In his paper of February 1910 Millikan did make a passing remark that Ehrenhaft later seized upon: “I have discarded one uncertain and unduplicated observation, apparently upon a singly charged drop, which gave a value of the charge on the drop some 30 percent lower than the final value of  $e$ .”<sup>11</sup> Millikan proposed that this one anomaly in his published data was a result of an error of measurement.<sup>12</sup>

Third, it would be a mistake to superimpose our present perception of the opposing points of view on the early phases of the controversy or to apply to this discussion criteria more appropriate in evaluating the final justification of a theory. It could not be known around 1910 that Millikan’s research on the electron would ultimately lead to results of great importance not only in physics but also in chemistry,

<sup>11</sup>Millikan, *op. cit.* (note 6), p. 220.

<sup>12</sup>*Ibid.*, p. 223: “The single observation mentioned above was probably on such a drop [a singly charged and very small drop of water or alcohol], but it was evaporating so rapidly that I obtained a poor value of  $e$ .” This explanation agrees with the opinion recently expressed by P. A. M. Dirac, who among contemporary physicists is probably the least unsympathetic to Ehrenhaft’s hope (although not sympathetic with his technique). In a letter of 11 October 1972, Professor Dirac has written to me: “It seems that Millikan’s anomalous drop was singly charged while all the others were doubly or triply charged. This puts Millikan in the same position as Ehrenhaft. So far as one can find from the published information, both Millikan and Ehrenhaft (in his more recent work) find an anomalous charge for all their smaller particles, and no anomalous charge for any of their larger particles. The conclusions are 1. There are no quarks. 2. There is some experimental error which makes all the smaller particles appear to have an anomalous charge. 3. By some unexplained coincidence, the anomalous charge is always about  $\frac{2}{3}e$ . I think this is the correct assessment of the historical information.”

In a letter to me of 4 December 1972, Professor Dirac added: “I just wanted to make the point that there is a similarity between Millikan and Ehrenhaft. They both found anomalous charges for their smallest particles, and in both cases their anomalous charge was about  $\frac{2}{3}e$ . One cannot suppose that quarks would just attach themselves to the smallest particles, so one must suppose there was a common error affecting their experiments and the factor  $\frac{2}{3}$  was a strange coincidence.” Professor Dirac has elaborated on these points in C. Weiner, ed., *History of Twentieth Century Physics* (New York, 1977), pp. 290–293.

Recently, Professor W. M. Fairbank, with A. F. Hebard and G. S. LaRue, has designed experiments attempting to detect the presence of free quarks, and hence of long-lived fractional charges ( $\frac{1}{2}$  and  $\frac{2}{3}e$ ) on small bodies such as superconducting niobium spheres of mass  $7 \times 10^{-8}$  grams. While in layout the experimental arrangement resembles Millikan’s in many ways, the physical effects being exploited are quite different. At this writing, the data are not yet conclusive; but it will be wise to have done the experiment. See Arthur F. Hebard, *Search for Fractional Charges Using Low Temperature Techniques* (diss., Stanford University, 1970), and A. F. Hebard and W. M. Fairbank, “Search for Fractional Charge (Quarks) Using a Low Temperature Technique,” *Proc. 12th International Conference on Low Temperature Physics*, ed. E. Kanda (Kyoto, 1971), pp. 855–857. For a recent review, see G. B. Lubkin, “Stanford Group Shows Apparent Evidence for Quarks,” *Physics Today*, 30 (1977), 17–20.

astronomy, and engineering. It could not be foreseen that Ehrenhaft's amazing results would give rise to nothing useful at all, unlike, for example, the experiments of Henri Becquerel which initiated the study of radioactivity with a misinterpretation of a critical experiment. The "risks" taken by Millikan and Ehrenhaft in their early work seem to differ greatly only in retrospect.

Fourth, the controversy was of special interest at the time because it addressed not only the nature of electric charges but also the behavior of the small particles that carried them. Recent improvements in microscopy, as well as the basic work of Einstein, Marian von Smoluchowski, and Perrin, had made more accessible what Wolfgang Ostwald called "the world of neglected dimensions." It was widely thought that research on the colloidal state (the dispersed state of matter where particle dimensions measure between  $10^{-4}$  and  $10^{-7}$  cm) was a great frontier for both pure and applied science, one that might bridge organic and inorganic matter. This field seemed filled with promise for medical-biological research as well as for industry.<sup>13</sup>

### 3. THE PROTAGONISTS

In their work Millikan and Ehrenhaft acted in response to one another and also within the accepted framework of public science (canonical knowledge, institutions for development of controversy or consensus, etc.) as it existed around 1910. Biographical details and some awareness of cultural and social contexts will therefore contribute to an understanding of their encounter.

Millikan was born in 1868 in Illinois and died in 1953 in Pasadena, California.<sup>14</sup> At the height of his career he was perhaps the most

<sup>13</sup>Felix Exner of the University of Vienna, apparently one of Ehrenhaft's early mentors, published observations on the size and motion of colloidal particles in 1900. After the introduction of the ultramicroscope and the theories of Brownian Movement, the colloidal state became a frontier for pure and applied science. In 1908 Wilhelm Ostwald added a chapter on colloid chemistry to the new edition of his influential textbook *Allgemeine Chemie*. His son Wolfgang, editor of the new *Zeitschrift für Chemie und Industrie der Colloide* from 1907, published two texts on colloid chemistry. Einstein thought it worth trying to reach readers outside pure physics by rendering his work on the Brownian Movement of small particles in two articles (1907, 1908) in the *Zeitschrift für Elektrochemie*.

<sup>14</sup>Biographies of Millikan include the *Biographical Memoirs* of the National Academy of Sciences by L. A. DuBridge and Paul S. Epstein, *op. cit.* (note 5); the article and extensive bibliography on Millikan by D. J. Kevles, *The Dictionary of Scientific Biography* (New York, 1974); D. J. Kevles, "Millikan: Spokesman for Science in the Twenties," *Engineering and Science* (California Institute of Technology, April 1969), pp. 17-22; H. V. Neher, "Millikan—Teacher and Friend," *American Journal of Physics*, 32 (1964), 868-877; and Millikan's *Autobiography* (New York, 1950).

renowned and influential scientist in the United States: physicist, administrator, educator, and policymaker. As he described them in his *Autobiography*, Millikan's origins were humble. Like many American scientists of his generation, he was the son of a small town minister. His parents Silas Franklin Millikan and Mary Jane Andrews brought up six children in a tradition free from pretensions. Robert's grandfather had been among the earliest farming settlers of the Mississippi River country in Western Illinois; it is said that in 1825 he walked alongside the covered wagon as the family was moving from the Berkshire Hills in the East to the frontier, the "Western Reserve." As a boy Millikan led a life recognizable in the stories of Mark Twain—steamboats on the Mississippi, family farm work in their one-acre yard, the swimming hole, the barefoot existence, the rural, simple, pragmatic, direct, and fundamentally pious background.

In 1886 Millikan went to Oberlin College where he registered for only one physics course, which he found "a total loss." He discovered his interest and aptitude in the subject only when a professor asked him to help teach physics. For graduate work he went to Columbia University and studied under Michael Pupin for two years as the only graduate student there in physics. Michelson, whom he met in Chicago in 1894, provided suggestions that helped Millikan plan his experimental thesis work. When he obtained a doctorate from Columbia in 1895, he could not find a satisfactory job. With a loan from Pupin, he went to Germany in May 1895 for additional study. It was the best moment to arrive. Within a few months the work of Wilhelm Conrad Röntgen came up like a storm; it was followed by that of Henri Becquerel, and the field of physics erupted in excitement. In 1896 Millikan accepted an invitation from Michelson to join the physics department at the University of Chicago, and Millikan's measurements of the charge of the electron began about a decade later in Chicago's Ryerson Laboratory. Eventually Millikan achieved a large and varied research output, first at Chicago and after 1921 while shaping and heading the California Institute of Technology. He listed nine fields of research in the second edition (1910) of *American Men of Science*, and twenty fields in the fifth edition (1933).

His scientific breadth is evident from the beginning in the archival material he left.<sup>15</sup> There is a revealing notebook, probably started in

<sup>15</sup>There are ninety-nine file boxes, well arranged and indexed by Alfred F. Gunns and Judith R. Goodstein with the assistance of Daniel J. Kevles with partial funding from the Center for the History of Physics of the American Institute of Physics. For a description and listings see A. F. Gunns and J. R. Goodstein, *Guide to the R. A. Millikan Collection at the California Institute of Technology* (New York, 1975), Publication no. R-269 of the American Institute of Physics.

1897 or 1898, entitled "References to Important Articles." Orderly entries list recently published articles in physics under such headings as "Zeeman Effect" (1897–1907), "Brownian Movement" (from 1905), and "Blondlot's N-Rays." These reading lists were probably connected at least in part with Millikan's teaching duties at Chicago. He writes in his *Autobiography*: "I soon found myself responsible for the weekly seminar in physics, which Professor Michelson asked me to take off his hands. . . . Furthermore, I soon began to give advanced courses on the electron theory, on the kinetic theory, and on thermodynamics. . . . [After 1900 Michelson was so absorbed in his research that] he asked me to assign research problems to three of the prospective candidates for the doctor's degree . . . and to take the whole responsibility for supervising their work, so that by 1902 and 1903 I had quite a group of problems going in addition to my own. . . ." <sup>16</sup> One set of pages in this notebook is entitled "Electron Theory of Matter," apparently compiled by adding entries from time to time over the years. It starts with "*m/e* Zeeman effect, *Phil. Mag.* 43, p. 226, 1897," and "Cathode Rays, J. J. Thomson, *Phil. Mag.* 44, p. 293, '97," and continues with significant articles over the following few years, including the early determination of *e* by Thomson, J. S. Townsend, and H. A. Wilson. Evidently, Millikan was keeping a careful eye on this work as it was developing.

The last page and inside cover of Millikan's notebook bear the title "Research Subjects"; the entries range from 1898 to 1914. The first of the twenty-seven entries, dated 21 May 1898, is "Resistance of air in its relation to the velocity of the (falling) moving body"—a major component of the problem that would be treated a decade later in Millikan's work on the charge of moving droplets. The ninth entry, probably made in 1903, reads "Stokes law for size of water particles in clouds, see J. J. T. articles on size of  $\epsilon$  and Barus, *Phil. Mag.* 4, p. 24/1902." Evidently, during the years prior to 1908 when Millikan was preoccupied with teaching and with his first investigations of the emission of electrons from metals by incident light and by high intensity electric fields, he was laying the conceptual bases for his later work on the electronic charge. <sup>17</sup>

<sup>16</sup>*Op. cit.* (note 14), pp. 58–60.

<sup>17</sup>The *Autobiography*, *ibid.*, p. 67, notes that Millikan was "just beginning to get some good leads in 1908" on the "evaluation of the charge of the electron." A remark in *The Electron*, *op. cit.* (note 7), pp. 54–55, puts the beginning date two years earlier: "In 1906, being dissatisfied with the variability of these results [published by H. A. Wilson in 1903 on *e* as determined by observations on falling clouds of charged drops], the author repeated Wilson's experiment without obtaining any greater consistency. . . . The results were not considered worth publishing."

We temporarily leave Millikan on the verge of starting his important work to turn to Ehrenhaft. He was born into a professional family in Vienna in 1879; his father was *Obermedizinalrat*, his mother was the niece of a student of J. B. L. Foucault. He studied at both the University and the Institute of Technology at Vienna. <sup>18</sup> In his earliest work, around 1900, he was one of the first to produce and study inorganic colloids. In 1903 he became assistant to Victor von Lang at the University of Vienna. Accepted as privatdocent in 1905, he was teaching statistical mechanics by 1909. Among his colleagues were Felix Exner, Friedrich Hasenöhr, Stefan Meyer, Egon von Schweidler, Karl Przibram, and Ernst Lechner. Ehrenhaft was appointed to an associate professorship at the University of Vienna in 1912 and became director of the third physical institute in 1920. He discovered photophoresis and named the effect in 1918. After the Nazis took over in Austria in 1938, he came to the United States as a refugee. He returned in 1946 to Vienna to resume his position, and he died there in 1952.

By 1909 he was already known for his experimental study of Brownian motion in gases, which he built on the theoretical ideas of Einstein and von Smoluchowski. For this work he received the Lieben Prize of the Vienna Academy of Sciences in 1910. He was a genial person whose house was always open to scientists from all corners of the world. According to Philipp Frank, <sup>19</sup> Einstein found Ehrenhaft congenial and would stay with him when passing through Vienna. From about 1920, and particularly after his claimed discovery of magnetic monopoles in the mid 1930s, Ehrenhaft's life centered on unresolvable controversies concerning the interpretation of complex physical phenomena. He made some thirty presentations on monopoles before skeptical audiences of the American Physical Society in the period 1940–1946. <sup>20</sup> When he is still remembered, it is usually in this context.

<sup>18</sup>According to an unpublished autobiographical account of his early years. Partly because of the forced flight from his homeland and the Second World War, many of Ehrenhaft's documents have disappeared; I am grateful to Dr. John Ehrenhaft for making available to me those documents that have survived. Some of Ehrenhaft's correspondence is in the Archives of the American Institute of Physics, The California Institute of Technology, and the Burndy Library, Norwalk, Connecticut.

<sup>19</sup>Philipp Frank, *Einstein: His Life and Times* (New York, 1947), p. 175. This book was translated by George Rosen and edited and revised by Shuichi Kusaka. It is always best to go back to Frank's original German version, published as *Einstein, sein Leben und seine Zeit* (Munich, 1949). This is the case here also (*q.v.*, p. 289).

<sup>20</sup>An unpublished report on Ehrenhaft's lectures in Austria in 1947 has been prepared by Paul K. Feyerabend, who was present as a student at the time ("Ehrenhaft in Post-War Vienna," mimeograph, 1967).

#### 4. ANTIATOMISM AND A FACULTY VACANCY

The European tradition of physics first influenced Millikan through his teacher Pupin, who had received his doctorate in Berlin. Millikan reports in his autobiography that Pupin's course on optics and electromagnetism was an eye-opener, and he came to admire and respect Pupin greatly. But Millikan was amazed by Pupin's attitude to atomism. Pupin had been impressed with the teachings of the schools of energetics and antiatomism, and he once told Millikan that he did not believe in the kinetic theory at all. Pupin was not alone in these doubts; the importance or truth of the atomic theory was still being argued in 1904, when it was a chief subject of debates at the scientific congress in St. Louis.<sup>21</sup> The first Solvay Congress, late in 1911, was largely concerned with fundamental, persisting impasses in a physics based on the atomic hypothesis, and a few critics such as Pierre Duhem scoffed at the hypothesis as late as 1913.

Many students absorb the epistemology of their honored teacher. In the case of Millikan, nothing of the sort happened. Let us recall what Millikan resisted, despite Pupin's example. Wilhelm Ostwald, Ernst Mach, J. B. Stallo, Georg Helm, and others around the turn of the century hoped to erect science on a purely phenomenological base, without "unnecessary hypotheses" like atomism, one of their frequently given examples. Despite triumphs of atomic theory such as J. C. Maxwell's proof of the independence of the viscosity of gases from density, there really was little direct evidence from phenomena for the reality of atoms and molecules, that is, for the necessity of discreteness. Scientists would not see particle tracks in cloud chamber photographs until around 1912.<sup>22</sup> Without the Geiger counter they did not perceive the persuasive individual flashes and clicks triggered by individual atomic events. They were working with average values, not individual atomic entities.

One of the best short descriptions of the antiatomistic school of thought, of which Mach was the most powerful proponent, is given in a biography of Mach written by the physicist Anton Lampa. Because Lampa, too, will soon enter this story, I shall use his account. Lampa points out that Mach's research interests were in very widely scattered specialties and that Mach sought one unifying position that

<sup>21</sup>This topic has been explored by Stephen Brush, Gerd Buchdahl, Erwin Hiebert, Laurens Laudan, Wilson Scott, W. H. Brock, and David Knight, among others. See particularly Mary Jo Nye, *Molecular Reality* (New York, 1972).

<sup>22</sup>*Autobiography, op. cit.* (note 14), p. 99. Millikan reports: "most of us had viewed [them] for the first time with amazement and thrill at the Dundee meeting [1912]."

he could adopt for any research. He found that basis in the world of elementary sensations which precedes the world of scientific construction:

In trying to find a point of view which required no change when Mach went from [problems in] physics to physiology and psychology, he started from a natural world picture which everyone, without conscious effort, finds within oneself upon one's intellectual awakening. Mach analysed that natural world picture. The result of the analysis is his Theory of Elements. The physical findings can be resolved into elements that hitherto are not further resolvable: colors, sounds, pressures, warmth, spaces, times, etc. These elements turn out to be dependent upon circumstances both outside the spatial limits of one's own body and within those limits. Insofar, and only insofar, as the latter is the case, we call these elements also experience (impressions, *Empfindungen*). The physical and the psychological [world] thus contains shared elements. . . . The natural world picture designates as corporeal objects relatively durable element-complexes of colors, sounds, heat, pressure, etc. . . .

The complex of all elements forms the world. . . . The pseudo-problems arise with the formulation of the conception of substance (matter, soul); such problems can be solved only if one analyses the complexes and goes back to the elements.<sup>23</sup>

One consequence of this position relates to what was called "atomistics." In looking for the ideal of a phenomenological physics, Mach refused to give the atom a fundamental basis in physics but instead asked it to be considered at most as a heuristic device for research. Under proper conditions and safeguards, he would tolerate a far more daring, speculative use of atomistic ideas than customary, for he proposed using more than three dimensions to represent the structure of molecules. However, making atomic entities the subject of research, whether in physics, chemistry, or electricity, was considered by the Machists a false and even a dangerous metaphysical hypothesis.

<sup>23</sup>Anton Lampa, *Ernst Mach* (Prague, 1918), pp. 40-41; see also p. 28. As in all cases where foreign language originals are cited, the translation is my own. Another source on the same point is a later essay by the positivist philosopher of science Moritz Schlick, presented in June 1926 on the occasion of the tenth anniversary of Mach's death; see M. Schlick, "Ernst Mach, der Philosoph," *Neue Freie Presse* (Supplement), Vienna, 12 June 1926. I have translated a portion of Schlick's essay on p. 222 of *Thematic Origins of Scientific Thought: Kepler to Einstein* (Cambridge, 1973), as part of an essay on the relationship between Mach and Einstein.



The liberation of science from all metaphysical bonds was Mach's lifelong ambition. Hence he acted not only as a productive physicist and influential philosopher, but also as a powerful figure in the politics of academic life.<sup>24</sup> He kept in touch with his students and followers and saw to it that his point of view would be represented in journals and on faculties. As it happened, the year 1910, when the Millikan-Ehrenhaft dispute first arose, brought also the culmination of the widely observed epistemological battle involving Mach. In fact, the same volume of the *Physikalische Zeitschrift* that carried Ehrenhaft's first detailed account of his discovery of subelectrons also contained the heated, polemical, often *ad hominem* articles that were being exchanged between Mach and Planck.<sup>25</sup>

A second event in 1910 also added to the sense of urgency felt in Mach's circle: a vacancy became available in the physics faculty of the German University in Prague, where Mach himself had been active for nearly three decades. Two members of the faculty there, Lampa and Georg Pick, began at once the search for proper candidates. Pick formerly had been an assistant of Mach; Lampa had been a disciple of Mach, then an assistant to von Lang at the University of Vienna, and from 1904 he taught there until his move to Prague in 1909. Lampa was a physicist as well as an idealistic fighter for the reform of education. As Frank, later his colleague in Prague, put it: "Lampa saw it as his life's chief goal to propagate Mach's views and to find adherents for them."<sup>26</sup>

Lampa and Pick looked for a man who could be relied upon to carry on physics in accord with Mach's views. A chief candidate was Gustav Jaumann of Brno. To obtain Mach's approval, Lampa wrote to Mach in a letter of 9 February 1910: "I need not reassure you that Jaumann's high talent seems to me beyond doubt and that his whole cast of thought is sympathetic. I consider the ideal of theoretical

<sup>24</sup>A good indication can be found in J. T. Blackmore, *Ernst Mach, His Life, Work, and Influence* (Berkeley, 1972), chs. 13 and 19.

<sup>25</sup>Ernst Mach, "Die Leitgedanken meiner naturwissenschaftlichen Erkenntnislehre und ihre Aufnahme durch die Zeitgenossen," *Physikalische Zeitschrift*, 11 (1910), 599-606; Max Planck, "Zur Machschen Theorie der physikalischen Erkenntnis," *Physikalische Zeitschrift*, 11 (1910), 1186-1190. The Mach-Planck controversy has been analyzed by J. Blackmore, *op. cit.* (note 24), ch. 14. For an English translation of the Planck and Mach essays together with an analysis, see S. Toulmin, ed., *Physical Reality* (New York, 1970).

<sup>26</sup>Frank, *Einstein, sein Leben*, *op. cit.* (note 19), p. 135. When Lampa died in January 1938, Ehrenhaft stressed in an obituary that Lampa had just been about to deliver an address commemorating the hundredth anniversary of Mach's birth. Ehrenhaft, "Anton Lampa," *Neue Freie Presse*, Vienna, 29 January 1938, p. 6. On Lampa see also Andreas Kleinert, "Anton Lampa and Albert Einstein," *Gesnerus*, 32 (1975), 284-292.

physics to be the purely phenomenological presentation (*Darstellung*), as lies at hand, for example, in thermodynamics. Jaumann proceeds from the wish to build up such a phenomenological presentation for electricity and all that can be connected with it. He therefore rejects the theory of atoms and of electrons. . . ." Lampa ended by sharing some worries about Jaumann and by announcing his visit to Mach in Vienna "in a few weeks." Evidently, Mach sent his approval speedily, for in a letter of 18 February 1910 Lampa thanked Mach for the reply, stating that all qualms were laid to rest and that he would intervene warmly on behalf of Jaumann.<sup>27</sup> Yet the selection process went on for many months more. Another candidate was Einstein, then at the University of Zürich, who was still regarded by the Machists to be of their persuasion.<sup>28</sup> He was just then corresponding with Mach and, indeed, signed one of his letters, "Ihr Sie verehrender Schüler."<sup>29</sup> Einstein was finally called to the chair in Prague in March 1911.<sup>30</sup>

## 5. SEEING ELECTRONS

Let us leave these Europeans for a time to their philosophies and academic negotiations and turn to Millikan, who was unaware of these events or of their future implications. Around him was a very different atmosphere. Millikan confesses to an unsophisticated, pragmatic, straightforward point of view of his own, one element of which is seeking direct explanation in terms of concrete visualization. The words "concrete visualization" recur in his writings, possibly to counter the charge that he engages in making up hypotheses. When Millikan writes about the electron in his early years, he does not of course think of a particle that has magnetic moment, angular momentum, wavelength, intrinsic self-energy, or any of the properties that we now think of as being associated with and defining the electron. He thinks of the electron as a discrete corpuscle of unitary

<sup>27</sup>Other aspects of Mach's influence on the selection of the candidate by Lampa and Pick are discussed in J. T. Blackmore, *op. cit.* (note 24), ch. 17. The Lampa-Mach correspondence from which I quote is located in the Ernst Mach Archive, Freiburg, Germany.

<sup>28</sup>Lampa writes to Mach on 1 May 1910: "I believe that relativity theory is the opening of a phenomenological epoch of physics" (Mach Archives, Freiburg, Germany). See also G. Holton, "Mach, Einstein and the Search for Reality," *op. cit.* (note 23), pp. 219-259.

<sup>29</sup>*Ibid.*, p. 227. The letter is dated 17 August 1909.

<sup>30</sup>When Einstein left Prague a year later for Zürich, his successor was Frank, whose candidacy was again supported by Mach, Lampa, and Pick, as well as by Einstein.

electric charge, whose action one can see with one's own eyes.<sup>31</sup> Indeed, he is not far from asserting that one can see the electron itself: "He who has seen that experiment," he writes in his autobiography about the oil drop experiment, "and hundreds of investigators have observed it, [has] in effect SEEN the electron." And again, with even less qualification: "But the electron itself, which man has measured . . . is neither an uncertainty nor a hypothesis. IT IS A NEW EXPERIMENTAL FACT that this generation in which we live has for the first time seen, but which anyone who wills may henceforth see."<sup>32</sup>

Because the autobiography was published when Millikan was over eighty, it may invite a suspicion about the reliability of some statements. There are passages where this is a valid concern. However, the autobiography is really a patchwork of new and old writings. One can gather by inspecting the materials in the Millikan archives that the publication probably was assembled with the aid of an editor under Millikan's supervision.<sup>33</sup> Large portions of the published book are repetitions of earlier publications. This is the case with the preceding passages, which come directly from Millikan's Nobel prize acceptance speech in 1924.<sup>34</sup>

Other passages in his documents and publications elaborate the anthropomorphic metaphor that Millikan adopted to deal directly with the experimental situation. He writes, for example, that when the small oil droplet was "moving upward [in the electric field, against gravitational pull] with the smallest speed that it could take on, I could be certain that just one isolated electron was sitting on its back. The whole apparatus then represented a device for catching and essentially seeing an individual electron riding on a drop of oil."<sup>35</sup> Sometimes, while watching a charged oil droplet held in the electric

<sup>31</sup>In his formal writings, Millikan does not always make it clear how strongly he opposes the earlier convention that the term "electron" should be reserved for the quantity of charge, regardless of the mass and other properties of the particle carrying the charge. He does state in his essay "New Proofs of the Kinetic Theory of Matter and the Atomic Theory of Electricity," *The Popular Scientific Monthly*, 80 (1912), 417-440, that his electron has charge, mass, and a discrete, small volume ("probably the smallest thing in existence," p. 434). An early draft of a manuscript in the Millikan Archives, California Institute of Technology, File Folder 4.11, carrying the notation "Probably 1921 or prior. H. H. 1/27/54," is almost certainly a draft of at least part of that 1912 paper; it was very probably written in late 1911 or early 1912.

<sup>32</sup>*Autobiography*, *op. cit.* (note 14), p. 80 and p. 82, respectively; emphases in original.

<sup>33</sup>Evidence is provided by the marginal notes on the various drafts of the *Autobiography*; see File Boxes 65-67.

<sup>34</sup>R. A. Millikan, "The Electron and the Light-Quant from the Experimental Point of View" [25 May 1924], *Nobel Lectures—Physics, 1922-1941* (Amsterdam, 1965), pp. 58-59. There is a slight difference between the two versions: none of the words are printed in capital letters in the Nobel lecture.

<sup>35</sup>*Autobiography*, *op. cit.* (note 14), p. 83.

field, he observed it change its motion suddenly, when the droplet encountered a charged molecule (ion) in the air. This observation was even more important; for the *discontinuity* in the observable phenomenon—new at the time—fitted splendidly with the hypothesized discontinuity in the concept of quantized charge. Here was a great new fact, and the image that helped interpret it was directly at hand: "One single electron jumped upon the drop. Indeed, we could actually see the exact instant at which it jumped on or off."<sup>36</sup> Earlier documents contain the same metaphor; in an early draft of his autobiography, Millikan wrote that he "could actually see the exact instant at which [the electron] jumped on or off."<sup>37</sup> He also provides other visual images; for example, "I had seen a balanced drop suddenly catch an ion."<sup>38</sup> Millikan had the same power of visualization as other distinguished scientists.<sup>39</sup> Thus in his brief essay on Ernest Rutherford, Millikan quoted with approval what he called "a very characteristic Rutherfordian remark": "Ions are jolly little beggars, you can almost see them."<sup>40</sup>

At about the time Millikan began to "see" his electrons, Perrin in France was battling for the atomicity of matter with the same strength of preconception and consequent focusing of vision which characterized Millikan's determination to demonstrate the atomicity of electric charge. Mary Jo Nye writes of Jean Perrin: "Perrin's primary goal from the very beginning of his scientific career was to prove the reality of the invisible atom, to eliminate as 'puerile anthropomorphism' those strictures which seemed logically necessary to many others. . . . One student wrote of Perrin . . . 'He "sees" atoms—there is no doubt at all—as Saint Thomas saw seraphim'."<sup>41</sup>

<sup>36</sup>*Ibid.*

<sup>37</sup>In File Box 67, particularly 67.3 and 67.4; but see also Boxes 65, 66, and 68. Folder 67.3 starts with "Scientific Recollections of R. A. Millikan. Personal Recollection of R. A. M. on Rise of American Science." On the file folder is a note that this is a first draft of the *Autobiography*, written on board ship while Millikan was traveling to India in 1939 (Millikan Archives, California Institute of Technology).

<sup>38</sup>Still earlier writings support the same point of view. For example, "Saw it here . . . pick up two negatives" (Millikan, "The Isolation of an Ion, a Precision Measurement of Its Charge, and the Correction of Stokes' Law," *Science*, 32 n.s. [1910], p. 439); also see his article in *Popular Scientific Monthly*, *op. cit.* (note 31).

<sup>39</sup>The ideas of Rom Harré on the role of iconic models and the relation between visual and conceptual thinking would seem useful here if one wished to carry the analysis further. See the discussion in E. MacKinnon, "A Reinterpretation of Harré's Copernican Revolution," *Philosophy of Science*, 42 (1975), 67-97.

<sup>40</sup>"Lord Rutherford of Nelson," *The American Philosophical Society Year Book*, 1938 (Philadelphia, 1939), p. 387.

<sup>41</sup>Nye, *op. cit.* (note 21), p. 65. I have analyzed the role of visualization in the "Gedanken" experiments of Einstein, *op. cit.* (note 23), pp. 353-380.

The way that Millikan launched his research on the charge of the electron combines three related factors: his ability to view what is going on with fresh insight; his powers of visualization in drawing conclusions; and most important, almost unconfessed and certainly unanalyzed, a preconceived theory about electricity.

## 6. ON THE ROAD TO THE ELECTRONIC CHARGE: METHOD I

Millikan describes frankly the series of accidents that set him on his way. At one of the weekly seminars in physics at Chicago, he presented a review of J. J. Thomson's great paper of 1897 on cathode rays. Millikan later wrote: "[It] put together in matchless manner, the evidence for the view that the 'cathode rays' consist not of ether waves, as Lenard and the Germans were maintaining, but rather of material particles carrying electric charges, each particle possessing a mass of about a thousandth of that of the lightest known atom and therefore constituting the most minute known masses in existence. He called these particles 'corpuscles'. . . . [This paper] impressed me greatly and started me on the researches which have been my life work."<sup>42</sup> However, for the next ten years Millikan's researches did not go well. Up to 1907 he had published only an article on his thesis of 1895, two short notes in 1897 and one in 1906, a translation of Paul Drude's *Optics*, and five introductory textbooks. In 1907 he published with George Winchester two articles on photoelectricity which received some notice.<sup>43</sup> In his autobiography Millikan hints that he was rather dissatisfied with himself at that point. He uses such phrases as "this apparently fruitless work"<sup>44</sup> and "my own research failures"<sup>45</sup> in describing his research. He may well have been concerned about his chances as a research scientist. In 1908, for some reason that one wishes to know more about, he "kissed textbook writing good-bye . . . and [while aware of the risk of further failures] started intensively into the new problem"—the magnitude of the elementary charge  $e$ .<sup>46</sup>

There were four obvious merits in Millikan's choice of this particular subject. One was that "everyone was interested in the magnitude

<sup>42</sup>*Autobiography, op. cit.* (note 14), pp. 58–59.

<sup>43</sup>J. S. Townsend, *Electricity in Gases* (Oxford, 1915), pp. 52–53, calls Millikan's results "interesting" and "the most reliable." On the other hand, when Townsend discusses Millikan's well-developed oil drop method, he treats it merely as one of the improvements of the art, as part of chapter 7, "The Formation of Clouds and the Determination of the Atomic Charge."

<sup>44</sup>*Autobiography, op. cit.* (note 14), p. 63.

<sup>45</sup>*Ibid.*, p. 69.

<sup>46</sup>*Ibid.*

of the charge of the electron," then known only to low accuracy and with results that varied widely depending on the method used.<sup>47</sup> Another merit was that the best experimental method seemed quite obvious and rather simple to Millikan, although later this turned out not to be the case. Third, measuring a basic constant with greater accuracy (rather than looking for daring new things in physics as Ehrenhaft was to do) was quite in keeping with Millikan's talents and temperament and with the tradition set by Michelson. Fourth, the theoretical basis or epistemological assumptions needed for the work seemed quite clear to Millikan: "Being quite certain that the problem of the value of the electric charge (Franklin's fundamental atom of electricity—apparently invariant and indivisible—the assumed unit building block of the electrical universe) was of fundamental importance, I started into it."<sup>48</sup>

Not for him all the turmoil and bitter debate raging in Europe concerning the "reality" of molecules, atoms, and electrons, or the admissibility of discrete rather than continuous entities! The electronic charge existed and it was of "fundamental importance" to find the value of the charge. If Millikan needed philosophical underpinnings for his work, he found them, appropriately enough, in the work of the great American folk hero, statesman, and scientist, the sensible Benjamin Franklin.

Millikan consistently refers to Franklin as the first to formulate a granular structure and material reality for the "electrical particle or atom,"<sup>49</sup> and he frequently quotes a sentence he ascribed to Franklin: "The electrical matter consists of particles extremely subtle, since it can permeate common matter, even the densest, with such freedom and ease as not to receive any appreciable resistance."<sup>50</sup> Franklin is

<sup>47</sup>*Ibid.*

<sup>48</sup>*Ibid.*, p. 72.

<sup>49</sup>*The Electron, op. cit.* (note 7), p. 15. Millikan was always interested in the history of science, and a useful review of "Early Views of Electricity" forms the first chapter of his book.

<sup>50</sup>Apparently first quoted by Millikan in the draft of his 1912 article, *op. cit.* (note 31), and again often, for example, in *The Electron, op. cit.* (note 7), p. 15; in the Nobel prize talk, *op. cit.* (note 34), p. 54; and in his *Autobiography, op. cit.* (note 14), pp. 69–70. Franklin's actual wording is slightly different: "The electrical matter consists of particles extremely subtle, since it can permeate common matter, even the densest metals, with such ease and freedom as not to receive any perceptible resistance." The sentence is at the beginning of an essay, "Opinions and Conjectures, Concerning the Properties and Effects of the Electrical Matter, Arising from Experiments and Observations, Made at Philadelphia, 1749." See I. B. Cohen, *Benjamin Franklin's Experiments* (Cambridge, 1941), pp. 212–213. After the publication of Cohen's book, Millikan referred to it and quoted Franklin's sentence correctly—except in his *Autobiography*. For sources of Franklin's atomism, see I. B. Cohen, *Franklin and Newton* (Philadelphia, 1956), chs. 6 through 10.

the father of the subject, "for there are no electrical theories of any kind which go back of our own Benjamin Franklin."<sup>51</sup> The result of all modern research has been merely "to bring us back very close to where Franklin was in 1750, with the single difference that our modern electron theory rests upon a mass of very direct and convincing evidence."<sup>52</sup> In 1948, looking back on the recent fiftieth anniversary celebration of Thomson's "unambiguous establishment of the electron theory of matter," Millikan remarked that since Franklin had started his experiments in 1747, one should have also been celebrating the bicentenary of "Franklin's discovery of the electron."<sup>53</sup> Even before Millikan turned seriously to his work on the charge on the electron, an account of Franklin's accomplishments (and his full-page portrait) could be found in some of the early school texts Millikan coauthored; a book published in 1908 describes Franklin's "so-called one-fluid theory," adding, "a modern modification . . . has recently come into prominence through . . . Lord Kelvin and J. J. Thomson," featuring "very minute negatively charged corpuscles, or electrons."<sup>54</sup>

Millikan was not the only one to see a connection between Franklin's ideas and the modern theory of electricity. Rutherford had pointed to it in an address in Philadelphia in 1906 at the bicentennial celebration of Franklin's birth,<sup>55</sup> and some years earlier Lord Kelvin had developed it in a paper that concentrated on Aepinus' elaboration of Franklin's theory.<sup>56</sup> Yet when Millikan began his work in the first decade of the new century, one did not have to accept the atomistic

<sup>51</sup>Millikan, *op. cit.* (note 31), and, with a few slight changes later, e.g., *The Electron, op. cit.* (note 7), p. 11.

<sup>52</sup>*The Electron, op. cit.* (note 7), p. 24.

<sup>53</sup>Millikan, "Franklin's Discovery of the Electron," *American Journal of Physics*, 16 (1948), 319.

<sup>54</sup>R. A. Millikan and John Mills, *A Short University Course in Electricity, Sound, and Light* (Boston, 1908), pp. 6-8. Having written this textbook, Millikan noted in his *Autobiography* (p. 69) that he then turned intensively to his work on *e*. In an earlier textbook, written with H. G. Gale, he had declared there was "much direct experimental evidence for the existence" of electrons (Millikan and Gale, *A First Course in Physics* [Boston, 1906], p. 244).

<sup>55</sup>E. Rutherford, "The Modern Theories of Electricity and Their Relation to the Franklinian Theory," in *The Record of the Celebration of the Two Hundredth Anniversary of the Birth of Benjamin Franklin* (Philadelphia, 1906), pp. 123-157, esp. p. 156. Millikan seems to have discovered this address rather late; his first reference appears to be in his obituary for Rutherford in 1938, *op. cit.* (note 40).

<sup>56</sup>Lord Kelvin, "Aepinus Atomized," *Philosophical Magazine*, 3 (1902), 257-283. Rutherford, *op. cit.* (note 55), cracked that its title should be changed to "Franklin and Aepinus Kelvinized." Kelvin writes: "My suggestion is that the Aepinus' fluid [of electricity] consists of exceedingly minute equal and similar atoms, which I call electrions [*sic*], much smaller than the atoms of ponderable matter . . ." (p. 257).

view of electricity let alone subscribe to a theory associated with Franklin. If Millikan had followed Pupin's example, he could have supported a rival theory of electricity, based on the thematic concept of the continuum rather than on the thematic concept of atomism. Maxwell's theory of electricity, while outwardly agnostic on the nature of electricity, permitted electricity to be more easily thought of in terms of continuous displacement, a motion within the electromagnetic ether, than in terms of an atomistic structure. Maxwell noted in 1873 in his *Electricity and Magnetism* that electrolysis seems to invite conceptualizing a definite value for an electric charge: "For convenience in description we may call this constant molecular charge (revealed by Faraday's experiments) one molecule of electricity." He added, however, that this convenient terminology, "gross as it is and out of harmony with the rest of this treatise," should not mislead us to ascribe reality to granules of electricity: "The theory of molecular charges may serve as a method by which we may remember a good many facts about electrolysis. It is extremely improbable, however, that when we come to understand the true nature of electrolysis we shall retain in any form the theory of molecular charges, for then we shall have obtained a secure basis on which to form a true theory of electric current and so become independent on these provisional hypotheses."<sup>57</sup>

Before the successes of the corpuscular view represented by the work of Pieter Zeeman, H. A. Lorentz, and Thomson, the view was widespread in Europe that the atomicity of electricity was only a heuristic device. Arthur Schuster wrote of the early 1880s: "The separate existence of a detached atom of electricity never occurred to me as possible, and if it had, and I had openly expressed such heterodox opinions, I should hardly have been considered a serious physicist, for the limits to allowable heterodoxy in science are soon reached."<sup>58</sup> Even in 1897 Lord Kelvin thought that careful consideration should be given to the idea that "electricity is a continuous homogeneous liquid."<sup>59</sup> Planck confessed that as late as 1900 he did not fully believe in the electron hypothesis.<sup>60</sup> Even where an atomistic hypothesis of

<sup>57</sup>James Clerk Maxwell, *Treatise on Electricity and Magnetism*, 1 (Oxford, 1873), 375ff.

<sup>58</sup>Arthur Schuster, *The Progress of Physics during 33 Years (1875-1908)* (Cambridge, 1911; New York, 1975), p. 59.

<sup>59</sup>Lord Kelvin, "Contact Electricity and Electrolysis According to Father Boscovich," *Nature*, 56 (1897), 84-85.

<sup>60</sup>There are numerous resources for the history of the theories of electricity. A good annotated guide is David L. Anderson, "Resource Letter (ECAN-1) on the Electronic Charge and Avogadro's Number," *American Journal of Physics*, 34 (1966), 1-7. The same author has published a useful introductory book, *The Discovery of the Electron* (Princeton,

electricity seemed persuasive, it did not necessarily follow that the electron had a unitary charge. Millikan later noted that the possibility of the electronic charge being merely a "statistical mean" was one that some physicists were supporting at the time.<sup>61</sup> According to all available documents, however, neither at the start of his work on the electron nor later did Millikan subject this possibility to any detailed test.

With hindsight it is easy to see evidence that should have convinced everyone of the particle theory of unitary electric charge, even prior to Millikan's work: Thomson's measurement of the constant charge to mass ratio of cathode rays; Rutherford's measurement of the charge on  $\alpha$  particles; the charge on cloud droplets of various liquids determined by Thomson, his student Townsend, and Wilson.<sup>62</sup> Yet even in the instances in which tolerable margins of error were obtained, the methods all shared the problem encountered with the calculation of the unit charge exchanged in electrolysis: they represented the determination of an *average* charge from observations made on many hypothetical individual charges at the same time. At best, these were indirect measurements of the charge  $e$ ; at worst,  $e$  would be the mean value of an unknown statistical distribution. Before Millikan no one had measured the charge of an individual object and found it to be equal to one, two, or any small multiples of a single unit of electricity. Certainly no one had watched a charged object changing its charge discontinuously by one, two, three, or more units of charge.

Millikan did not have the slightest hope of accomplishing such measurements when he set out to determine the value of the electronic charge. With his student L. Begeman, Millikan directly followed Wilson's method. Clouds of droplets were produced in an expansion cloud chamber between the parallel, horizontal plates of a charged condenser. Millikan and Begeman observed the slowly falling top layers of the clouds containing the smallest droplets. One set fell under gravity (at speed  $v_1$ ) and another set fell faster (at speed  $v_2$ ) with the additional aid of an electric field across the condenser. By

1964); chapter four has a good explanation of Millikan's method of measurement. Among the more recent articles see A. Pais, "The Early History of the Theory of the Electron, 1897-1947," in *Aspects of Quantum Theory*, eds. A. Salam and E. P. Wigner (Cambridge, 1972), pp. 79-93, and A. I. Miller, "A Study of Henri Poincaré's 'Sur la Dynamique de l'Électron'," *Archive for History of Exact Sciences*, 10 (1973), 207-328, sections 1-4.

<sup>61</sup>Autobiography, *op. cit.* (note 14), p. 75.

<sup>62</sup>See J. S. Townsend, *op. cit.* (note 43), ch. 7. Research on clouds condensed out on ions was still a young science; it was largely a by-product of the intense interest in ionization of gases, discovered by J. Elster and H. Geitel in 1894, and was vastly propelled by the discovery of the ionizing radiations, X rays, and radioactivity.

assuming that Stokes' law holds for the droplets, that each of the droplets is formed on a singly charged ion and does not shrink noticeably as a result of evaporation, and that the successive clouds are all formed in the same way, one could quickly obtain the charge  $q$  of the hypothetical unit of electricity in terms of the observables: speeds of fall  $v_1$  and  $v_2$ , electric field strength  $E$ , density of drop  $\delta$ , and viscosity of the gas  $\mu$ .<sup>63</sup> The average charge per droplet would thus be given by

$$q = \frac{4}{3} \pi \left( \frac{9\mu}{2g} \right)^{3/2} \frac{g}{E\delta^{1/2}} (v_2 - v_1) v_1^{1/2}.$$

This method (which I call Method I) contained unsatisfactory features, both theoretically and practically. Wilson's published measurements had shown a spread of values for  $e$  from  $2.0 \times 10^{-10}$  esu to  $4.4 \times 10^{-10}$  esu, with a mean of  $3.1 \times 10^{-10}$  esu. Earlier in 1903 Thomson had obtained  $3.4 \times 10^{-10}$  esu by a similar method. However, Millikan's plan in 1907-1908 was to improve accuracy by making only minor changes in the procedure. Thus, Millikan and Begeman used radium instead of X rays to ionize the moist gas prior to the expansion that formed the cloud. Their results for ten sets of observations for  $e$  spread from  $3.66 \times 10^{-10}$  esu to  $4.37 \times 10^{-10}$  esu, with the mean given as  $4.03 \times 10^{-10}$  esu. This value was evidently an improvement over Wilson's results—although it still invoked the implicit assumption that the results were not due to a statistical distribution of divergent values of electric charges occurring in nature.

## 7. THE ACCIDENTAL DISCOVERY OF AN EXPERIMENT: METHOD II

The joint paper by Millikan and Begeman was read at the American Physical Society meeting in Chicago early in January 1908. A one-

<sup>63</sup>Millikan's determination of  $e$  from the observation of charged clouds (which I call his Method I) was based on the following: J. J. Thomson, "On the Charge of Electricity Carried by a Gaseous Ion," *Philosophical Magazine*, 5 (1903), 346-355; J. J. Thomson, "On the Masses of the Ions in Gases at Low Pressures," *ibid.*, 48 (1899), 547-567; and H. A. Wilson, "Determination of the Charge on the Ions Produced in Air by Röntgen Rays," *ibid.*, 5 (1903), 429-440.

First one observes a layer of cloud droplets, each presumed to be of mass  $m$ , radius  $a$ , and density  $\delta$ , falling at speed  $v_1$  under its weight  $mg$ . Then one observes a similarly formed layer of cloud droplets, falling at  $v_2$  in a superposed electric field  $E$ , which now acts on the charge  $q$  of each droplet. Hence  $mg/(mg + Eq) = v_1/v_2$ . Assuming Stokes' law holds without modification for the droplets falling in air (viscosity  $\mu$ ),  $F_{\text{netton}} = 6\pi\mu av_1 = mg$  (in equilibrium)  $= (4/3)\pi a^3 \delta g$ . Therefore,  $v_1 = (2/9) \cdot (ga^2\delta/\mu)$ , or  $a^3 = (9v_1\mu/2g\delta)^{3/2}$ . Solving for  $q$  yields  $q = (mg/E) \cdot [(v_2 - v_1)/v_1] = (4/3)\pi \cdot (9\mu/2g)^{3/2} \cdot (g/E\delta^{1/2}) \cdot (v_2 - v_1)v_1^{1/2}$ . Assuming  $q$  is an integral multiple of the unitary charge  $e$ ,  $e = q/n$ , where  $n = 1, 2, 3, \dots$

page abstract was published that February.<sup>64</sup> Almost immediately prominent attention was drawn to this maiden effort by none other than Rutherford.<sup>65</sup> Millikan's result had come just in time to help Rutherford and Hans Geiger in their major new work at Manchester. They had determined the magnitude of the  $\alpha$  particle's charge as  $9.3 \times 10^{-10}$  esu and had assumed that it should be equal to  $|2e|$ . Hence  $e$  should be  $4.65 \times 10^{-10}$  esu. The values for  $e$  found earlier by Thomson and by Wilson had been thirty percent lower, but the new ones by Millikan and Begeman were only fifteen percent lower. While the work of Millikan and Begeman appeared to be the best of the three, Rutherford implied that it, too, could be brought closer to his value. Rutherford suggested that a failure to allow adequately for the evaporation of the droplets could cause the estimate of the number of ions (droplets) present to be too large and hence the value of  $e$  too small. Pending improvements of the methods of others, Rutherford's Manchester group continued to use his value  $e = 4.65 \times 10^{-10}$  esu with confidence until Bohr's early work of 1912; it "had been gospel at Manchester since the measurement of Rutherford and Geiger in 1908."<sup>66</sup>

With the incentive of Rutherford's suggestion, if indeed it was needed, Millikan's strategy was now clear: the error owing to evaporation had to be eliminated.<sup>67</sup> Millikan planned to work in his typically gradualist way by arranging the electric field to hold the top surface of the charged cloud suspended to permit studying its rate of evaporation. The work was apparently done in the spring and summer of 1909 and seemed at first to require only small modifications of existing technique, chiefly the use of an exceptionally large battery of

<sup>64</sup>R. A. Millikan and L. Begeman, "On the Charge Carried by the Negative Ion of an Ionized Gas," *Physical Review*, 26 (1908), 197-198. Despite the title, "the charge" was an average of the charges on the droplet in the top layer of the falling cloud. Since this paper was delivered around the beginning of January 1908 and published in February 1908, the work cannot have been performed during the summer of 1908, as Millikan states in *The Electron*, *op. cit.* (note 7), p. 55.

<sup>65</sup>E. Rutherford and H. Geiger, "The Charge and the Nature of the  $\alpha$ -Particle," *Proc. Royal Society (London)*, 81 (1908), 168-171. Rutherford slightly misquoted Millikan's mean value as  $4.06 \times 10^{-10}$  instead of  $4.03 \times 10^{-10}$ —and everyone then followed Rutherford's example, including Millikan himself, *The Electron*, *op. cit.* (note 7), p. 55. Millikan and Begeman's paper was also mentioned in what turned out to be the last flower of the Thomson-Townsend-Wilson cloud method, an article by R. F. Lattey, "The Ionization of Electrolytic Oxygen," *Philosophical Magazine*, 18 (1909), 26-31. Lattey reported that "Millikan's" value was "not so accurate" when compared with Rutherford's and Lattey's own.

<sup>66</sup>John L. Heilbron and Thomas S. Kuhn, "The Genesis of the Bohr Atom," *Hist. Stud. Phys. Sci.*, 1 (1969), 211-290, on 251.

<sup>67</sup>See *The Electron*, *op. cit.* (note 7), p. 55, and *Autobiography*, *op. cit.* (note 14), p. 72.

opening up a new experimental field. Then serendipity struck Millikan a second time:

I chanced to observe... on several occasions on which I had failed to screen off the rays from the radium [for ionizing the air before producing the cloud] that now and then one of [the balanced drops] would suddenly change its charge and begin to move up or down in the field... This opened the possibility of measuring [later] with certainty, not merely the charges on individual droplets as I had been doing, but the charge carried by a single atmospheric ion by comparing the speeds in the electric field on one drop before and after it chanced to catch an ion.<sup>72</sup>

Watching a water droplet suspended in a field made it easier to allow for evaporation and so increased the precision of measurement.<sup>73</sup> This situation provided a direct response to Rutherford's challenge. The rest of Millikan's work would soon proceed fairly naturally, from the replacement of water by a liquid with much lower vapor pressure to the long labors of removing or narrowing the sources of uncertainty, for example, by modifying Stokes' law for small droplets. Even in the first months, in the summer of 1909, Millikan claims that the "charges actually always came out, easily within the limits of error of my stop-watch measurements, 1, 2, 3, 4, or some other exact multiple of the smallest charge on a [water] droplet that I ever obtained. Here, then, was the first definite, sharp, unambiguous proof that electricity was definitely unitary in structure..."<sup>74</sup>

The importance of the discovery should not divert attention from the fact that Millikan did not design or devise the experiment from which his early fame sprang; rather, he discovered the experiment.<sup>75</sup>

<sup>72</sup>*The Electron*, *op. cit.* (note 7), p. 63.

<sup>73</sup>As he described it later, one starts with a drop that is slightly too heavy to be held by the electric field and which therefore falls. But before it is out of the view field, evaporation causes it to become light enough to stop falling, and finally it rises as it becomes too light. In the middle there is therefore a period of some ten to fifteen seconds during which the droplet appears to be essentially stationary. See Millikan, *op. cit.* (note 6), pp. 217-218. Millikan also discussed such continuing sources of error in this method as the short time of fall (five or six seconds at most) for determining  $v_1$ .

<sup>74</sup>*Autobiography*, *op. cit.* (note 14), p. 74; emphasis in original. Similar confidence is expressed in *The Electron*, *op. cit.* (note 7), p. 70: "In no case have I ever found one [ion] the charge of which, when tested as above, did not have either exactly the value of the smallest charge ever captured or else a very small multiple of that value." See also Millikan in *Science*, *op. cit.* (note 38), p. 440. Strictly speaking, his experiments showed not that the elementary charge of electricity itself had to be atomic, but only (as he was aware) that the transfer of charges to and from small material bodies occurred in integral multiples of  $e$ .

<sup>75</sup>The discovery of an experiment has apparently not been treated in the philosophy of science. There are evident differences between the discovery and the design of experiments, but also at least this similarity: both design and discovery generally occur

10,000 volts to set up a stronger electric field, now in opposition to the effect of gravity.<sup>68</sup>

When Millikan turned on the electric field, he chanced upon a sequence of accidents which he described consistently and frankly in the resulting publication of 1910, in the 1939 draft autobiography, and in the published autobiography. As he put it, the accident "*made it possible for the first time to make all the measurements on one and the same individual droplet, and . . . made it possible to examine the attracting or repelling properties of an individual isolated electron. . . .*"<sup>69</sup> The event allowed him to mobilize his immense energy, his skill as an observer and researcher, his ability to use students, his instinct for recognizing important and basic problems, and his great eye for the accident that opens an unsuspected door.

When he turned on the switch, the cloud, far from being held stationary, dissipated instantaneously and completely. The strong field cleared out the variously (not, as had always been assumed, equally) charged droplets; as a result there was no top surface of the cloud available for measurements. Indeed, the decade-long technique of measuring  $e$  by cloud watching came to an abrupt end. I have found no evidence that anyone used it again. Millikan wrote that the dispersal "seemed at first to spoil my experiment. But when I repeated the test, I saw at once that I had something before me of much more importance than the top surface. . . . For repeated tests showed that whenever a cloud was thus dispersed by my powerful field, *a few individual droplets would remain in view,*"<sup>70</sup> those with just the right charge to balance their weight in the electric field.<sup>71</sup>

It was the first time that one of the cloud experimenters concentrated on the individual charged droplet instead of the whole cloud. Millikan had stumbled on a new instrument, a very sensitive balance for holding in view an object of the order of  $10^{-13}$  to  $10^{-15}$  grams. It marked the change from Method I (falling cloud of water droplets) to Method II (balanced droplets of water, later also of alcohol). While that was only an intermediate stage before he arrived at Method III (nonsuspended oil drops), his perception had guided him to a tool for

<sup>68</sup>*The Electron*, *op. cit.* (note 7), p. 56: "The first determination which was made upon the charges carried by individual droplets was carried out in the Spring of 1909." *Autobiography*, *op. cit.* (note 14), p. 75: "I finished the foregoing measurements just prior to . . . September 1909." See also R. A. Millikan, "The Existence of a Subelectron?" *Physical Review*, 8 (1916), 596.

<sup>69</sup>*Autobiography*, *op. cit.* (note 14), p. 72; italics in original.

<sup>70</sup>*Ibid.*, p. 73; italics in original.

<sup>71</sup>The method for calculating  $e$  was now as follows: in the "balanced droplet" method, which I call Millikan's Method II, the equation for  $q$  of note 63 applies, except  $v_2$  is now zero.

The character of this discovery differs somewhat from that of, say, the discovery of Uranus or America. No one had doubted the existence of individual droplets. Anyone could have put together existing equipment a decade or more earlier if one had only thought of watching a drop instead of a cloud. The equipment used by Millikan in 1909 was a simple structure made of available materials; even making a big enough battery had ceased being a major challenge long ago. It is not altogether clear why Thomson, Townsend, and Wilson, among many others, did not think of determining the electric charge in the first place by abandoning the rather complex cloud experiments in favor of the far simpler method of individual droplets. The stranglehold on the imagination applied by the tradition of work on clouds appears to have yielded only to Millikan's accident.

## 8. EHRENHAFT IN 1909—THE PATHS CONVERGE

Millikan's first reports on individual droplets were not published until December 1909 in *The Physical Review* and in February 1910 in the *Philosophical Magazine*. Before analyzing the papers or describing the setting in which the scientific community first heard Millikan's oral report of his discovery in August 1909, I turn to the work that Ehrenhaft, quite independently, was doing at about the same time—for the trajectories of the scientific work of the two protagonists are about to intersect. Millikan, starting from his cloud work, had been led to determine charge from observations of a single object by following the line of research techniques developed in England and the United States. Meanwhile, Ehrenhaft had been progressing toward the same determination by techniques more characteristic of work on the Continent, namely, the preparation of colloids and the ultramicroscopic Brownian Movement observations of individual fragments of metal such as those from the vapor of a silver arc and of cigarette smoke. In this period Ehrenhaft's atomistic preference was as clear and as explicit as Millikan's; for example, he ended one of his papers of 1907 with the hope that the work would be "a new support for the molecular-kinetic hypothesis."<sup>76</sup>

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within the framework of a more or less explicit, prior problem. Thus, we can accept Ernest Nagel's description of this experiment if we change the word "devised" to "discovered": "It is unlikely that Millikan (or anyone else) would have devised the oil-drop experiment if some atomistic theory of electricity had not first suggested a question that seemed important in the light of the theory and that the experiment was intended to settle." E. Nagel, *The Structure of Science* (New York, 1961), p. 90.

<sup>76</sup>Ehrenhaft's early progress can be followed through these selected articles:

1. "Kolloidale Metalle," *Anzeiger Akad. Wiss. (Vienna)*, 18 (1902), 241-243.

Ehrenhaft's first report on a new method to measure the charge on small particles to determine what he called the "elektrische Elementarquantum" was dated 4 March 1909 and appeared as a one-page summary in the *Anzeiger* of the Academy of Sciences of Vienna.<sup>77</sup> He explained there how he had noted that colloidal metal particles occasionally showed an electric charge, as indicated by their motion in a horizontal electric field. (It is plausible that he happened upon this effect during earlier studies on Brownian Movement.) By measuring the motions of particles with and without an electric field and applying Stokes' law to obtain their mass, he could measure the charges on the particles. He was doing very nearly what Millikan did, but unlike Millikan he did not use a vertical electric field. Two weeks later, in a longer report,<sup>78</sup> Ehrenhaft announced that he intended to employ a vertical field; however, the results were delayed for a year. In 1909 his method suffered from obvious shortcomings. Different sets of particles were needed. One set was observed when moving with a horizontal component in the presence of the electric field, the other when moving vertically without the electric field. Following this procedure,  $e$ , therefore, cannot be the charge determined on a single object but must be an average. Nevertheless, Ehrenhaft's three papers submit-

2. "Das optische Verhalten der Metallkolloide und deren Teilchengröße, *Sitzungsber. Akad. Wiss. Math.-Naturw. Kl. (Vienna)*, IIa, 112 (1903), 181-209; also *Annalen der Physik*, 4 (1903), 489-514.

3. "Die diffuse Zerstreuung des Lichtes an kleinen Kugeln," *Anzeiger Akad. Wiss. (Vienna)* (1905), pp. 213-214; also *Sitzungsber. Akad. Wiss., Math.-Naturw. Kl. (Vienna)*, 114 (1905), 1115-1141.

4. "Die Brownsche Molekularbewegung in Gasen," *Anzeiger Akad. Wiss. (Vienna)*, 5 (1907), 72-73.

5. "Über eine der Brownschen Molekularbewegung . . .," *Sitzungsber. Akad. Wiss., Math.-Naturw. Kl. (Vienna)*, 116 (1907), 1139-1149, dated 11 July 1907.

6. "Über kolloidales Quecksilber," *Anzeiger Akad. Wiss. (Vienna)*, 25 (1908), 513-514.

7. "Eine Methode zur Messung der elektrischen Ladung kleiner Teilchen zur Bestimmung des elektrischen Elementarquantums," *ibid.*, 7 (1909), 72, dated 4 March 1909.

8. "Eine Methode zur Bestimmung des elektrischen Elementarquantums, I," *Sitzungsber. Akad. Wiss., Math.-Naturw. Kl. (Vienna)*, 118 (1909), 321-330, dated 18 March 1909, nominally appeared 1 May 1909.

9. "Eine Methode zur Bestimmung des elektrischen Elementarquantums, I," *Physikalische Zeitschrift*, 10 (1909), 308-310; paper received 10 April 1909, nominally appeared 1 May 1909. (The next publication was a year later—see note 108 below.)

It is unlikely that Millikan had easy access to these papers, partly because publication may have been delayed. Millikan hinted at this once in passing, *op. cit.* (note 68), p. 598: referring to a paper in the *Sitzungsber. Akad. Wiss., Math.-Naturw. Kl. (Vienna)*, dated 12 May 1910, he notes that "this publication does not seem to have appeared till December 1910, at least it is not noted in 'Naturae Novitates' before that date." Notices in the *Anzeiger* of the *Sitzungsber. Akad. Wiss., Math.-Naturw. Kl. (Vienna)* volumes bear out Millikan's observation concerning such delays.

<sup>77</sup>Paper no. 7 (note 76).

<sup>78</sup>Paper no. 8 (note 76).

ted between 4 March 1909 and 10 April 1909 are the first in the literature in which the paths and motions of individual, charged particles were followed and used to compute a value of  $e$ .<sup>79</sup> Moreover, the value for  $e$  obtained by Ehrenhaft,  $4.6 \times 10^{-10}$  esu, is far closer to Rutherford's  $4.65 \times 10^{-10}$  and Planck's  $4.69 \times 10^{-10}$  (from blackbody radiation) than that of Millikan and Begeman from 1908, and Ehrenhaft did not hide this fact.<sup>80</sup> In view of the subsequent debate it is ironic that Ehrenhaft here notes that Millikan's values from 1908 were lower than Rutherford's, that Millikan's effort "yielded values of the elementary quantum that are too small."

## 9. AT THE WINNIPEG MEETING, AUGUST 1909

In attempting to gain acceptance for one's work, early communication in an excellent forum is a great advantage. Here again Millikan was extraordinarily lucky. He finished the measurement of  $e$  by the balanced waterdrop method in the late summer of 1909, just before the British Association for the Advancement of Science was to hold its seventy-ninth meeting in Winnipeg, Manitoba, in Canada. He was too late to appear on the printed program,<sup>81</sup> and to the last moment Millikan did not know if he would be called on to present his results during the sessions.<sup>82</sup> It must have been a heady meeting for this

<sup>79</sup>Papers no. 7, 8, and 9 (note 76).

<sup>80</sup>Paper no. 8 (note 76), p. 330; no. 9, p. 310. The whole volume 118 of the *Sitzungsberichte* is a good indicator of the type and intensity of research in Austria circa 1909 and of the intellectual relations between individual researchers involved in this case. Frank writes an excellent pedagogic exposition of relativity, based on Einstein and Minkowski. Lampa writes on the effect of colloidal gold suspensions on light, drawing heavily on Hasenöhl (1902) and Ehrenhaft (1903). Prziabram, in a paper immediately following Ehrenhaft's, studies the mobility of ions of a great number of vapors and parenthetically adopts the value  $e = 4.65 \times 10^{-10}$  esu.

<sup>81</sup>Or even into the *Report* of the meeting, published by BAAS in 1910.

<sup>82</sup>A letter from R. A. Millikan to his wife Greta conveys something of the meeting and of the man (Millikan Archive, California Institute of Technology):

Monday, August 30, 1909

Dearest Greta

It is now 5.30 PM and I have been attending meetings all day. Had a fine letter waiting for me here when I came from the meeting hall to the official post office of the association. Crew had asked me to take dinner with him tonight at six o'clock at the Hotel Royal Alexandra so I shall have but a minute to tell you how much I love you, for the hotel is at least 15 minutes from here by the St. car. If we get through with our sessions I shall leave here tomorrow at 5.30 and be home Wednesday at 10.30 PM. It is barely possible however that I shall not be able to get away before the next night. This is staying away longer than I thought to do isn't it honey, but I am finding this meeting quite profitable. I haven't presented my results as yet and don't know that I shall do so, but if they want them tomorrow I



relative newcomer to scientific research. The Presidential Address was given by Thomson, who chiefly devoted his time to discussing three research frontiers: the structure of electricity ("We know that negative electricity is made up of units all of which are of the same kind. . . ."); the ether ("The ether is not a fantastic creation of the speculative philosopher; it is as essential to us as the air we breathe. . . . The study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist"); and radioactivity. Among the physicists and astronomers listed as reading papers at the large meeting were C. V. Boys, A. S. Eddington, A. S. Eve, E. Goldstein, Otto Hahn, W. J. S. Lockyear, Oliver Lodge, Percival Lowell, A. E. H. Love, Theodore Lyman, D. C. Miller, J. H. Poynting, Lord Rayleigh, Rutherford, Schuster, and G. J. Stoney.<sup>83</sup>

The ground for Millikan's presentation was prepared not only by the attention directed to the electron problem by Thomson, but also by Rutherford's address on 26 August as President of the Section on Mathematical and Physical Science.<sup>84</sup> Rutherford attempted in the address to summarize how recent progress in physics strengthened the credibility of the atomic theories of matter and of electricity. As he had also done in 1906, Rutherford wasted little time opposing directly the antiatomists who were still active on the Continent.<sup>85</sup> Rather, he made short thrusts: "The negation of the atomic theory has not and does not help us make discoveries," he proclaimed; casting doubt on the atomic theory "is quite erroneous"; and in the very last sentence of the paper he announced that "in the light of these and similar direct deductions, based on a minimum amount of assumption, the physicists have, I think, some justification for their faith that they are building on the solid rock of fact, and not, as we are often so solemnly warned by some of our scientific brethren, on the shifting sands of imaginative hypothesis."<sup>86</sup>

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am ready to give them. I made some lantern slides this AM so that they can be presented in short time. Goodnight honey I'll be back soon. Kiss the kiddies, and take a dozen of them for yourself.

Your own  
Robert

<sup>83</sup>Report of the Seventy-Ninth Meeting of the British Association for the Advancement of Science, Winnipeg, 1909 (London, 1910).

<sup>84</sup>*Ibid.*, pp. 374-385. Rutherford's article was quickly republished in the *Physikalische Zeitschrift*, 10 (1909), 762-771, under the title "Die neuesten Fortschritte der Atomistik." Millikan was probably in the audience to hear the paper, and he refers to it in his descriptions of the meeting; *op. cit.* (note 7), p. 75.

<sup>85</sup>Rutherford, *op. cit.* (note 55).

<sup>86</sup>Rutherford, *op. cit.* (note 84), pp. 375, 381, and 385 (italics supplied).

Rutherford's attention was devoted mainly to reviewing the developments favorable to the atomistic point of view, which, he acknowledged, appealed particularly "to the Anglo-Saxon temperament." On the list of findings he reported were efforts by three Austrians: Exner and Richard Zsigmondy's determination of mean velocity of particles in various solutions from Brownian Movement calculations, and Ehrenhaft's 1907 experimental determination of Brownian Movement by observing small particles suspended in gases. Rutherford's recent work with Geiger on the charge of  $\alpha$  particles was further support, "showing that this radiation is, as the other evidence indicated, discontinuous, and that it is possible to select by a special electric method the passage of a single  $\alpha$  particle. . . ."<sup>87</sup>

As in the paper he had written with Geiger during the previous year, Rutherford cited the work of Thomson, Townsend, Millikan, and Begeman on clouds (Millikan's report on using individual water droplets had not yet been delivered) as another indication that "electricity, like matter, is supposed to be discrete in structure." He added: "This method is of great interest and importance," although the exact determination of  $e$  in this manner was "beset with great experimental difficulties."<sup>88</sup> Rutherford lauded recent work by Ehrenhaft in 1909 on the charge carried by ultramicroscopic dust particles of metal and grouped Ehrenhaft's value for  $e$  with Rutherford's and Geiger's as one of "the most recent measurements by very different methods which are far more reliable than the older estimates."<sup>89</sup> The implication was that these values were more reliable than those of Thomson, Townsend, and Wilson. It now is no longer reasonable, he concluded, "to believe that such concordance [in the experimental values of  $e$  and  $N$ , based on different theories] would show itself if the atoms and their charges had no real existence"; hence doubts concerning the atomic theory of matter are "quite erroneous."<sup>90</sup>

Rutherford did voice one regret: "It has not yet been possible to detect a single electron by its electrical or optical effect, and thus count the number directly, as in the case of  $\alpha$ -particles." This was precisely the missing link, and while he could not have known that Millikan had already found that link, Rutherford was optimistic: "There seems to be no reason why this should not be accomplished by the electrical method." Rutherford evidently had in mind the pos-

<sup>87</sup>*Ibid.*, p. 376.

<sup>88</sup>*Ibid.*, pp. 379, 380.

<sup>89</sup>*Ibid.*, p. 381.

<sup>90</sup>*Ibid.*

sibility of using scintillations produced by  $\beta$  rays for this purpose. But Millikan was on hand at that very meeting, waiting to give his paper a few days later in which he would show just how one might go about detecting the single electron by another method, that is, by its effects on the observed motion of a small droplet of liquid.

At last, five days after Rutherford's address, Millikan's turn to speak came, and apparently he was well received. Millikan later singled out Joseph Larmor as having been "intensely interested in my paper"<sup>91</sup> and as having suggested that Millikan should look into the limits of Stokes' law, promising to do the same himself from the theoretical side.<sup>92</sup> After this presentation on 31 August 1909, Millikan must have thought that the end of the quest to establish the unitary nature of the electric charge was in clear sight. The subject had been acknowledged at the highest level to be at the very frontier of urgent research; his earlier results, even before his recent improvements, had been believed and cited with respect; they clearly seemed to fit well with the other pieces in the jigsaw puzzle of physical theory; and he had been able to present his new method and new results just as soon as the need for them was announced by Rutherford. He later recalled that even his final, major improvement of technique, that of using oil drops to avoid all the problems caused by evaporation, occurred to him suddenly while he was riding the train back to Chicago from the Winnipeg meeting.<sup>93</sup>

## 10. MILLIKAN'S FIRST MAJOR PAPER, FEBRUARY 1910

We do not have a copy of the talk Millikan delivered at Winnipeg, but a few weeks later he published a very brief account of his work,<sup>94</sup> and on 9 October he sent the paper—his first major one—to the *Philosophical Magazine* for publication in February 1910.<sup>95</sup> These two

<sup>91</sup>*Autobiography, op. cit.* (note 14), p. 75.

<sup>92</sup>One of Larmor's students soon published a result: see E. Cunningham, *Proc. Royal Society (London)*, 83 (1910), 135. Millikan's new results are referred to twice.

<sup>93</sup>Millikan reports this in his *Autobiography, op. cit.* (note 14), p. 75. A rather different account of the origin of the idea of using oil or mercury instead of water and alcohol was later given by Millikan's student, Harvey Fletcher. See "Harvey Fletcher, Autobiographical Notes," correspondence with Fletcher, and a transcript running to sixty-nine pages of an interview conducted by Vern Knudsen, 15 May 1964, in the Center for History of Physics, American Institute of Physics, New York.

<sup>94</sup>R. A. Millikan, "A New Modification of the Cloud Method of Measuring the Elementary Electrical Charge, and the Most Probable Value of That Charge," *Physical Review*, 29 (1909), 560–561; abstract of a paper given on 23 October 1909 at the Princeton meeting of the American Physical Society.

<sup>95</sup>Millikan, *op. cit.* (note 6).

contributions are the first reports in the literature of the use of single, isolated drops and of the balancing field method (Method II).

If Millikan and his readers thought that the search for the value of  $e$  was essentially over with the publication of this paper, they soon found that the battle was just beginning. Indeed, two likely causes for the approaching "fight over the electron" can be found directly in Millikan's *Philosophical Magazine* paper. One has to do with the well-known role which feelings about priority claims play in unfolding scientific controversies. In the section giving "The Most Probable Value of the Elementary Electrical Charge," Millikan presents his new mean value  $e = 4.65 \times 10^{-10}$  esu and assigns also equal weight to "all the recent determinations of  $e$  by methods which seem least open to question."<sup>96</sup> These, he says, included the value obtained by Planck from radiation theory,  $4.69 \times 10^{-10}$ , which Rutherford had mentioned with favor at Winnipeg; the value of Rutherford and Geiger,  $4.65 \times 10^{-10}$ ; Erich Regener's value  $4.79 \times 10^{-10}$ , obtained by a method very similar to Rutherford's; and Begeman's "recent and as yet unpublished" value of  $4.67 \times 10^{-10}$ , obtained in Millikan's laboratory.<sup>97</sup> The final mean for  $e$ , Millikan declares, is thus  $4.69 \times 10^{-10}$  esu. Rutherford's objections of 1908 and 1909 have been well met. In addition, because Millikan's and Rutherford's results are so close, Millikan feels the "results seem to constitute experimental verification of Stokes' law for these drops."

But while accepting the work of these authors, Millikan specifically rejects the values for  $e$  published by four others, including Ehrenhaft, and gives explicit reasons. Perrin's value "involves [too] many assumptions of questionable rigor"; Maurice de Broglie's relies on Perrin's  $N$ , among other difficulties; Moreau's depends on Perrin's  $e$ ; and Ehrenhaft's results are "obtained by a method similar to the one here presented save that it involves the measurement of the velocities produced first by the action of gravity, and second by the action of an electrical field upon the charged particle thrown off by a metallic arc." The resulting "uncertainties" that he specifies make Ehrenhaft's values unacceptable. Still, Millikan acknowledges that Ehrenhaft's mean value for  $e$ ,  $4.6 \times 10^{-10}$ , is in "very good agreement" with the other, accepted values.<sup>98</sup>

<sup>96</sup>*Ibid.*, p. 227.

<sup>97</sup>*Ibid.*

<sup>98</sup>Ehrenhaft, paper no. 9 (note 76); also Millikan, *op. cit.* (note 6), p. 226. Millikan's objections were: (1) in Ehrenhaft's method, Stokes' law is applied without modification to very small particles of doubtful sphericity; (2) the important velocity measurements are not made on one and the same particle but are mean values of observations on particles having speeds that can differ widely; (3) the radii are determined in a dubious

Millikan's reservations regarding Ehrenhaft's measurements seem reasonable, at least in retrospect. In Ehrenhaft's view they may have seemed to verge on a calculated insult. Had Millikan accepted Ehrenhaft's value, it would have worked to Millikan's advantage in the sense that it would have brought the average value of all accepted determinations of  $e$  slightly down, toward Millikan's own. In fact, he was rejecting a confirmatory value, one obtained by an established researcher who had used a method closer to his own than the methods of others whom Millikan was not rejecting. Millikan's decision was grounded in his suspicions, plausible but far from proved, that the value obtained by Ehrenhaft was invalidated by the method used to obtain it. As we shall soon see, Millikan was also sensitive to the obverse, to the possibility that his own results of measurement were at times unwelcome and disconfirmatory, but that even without a solid analysis of the causes he could continue to accept his hypothesis and reject the apparent falsification of it.

It appears that from Ehrenhaft's point of view the glove had been thrown down before him. Beginning in his next publication, he and some of his students dedicated themselves to the "question of the elementary quantum of electricity." Ehrenhaft's own output of a dozen papers over the next four years was entirely on this subject, and all were implicitly aimed at discrediting Millikan's measurements. The battle over the electron had begun.

Ehrenhaft's rejection of Millikan's results, however, need not have been wholly motivated by his feeling of having suffered a professional slight. The fact was that Millikan's publication of February 1910 was vulnerable to criticism. With idiosyncratic frankness and detail, Millikan shows in the section "The Results" that the measurements with the new technique were still difficult to make, that he relied

way; (4) no provision is made for the possibility that multiple charges may be carried by some of the particles. As the controversy heated up, additional and even more serious reservations came into view, such as the difficulty of knowing the density of the metal particles and the role of Brownian Movement in making measurements difficult. See H. Fletcher, "A Verification of the Theory of Brownian Movements and a Direct Determination of the Value of  $N_e$  for Gaseous Ionization," *Physical Review*, 33 (1911), 107-110; R. A. Millikan and H. Fletcher, "Ursachen der scheinbaren Unstimmigkeiten zwischen neueren Arbeiten über  $e$ ," *Physikalische Zeitschrift*, 12 (1911), 161-163; R. A. Millikan, "The Isolation of an Ion, a Precision Measurement of Its Charge, and the Correction of Stokes' Law," *Physical Review*, 32 (1911), 392-396; and Millikan, *op. cit.* (note 68), pp. 595-625, largely repeated as ch. 8 of *The Electron*, *op. cit.* (note 7). Ehrenhaft vigorously rebutted these and all other critics in numerous and extensive publications; see, for example, "Über die Quanten der Elektrizität," *Sitzungsber. Akad. Wiss., Math.-Naturw. Kl. (Vienna)*, 123 (1914), 53-132. There were usually new results to report, even in his last lengthy paper on the subject, "The Microcoulomb Experiment," *Philosophy of Science*, 8 (1941), 403-457.

heavily on personal judgment, and that it was really still his first major paper. In the paper he records the most important raw data for five series of observations on balanced water drops and "to vary the conditions" for one series of those on balanced alcohol drops. The observer—Millikan or Begeman—is also identified; then, in a move rarely found in the scientific literature, each of the thirty-eight sets of observations is given a personal rating:

The observations marked with a triple star are those which were marked "best" in my notebook and represent those which were taken under what appeared to be perfect conditions. This means that we could watch the drop long enough to be very certain that it was altogether stationary: that we could time its passage across the cross-hairs with perfect precision, and that it showed no apparent retardation in falling through the two equal spaces. The double-starred observations were marked in my notebook "very good." Those marked with single stars were marked "good" and the others "fair."<sup>99</sup>

There were two "three star" observations, seven "two star" ones, ten "single star" ones, and thirteen without any star. The average value of  $e$  obtained in each of the seven series or sets of observations is then assigned a "weight" from one to seven to obtain the final, weighted grand average  $e = 4.85 \times 10^{-10}$  esu, as compared with the unweighted simple mean of  $4.70 \times 10^{-10}$ . Although we can discern a general relation between the total number of "stars" in a series and the weight given to the particular individual series average, the relation is not explained nor is it linear. Millikan was evidently saying he knew a good run when he saw one, and he was not going to overlook that knowledge even if it was not obvious how to quantify and share it on the record.

Equally significant was Millikan's frank admission that another seven observations had been discarded altogether and so had not entered at all into the computation of the final average value of  $e$ : "First, I discarded three very good observations of my own, taken under conditions of potential and position of cross-hairs which made them uncertain in spite of the accurate timing. These observations . . . would not affect appreciably the final result if they were included."<sup>100</sup> Only the internal ethos of science, which prizes the fullest disclosure of data, seems to have motivated him to mention this set of discarded observations. Millikan continues:

<sup>99</sup>Millikan, *op. cit.* (note 6), p. 220.

<sup>100</sup>*ibid.*

Second, I have discarded three observations which I took on unbalanced drops, timing them as they rose against gravity under the influence of the field, and then again as they fell under gravity between the same cross-hairs, when the field was thrown off. Although all of these observations gave values of  $e$  within 2 per cent. of the final mean, the uncertainties of the observations were such that *I would have discarded them had they not agreed with the results of the other observations*, and consequently I felt obliged to discard them as it was.<sup>101</sup>

This is an unusual statement. Nothing in the rest of his paper has prepared us to expect that, as Millikan mentions casually in passing, he would discard some observations if they “disagreed with the results of the other observations.” Was it enough that these three runs were on *unbalanced drops*, which became his method of choice immediately afterwards? His comment that, in this instance, the omission had no practical effect on the final results either way is reassuring, but one must not overlook the more general methodological point. Such judgments are not infrequent. They often can be (and in this case were) supported by appeals to plausibility which allow the experimenter to assert that he believes the discordant observations do not go to the heart of the matter, that is to say, are not grounded in a serious way in the phenomenon being studied. For just this reason such judgments expose the researcher to a risk, one that he is willing to take given his framework of beliefs and assumptions. This framework renders the judgments as plausible acts, acts that allow him to avoid the interruptions, delays, and detailed research that might be necessary to pin down the exact disturbing causes behind the discrepant observations.

Scientific judgments such as Millikan’s rest upon the belief that, in principle, a reinterpretation can be found that would fit the discarded observations into the pattern of nature signalled by the accepted ones. Conversely, such judgments also imply the belief that the alternative picture of nature that would have to be adopted if the discarded observations were accepted is so unlikely or abhorrent that it is not worth even the effort of falsifying it in detail. In Millikan’s case this alternative picture would portray charges exhibiting a stochastic distribution about the mean value  $e$  or charges made up of subelectrons or of some congealing of a continuum of charge. Millikan’s confidence was explicit. He announces: “there is no theoretical un-

<sup>101</sup>*Ibid.*

certainty whatever left in the method,” unless, he adds, “it be an uncertainty as to whether or not Stokes’ law applies to the rate of fall of these drops on the gravity.”<sup>102</sup> On the next page even this doubt is laid to rest: “It is scarcely conceivable that Stokes’ law fails to hold for them.”<sup>103</sup> As to the last of the neglected observations, Millikan says simply, without apology or plausibility, that “in the third place, I have discarded one uncertain and unduplicated observation apparently upon a single charged drop, which gave a value of the charge on the drop some 30 per cent. lower than the final value of  $e$ . With those exceptions all of the data recorded in our notebooks are given below.”<sup>104</sup>

The data, presented in six tables such as the one shown in Figure 1, also raise questions. Just how many individual droplets were used in these observations? Each of the six series of observations may have used as many droplets as there were observations. Why, then, were the raw data of voltages and times on all drops in each series pooled and averaged in order to obtain one preliminary average value of  $e$ , which in turn received a single weight for the final averaging to obtain one final value of  $e$ ? How confident can the reader be that one integer is the correct divisor for all data in a given series (for example,  $n = 3$  in Figure 1), and how were the data assembled to form a given series in the first place? Finally, since the chief point was to find  $e$ , what prevented Millikan from using a bigger section of the battery, or smaller spacing between the condenser plates, to obtain usable data on singly charged droplets—not to speak of testing, head-on, whether fractional charges do exist? Ehrenhaft may well have asked himself these questions.

Unfortunately, the notebooks containing the data of Millikan’s work of 1909 have not been found, and we cannot find there the answers to such questions nor use them to watch through the keyhole what occurred in the laboratory. But soon we shall have better luck, for Millikan continued his work with great energy. It was beginning to be seen as good scientific work by the criterion of fruitfulness. For example, his 1909 value for  $e$  was adopted in 1913 in Niels Bohr’s

<sup>102</sup>*Ibid.*, p. 219.

<sup>103</sup>*Ibid.*, p. 224. Millikan did continue to be concerned about this point and improved the Stokes’ law calculations over the next years, for example, in his next paper, *op. cit.* (note 38), and in his 1912 report at the B.A.A.S. meeting in Dundee. Here is evidently a point where one would like to have access to Millikan’s laboratory notebooks of 1909 with the data of his work of that period, for they might help us to see how his beliefs concerning the nature of electric charge aided him in deciding which observations were grounded in the nature of the phenomenon and which were not.

<sup>104</sup>*Ibid.*

SERIES No. 1 (Bal. pos. water drops).			
Distance between plates ·545 cm.			
Measured distance of fall ·155 cm.			
Volts.	Time. 1 space.	Time. 2 spaces.	Observer.
**2285	2·4 sec.	4·8 sec.	Millikan.
2285	2·4 sec.	4·8 sec.	"
**2275	2·4 sec.	4·8 sec.	Begeman.
***2325	2·4 sec.	4·8 sec.	Millikan.
2325	2·6 sec.	4·8 sec.	"
*2325	2·2 sec.	4·8 sec.	"
**2365	2·4 sec.	4·8 sec.	"
2312	2·4	4·8	
Mean time for ·155 cm. = 4·8 sec.			
$e_3 = 3.422 \times 10^{-9} \times \frac{980.3}{14.14} \times \left(\frac{.155}{4.8}\right)^3$ $= 13.77 \times 10^{-10}.$ $\therefore e = 13.85 \times 10^{-10} \div 3 = 4.59 \times 10^{-10}.$			

Figure 1.

epochal paper on the hydrogen atom,<sup>105</sup> and Millikan's observational method became directly applicable in the closely related efforts by his

<sup>105</sup>J. Heilbron and T. S. Kuhn, *op. cit.* (note 66), p. 266. In N. Bohr, "On the Constitution of Atoms and Molecules," Pt. II, *Philosophical Magazine*, 26 (1913), 5, Bohr abandoned Rutherford's and Geiger's 1908 value of  $e = 4.65 \times 10^{-10}$  esu and "adopts Millikan's value for  $e$  . . ." It was a factor in improving Bohr's calculation of Rydberg's constant, bringing it from within 7 percent of the spectroscopic value, as in his previous work, to within 1 percent. Bohr actually writes  $e = 4.7 \times 10^{-10}$  esu, without stating his source; but it is equivalent to Millikan's value of  $4.69 \times 10^{-10}$  esu, given in his 1909 *Physical Review* and 1910 *Philosophical Magazine* papers, *op. cit.* (notes 5 and 6, respectively). Millikan's value of  $e$  changed as he continued his work. In *Science* (1910), it

student H. Fletcher to obtain  $N$  and hence  $e$  from measurements of Brownian Movement.<sup>106</sup>

Between the fall of 1909 and the spring of 1910, Millikan's methods of experimentation and calculation underwent a process of significant maturation toward his Method III, to be examined below. By this method he would work not with balanced water drops but with falling and rising oil drops and, as well, calculate values of elementary charge from each set of observations on a given drop. Unknown to Ehrenhaft, Millikan reported on his new method first on 23 April 1910 to the American Physical Society.<sup>107</sup> In the meantime, however, Ehrenhaft had committed himself.

### 11. EHRENHAFT'S ATTACK ON $e$

It started with a note to the Vienna Academy session of 21 April 1910.<sup>108</sup> Ehrenhaft had been silent for a year, but now he had startling news. He used a horizontal condenser with a vertical electric field strong enough to make particles rise against gravitation—a deployment equivalent to the one Millikan reported shortly afterwards in the paper read at an American Physical Society meeting on 23 April 1910—and he studied platinum and silver particles from arcs. Ehrenhaft reported more than three hundred measurements which

became  $4.9016 \times 10^{-10}$ , "to less than 1/2%." By 1911 it was  $4.891 \times 10^{-10}$  esu, and in the grand *Physical Review* paper of 1913, it was  $4.774 \times 10^{-10}$  esu—a value that survives in the 1924 edition of his book *The Electron*, p. 120. Evidently Bohr knew which value to keep his eye on in 1912–1913.

<sup>106</sup>Cf. H. Fletcher, "Einige Beiträge zur Theorie der Brownschen Bewegung mit experimentellen Anwendungen," *Physikalische Zeitschrift*, 12 (1911), 202–208; Fletcher, *op. cit.* (note 98), pp. 81–110. Space limitations forbid going into this field here, a necessity made more palatable by the fact that Millikan seems not to have been deeply involved in Fletcher's research.

<sup>107</sup>A one-page abstract of Millikan's lecture of 23 April 1910 was published in July, "The Isolation of an Ion and a Precision Measurement of Its Charge," *Physical Review*, 31 (1910), 92. A lengthy abridgment of the paper was published on 30 September, "The Isolation of an Ion, a Precision Measurement of Its Charge and the Correction of Stokes' Law," *Science*, 32 (1910), 436–448; it was republished in December in German, "Das Isolieren eines Ions, eine genaue Messung der daran gebundenen Elektrizitätsmenge und die Korrektur des Stokesschen Gesetzes," *Physikalische Zeitschrift*, 11 (1910), 1097–1109, and in abridged form in French, "Obtention d'un ion isolé, mesure précise de sa charge; correction à la loi de Stokes," *Le Radium*, 7 (1910), 345–350. In the French paper Millikan writes that Fletcher and he "studied in this way between December [1909] and May [1910] from one to two hundred drops which had initial charges varying between the limits 1 to 150." Falling and rising oil drops are used; the values of  $e$  are now computed for each drop and for each run separately; the maximum electric field strength is now twice as large. But only one of the eleven drops for which actual data are given shows a unitary charge.

yielded a new and remarkable result: particles are not only singly or doubly charged but can also have charges "between and below" these values. The twenty-two measurements of charge reproduced in Ehrenhaft's paper range from  $7.53 \times 10^{-10}$  esu down to  $1.38 \times 10^{-10}$  esu—about one third of the value of the elementary charge he had previously measured. Ehrenhaft concluded that these findings cannot be explained away as inadequacies of method. Rather, these quantities are "in nature." If "theory presupposes the existence" of an indivisible quantum of electricity, the value of the latter will thus have to "fall considerably below" the hitherto accepted value. A counter challenge was thus issued to all believers in  $e$  as the quantum of charge for which nothing in theory or experiment seemed to have prepared the ground. Out of the blue, the subelectron had appeared on the stage.

Ehrenhaft followed his announcement of 21 April 1910<sup>109</sup> with a report delivered to the Vienna Academy on 12 May 1910, in which he coined the word subelectron and announced that his results indicated that indivisible quantities of electric charge do not exist in nature at the level of  $1 \times 10^{-10}$  esu or above.<sup>110</sup> His subelectrons do show a propensity for aggregating; for example, he reports the total charge on gold particles to have ranged continuously from  $5 \times 10^{-11}$  esu to a heaping up (*Häufung*) of  $1.75 \times 10^{-10}$  esu, that is, up to a third of the

<sup>108</sup>Felix Ehrenhaft, "Über die kleinsten messbaren Elektrizitätsmengen. Zweite vorläufige Mitteilung der Methode zur Bestimmung des elektrischen Elementarquantums," *Anzeiger Akad. Wiss. (Vienna)*, no. 10 (21 April 1910), pp. 118–119. A week later in the *Anzeiger*, no. 11 (28 April 1910), pp. 175–176, Ehrenhaft's younger colleague Przibram reports briefly and mostly qualitatively about having repeated Millikan's experiment with water droplets. He acknowledges obtaining much the same results; but he adds a final sentence: "The few smaller values observed so far, that is, those smaller than  $e$  [which he takes to be about  $3.5 \times 10^{-10}$  esu], appear, in view of the certainty of measurement, to support the conclusion of Ehrenhaft that the deviation from the mean values—from  $3 \times 10^{-10}$  downwards—is considerably larger than the experimental error" (p. 176).

<sup>109</sup>*Ibid.*

<sup>110</sup>Felix Ehrenhaft, "Über die Messung von Elektrizitätsmengen, die die Ladung des einwertigen Wasserstoffions oder Elektrons zu unterschreiten scheinen. Zweite vorläufige Mitteilung seiner Methode zur Bestimmung des elektrischen Elementarquantums," *Anzeiger Akad. Wiss. (Vienna)*, no. 13 (12 May 1910), p. 215. A full paper appeared in the 12 May 1910 issue of *Sitzungsber. Akad. Wiss., Math.-Naturw. Kl. (Vienna)*, 119 (1910), 815–866, under the title "Über die Messung von Elektrizitätsmengen, die kleiner zu sein scheinen als die Ladung des einwertigen Wasserstoffions oder Elektrons und von dessen Vielfachen abweichen." Nearly the same material appeared in "Über eine neue Methode zur Messung von Elektrizitätsmengen an Einzelteilchen, deren Ladungen die Ladung des Elektrons erheblich unterschreiten und auch von dessen Vielfachen abzuweichen scheinen," *Physikalische Zeitschrift*, 11 (1910), 619–630, received 23 May 1910. See *op. cit.* (note 76) for Millikan's opinion on the date of actual publication.

usual charge of the electron. Although all his observations are made on very small particles observed in the ultramicroscope, he sees no reason to abandon Stokes' law in the classical form, which would, in any case, make the true charge even smaller, or to worry about Brownian Movement, although it made time measurements uncertain. Moreover, he assumes again that the density of the metal fragments, developed in an electric arc, are of the same density as the mother material in the electrode.<sup>111</sup> More remarkable, however, is the conclusion that emerges ever more forcefully as the number and length of the articles by Ehrenhaft and his collaborators increase: these experiments do not permit them to "hold on to the fundamental hypothesis of the electron theory," namely, the indivisible electron.<sup>112</sup> The large spread of values for  $e$  which have been measured by various researchers and by different methods should be taken as a signal that one is dealing here with an aspect of natural law itself. These variations of net charge are "in nature."

If Millikan and others felt that Ehrenhaft's data could in principle be interpreted without giving up the undivided electron, they must have felt somewhat embarrassed when Ehrenhaft turned to Millikan's data in the 1910 paper in the *Philosophical Magazine* on balanced water and alcohol drops. He subjected the data to a devastating attack, turning them against Millikan.<sup>113</sup> Ehrenhaft recalculated the charge on each drop from each of Millikan's observations separately instead of following Millikan's method of lumping several runs to obtain values of  $e$  from the average of measured values of voltage, etc., measured on different droplets. The result was a large spread of values of droplet charge from  $8.60 \times 10^{-10}$  esu to  $29.82 \times 10^{-10}$  esu. The case for each of these being an integral multiple of one elementary charge now did not look at all self-evident (see Figure 2).<sup>114</sup> It appeared rather that the same observational record could be used to demonstrate the

<sup>111</sup>This assumption was probably off by an order of magnitude; see H. Fletcher, *op. cit.* (note 98), p. 108, and other arguments in the article.

<sup>112</sup>See Ehrenhaft, *Sitzungsber., op. cit.* (note 110), p. 866, and *Phys. Zs., op. cit.* (note 110), p. 630. See also Felix Ehrenhaft, "Über eine neue Methode zur Messung von Elektrizitätsmengen, die kleiner zu sein scheinen als die Ladung des einwertigen Wasserstoffions oder Elektrons und von dessen Vielfachen abweichen," *Physikalische Zeitschrift*, 11 (1910), 940–952 (where Ehrenhaft announces he will replace Stokes' law "entirely by empirical formulas"), and articles of Przibram beginning in 1910 in the *Anzeiger*, *Sitzungsberichte*, and *Physikalische Zeitschrift*.

<sup>113</sup>Ehrenhaft, *Sitzungsber., op. cit.* (note 110). Ehrenhaft showed that Millikan's method of treating data led to paradoxical situations. A drop with  $q = 15.59 \times 10^{-10}$  esu had been placed among those assumed to be carrying three electrons, while another with charge  $q = 15.33 \times 10^{-10}$  esu was among the drops assumed to be carrying four electrons.

<sup>114</sup>Figure from Ehrenhaft, *op. cit.* (note 110), Table I.

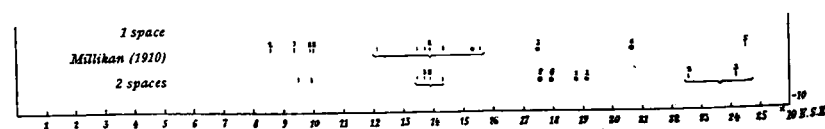


Figure 2.

plausibility of two diametrically opposite theories, held with great conviction by two well-equipped proponents and their respective collaborators. Initially, there was not even the convincing testimony of independent researchers.<sup>115</sup>

## 12. THE OIL DROP EXPERIMENT, 1910: METHOD III

Happily for Millikan, Ehrenhaft's attack in mid 1910 was quickly blunted by the timely publication of Millikan's new results obtained from his new method. Millikan himself extensively documents his adoption of and early success with the oil drop.<sup>116</sup> His account in the

<sup>115</sup>When such testimonies began to come in shortly afterwards, they tended to favor Millikan's view. E. Regener, "Über Ladungsbestimmungen an Nebelteilchen. (Zur Frage nach der Grösse des elektrischen Elementarquantums)," *Phys. Zs.*, 12 (1911), 135–141; R. Pohl, "Bericht über die Methoden zur Bestimmung des elektrischen Elementarquantums," *Jahrbuch der Radioaktivität und Elektronik*, 8 (1911), 406; A. Joffé, "Zu den Abhandlungen von F. Ehrenhaft: 'Über die Frage nach der atomistischen Konstitution der Elektrizität,'" *Physikalische Zeitschrift*, 12 (1911), 268; L. M. McKeehan, "Die Endgeschwindigkeit des Falles kleiner Kugeln in Luft bei verminderten Druck," *Physikalische Zeitschrift*, 12 (1911), 707–721. Major support for Millikan's view concerning the quantum of charge came at the Solvay Congress of 1911; see P. Langevin and M. de Broglie, eds., *La Théorie du Rayonnement et les Quanta* (Paris, 1912), esp. pp. 149, 150, 233–237, 251, 252. Emil Warburg, Heinrich Rubens, Wilhelm Wien, Einstein, and especially Perrin spoke favorably of Millikan's results. Perrin took these views to a large public in his book *Les Atomes* (Paris, 1913 and later editions). On the other hand, Ehrenhaft's supporters came chiefly from the circle of his students and collaborators in Vienna, e.g., D. Konstantinowsky, F. Zerner, G. Laski, I. Parankiewicz. Przi Bram (cf. note 108) was caught in a difficult position. A good researcher, he found his data used sometimes by one side, sometimes by the other. At the Solvay Congress in 1911 (Langevin and de Broglie, *op. cit.*, p. 252), Hasenöhrl reported that Przi Bram was not joining in Ehrenhaft's conclusion regarding  $e$ ; similarly, letters in the Caltech Archives from Przi Bram to Millikan (5 November 1912, 10 December 1912) show that Przi Bram was then prepared to accept Millikan's results and method. The letter of December ended: "It is very gratifying to me that my researches on the subject, commenced with such a grievous mistake, are at last in fair agreement with your standard work."

<sup>116</sup>Prior to publication, Millikan discussed his work on 24 May 1910 at a Sigma Xi meeting in Chicago. An article, "Substance of Address," which reads like a transcript of Millikan's remarks, was published by *The Daily Maroon* of the University of Chicago, 25 May 1910, under the heading "Millikan Makes Great Scientific Discovery. Associate Professor in Physics Department Succeeds in Isolating Individual Ion. Holds it Under Observation. Proves Truth of Kinetic Theory of Matter—Result of Four Years of Re-

second major paper in his career, in *Science* in September 1910, verged on the euphoric.<sup>117</sup> He could now measure separately the frictional charge on an oil drop as well as the additional charges it may pick up from ions in the atmosphere during its travel, and both types of charges were found to be quantized in the same manner, "exact multiples of one definite, elementary, electrical charge."<sup>118</sup> He boasted that he could "catch upon a minute droplet of oil and hold under observation for an indefinite length of time one single atmospheric ion or any number of such ions between 1 and 150." The method is free from "all questionable theoretical assumptions," and the limitation on the accuracy of determining  $e$  is only the accuracy with which the value of the viscosity of air ( $\mu$  or  $\eta$ ) is known. He found that Stokes' law breaks down for very small spheres and determined a correction. A view "advanced many years ago" was confirmed by these experiments, he claimed, namely that "an electrical charge, instead of being spread uniformly over a charged surface, has a definite granular structure, consisting, in fact, of an exact number of specks, or atoms, of electricity, all precisely alike, peppered over the surface of the charged body." Indeed, Millikan now held that "the conclusions follow so inevitably from the experimental data that even the man on the street can scarcely fail to understand the method or to appreciate the results."

Nor would the scientist be less impressed by the confidence expressed in the findings: Millikan reports that working with Fletcher from December 1909 to May 1910 on droplets of oil, mercury, and glycerin—on "one to two hundred drops" in all—they "found in every case the original charge on the drop [to be] an exact multiple of the smallest charge which we found that the drop caught from the air." Between one thousand and two thousand changes of charge were observed, yet "in not one single instance has there been any change which did not represent the advent upon the drop of one definite invariable quantity of electricity, or a very small exact multiple of that quantity."<sup>119</sup>

Of interest is Millikan's treatment of his data. His final value of  $e$  is the mean value of twenty-seven determinations of  $e$  on that many

search." Other newspapers also carried condensations of the talk. Millikan discussed his developing ideas on the oil drop method in "The Unit Charge in Gaseous Ionization," *Transactions of the American Electrochemical Society*, 18 (1910), 283–288. See also Millikan's unpublished lecture notes for summer 1910, in Folder 1.15, Millikan Archives, California Institute of Technology.

<sup>117</sup>Millikan, *op. cit.* (note 107).

<sup>118</sup>This and the next quotations are from Millikan's *Science* article, *op. cit.* (note 107), p. 436; italics supplied.

<sup>119</sup>*Ibid.*, p. 440; italics in original.

individual droplets, taken from a larger number "studied throughout a period of 47 consecutive days." Three other drops "have been excluded [because they] all yielded values of  $e$  from two to four per cent. too low" compared with the plotting of the values from the other drops. A "natural" hypothesis concerning these three drops is that each may have been "two drops stuck together." At any rate, he adds, "after eliminating dust we found not more than one drop in ten which was irregular." The context shows that by the word "irregular" Millikan means that the drop's unitary charge  $e$  deviated by as much as four percent from the curve plotting the other values.<sup>120</sup> Nevertheless, ten more drops, the four slowest and six fastest ones, studied during that period were also eliminated from the final averaging in the *Science* article of 1910 before Millikan obtained his "final mean value of  $e$ ." While these ten drops would not appreciably alter the final mean value, the probable error in each of the individual determinations is necessarily much higher than in the middle range of speeds.<sup>121</sup>

### 13. THE PUBLICATION OF 1913: DROP NO. 41

With this knowledge of Millikan's treatment of data in his published work, we can turn to the last and most mature of his major papers in the 1909–1913 period: his publication of August 1913 in the *Physical Review*: "On the Elementary Electrical Charge and the Avogadro Constant."<sup>122</sup> This is the most authoritative version of the oil drop experiment to that point, and while Millikan continued to make improvements for years, all the chief elements were now assembled: a new optical system, a chronoscope accurate to 0.001 sec., temperature control to 0.02°C, a more accurately calibrated voltmeter, a better value for  $\mu$ , and the ability to change the gas pressure in the viewing chamber over a wide range. As a result, he could announce that "the largest departure from the mean value found anywhere in the table

<sup>120</sup>In Millikan's more extensive report of the same work, *op. cit.* (note 98), he adds: "Before we eliminated dust [in the viewing chamber] we found many drops showing these lower values of  $e_1 \dots$ " (p. 376).

<sup>121</sup>In the 1911 version, *op. cit.* (note 98), Millikan omits only eight of the additional drops rather than ten, but with the same explanation (p. 382). See also Millikan and Fletcher, *op. cit.* (note 98), pp. 161–163.

<sup>122</sup>R. A. Millikan, "On the Elementary Electrical Charge and the Avogadro Constant," *Physical Review*, 2 (1913), 109–143, completed 2 June 1913. In *The Electron*, *op. cit.* (note 7), p. 106, he refers to the same results as having been "first presented before the Deutsche physikalische Gesellschaft in June 1912, and again before the British Association at Dundee, in September, 1912."

[of values of  $e$ , determined for fifty-eight droplets] amounts to 0.5 per cent., and the probable error in the final mean value computed in the usual way is 16 out of 61,000."<sup>123</sup>

We are now getting close to Medawar's keyhole, for Millikan is generous in describing his procedure in this publication. Millikan provides panels containing the critical raw observations together with sample calculations for sixteen of the many drops he had followed. A typical example is "Drop No. 41" in his Table XV. It is reproduced here in Figure 3. Millikan also gives in his Table XX "a complete summary of the results obtained on all of the 58 different drops upon

<sup>123</sup>Millikan, *Phys. Rev.*, *op. cit.* (note 122), p. 139. The uncertainty in  $e$  claimed at the end of the paper is "2 parts in 1000," with  $e = 4.774 \pm 0.009 \times 10^{-10}$  esu. A summary follows of the calculational path to  $e$ , using terms Millikan also employed.  $v_1$  is the constant speed of descent of the drop under the force of gravity ( $mg$ ) equilibrated by the viscous force given by the (unmodified) Stokes' law expression ( $6\pi\mu av_1$ ).  $v_2$  is the constant speed of ascent as the drop, with frictional charge ( $q$ ), rises in the electric field  $E$ . Therefore,

$$\frac{v_1}{v_2} = \frac{mg}{Eq - mg}, \text{ and } q = \frac{mg}{E} (v_1 + v_2)v_1^{-1} = \frac{(\frac{4}{3})\pi a^3 \delta}{E} g (v_1 + v_2)v_1^{-1}.$$

Replacing  $a^3$  from Stokes' law gives

$$q = \frac{4}{3} \pi \left( \frac{9\mu}{2g} \right)^{3/2} \delta^{-1/2} \cdot \frac{g}{E} (v_1 + v_2)v_1^{1/2}.$$

Thus if  $q$  is quantized,  $e_{\text{net}} = q/n$ , or

$$e_{\text{net}} \propto \frac{(v_1 + v_2)}{n}.$$

Similarly, the charge picked from ions between successive ascents is

$$e_{\text{ionic}} = \frac{q'_n - q_n}{n'} = \frac{mg}{E} \left( \frac{v'_2 - v_2}{n'} \right) \cdot v_1^{-1}, \text{ or } e_{\text{ionic}} \propto \left( \frac{v'_2 - v_2}{n'} \right).$$

To take into account the breakdown of Stokes' law for small spheres moving with small observed speeds  $v_1$ , assume

$$\frac{v_1}{(1 + Alla)} = \frac{2g a^2 \delta}{9 \mu}.$$

But since  $e \propto v_1^{3/2}$ , the "true" value of  $e$  is

$$e = \frac{e_1 (\text{as "observed"})}{(1 + Alla)^{3/2}}.$$

One then obtains  $e$  by plotting  $e_1^{2/3}$  versus  $lla$  for many runs and reading off the intercept, that is,  $e_1^{2/3}$  when  $lla = 0$ .



Figure 3. Drop No. 41, from Table XV, Millikan, "On the Elementary Electrical Charge and the Avagadro Constant," *Physical Review*, 2 (1913), 109-143.

$t_g$	$t_F$	$\frac{x}{t_F}$	$n'$	$\frac{x}{n'} \left( \frac{1}{t_F} - \frac{1}{t_F'} \right)$	$n$	$\frac{x}{n} \left( \frac{1}{t_g} + \frac{1}{t_F} \right)$	
24.016	42.188	.02369	1	.009380	8	.009336	$V_i = 5065$ $V_f = 5059$ $t = 23.05^\circ \text{C.}$ $p = 19.01 \text{ cm.}$ $v_1 = .04253$ $a = .0001816$ $l/a = .1394$ $e_1 = 6.097 \times 10^{-10}$
24.142	42.078						
24.130	42.098						
24.070	69.900						
24.000	203.200						
24.030	23.844						
24.046	30.606						
24.028	42.800						
23.968	42.944						
24.018	71.400						
23.770	30.652	.03259	2	.009295	6	.009277	
23.882							
<b>24.008</b>				<b>.009314</b>		<b>.009301</b>	

(1) (2) (3) (4) (5) (6) (7)

$V_i, V_f$ : Initial and final potential differences of battery

$t$ : Temperature

$p$ : Pressure in chamber

$v_1$ : (cm/sec), average speed of fall without electric field

$a$ : Calculated radius of oil drop

$l/a$ : Mean fall path  $\div$  radius (Misprint for 0.2073, as given later in Millikan's Table XX.)

$e_1$ : Calculated mean value of elementary charge, before Stokes' law correction (Misprint for  $6.110 \times 10^{-10}$ , as given later in Millikan's Table XX.)

- (1)  $t_g$  column = time (in seconds) of drop's fall under gravity through 10.21mm distance; 12 successive observations, and the mean value [ $t_g \propto 1/v_1$ ].
- (2)  $t_F$  column = time of rising when the (charged) drop is retrieved after its fall by applying the electric field [ $t_F \propto 1/v_2$ ].
- (3)  $1/t_F$  column = reciprocal of some of  $t_F$  observations, hence proportional to  $v_2$ .
- (4)  $n'$  column = change of charge (in units of  $e$ ) between successive ascents ( $t_F$  being followed by  $t_F'$ ), owing to the encounters of the drop with gas ions during the fall. Since  $e_{\text{ionic}} \propto (v_2 - v_1)/n'$ ,  $n'$  is given by  $(1/t_F' - 1/t_F)$ ; so in a given case  $n'$  is found by adopting that trial value—assumed to be a small integer—which assures that the product  $(1/n')(1/t_F' - 1/t_F)$  is constant throughout the experiment with that drop.
- (5) Column of values of  $e_{\text{ionic}}$  indicates uncorrected values of elementary charge on the gas ions encountered in 5 excursions.
- (6),(7) Columns showing number of elementary charges  $n$  on drop, initially owing to friction in preparation of drop (by "atomizer"). Since  $e_{\text{net}} \propto (v_1 + v_2)/n$ ,  $n$  is obtained similarly to (4) and (5), but now  $v_1$  is given by  $1/t_g$  and  $v_2$  by  $1/t_F$  for the ascent immediately after the descent measurement,  $t_g$ .

N.B. The chief point is the determination of  $e_{\text{ionic}}$  (col. 5) and  $e_{\text{net}}$  (col. 7) and the coincidence of the two values. They are then used together in Millikan's paper to obtain  $e_1$  and, after Stokes' law correction,  $e$ ; see also note 123.

which complete series of observations like the above [i.e., a table of data on one of the drops] were made during a period of 60 consecutive days." And again, after showing that no more than a single one of these fifty-eight drops gives results for  $e$  that deviate "as much as 0.5 per cent." from the others, Millikan writes, in italics and without repeating the previous qualification: "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, during which time the apparatus was taken down several times and set up anew."<sup>124</sup> In his book *The Electron*, Millikan uses this passage and all the data of the 1913 paper in Chapter five, "The Exact Evaluation of  $e$ ." He adds for extra emphasis: "These [58] drops represent all of those studied for 60 consecutive days, no single one being omitted."<sup>125</sup>

#### 14. DROP NO. 41 REVISITED: THE LABORATORY NOTEBOOKS, 1911-1912

All these publications and the controversy itself take on additional significance in view of the happy circumstance that two laboratory notebooks have been found from the years of 1911 and 1912, which contain the data of observations and some of the data reduction which led to Millikan's *Physical Review* paper of 1913.<sup>126</sup> The first notebook starts with an entry dated 28 October 1911, "Density of Clockoil. By R. A. Millikan,"<sup>127</sup> and ends some 110 pages later, with a run dated 11 March 1912. On each page there is typically an experiment in which one oil drop is followed during changes of charge as it picks up ions from the air. Some experiments are lengthy and elaborate, fewer are brief, and a small fraction is aborted early. The second notebook starts with a run on 13 March 1912; and the last run, about sixty-five pages later, is dated 16 April 1912. Again, there is usually one experiment per page. In all there are about 140 identifiable runs in approximately six months.

Millikan's energy is evident in long series of runs following one another. The controversy over the existence of the electron is in full

<sup>124</sup>Or more precisely, deviates from the straight line on the graph of  $e_1^{2/3}$  against  $l/a$  or  $1/pa$ , where  $l$  = mean free path,  $a$  = radius of droplet,  $p$  = pressure in chamber.

<sup>125</sup>*The Electron*, op. cit. (note 7), p. 106.

<sup>126</sup>These protocols are in the Millikan Archives, Folders 3.3, 3.4, with "Oil Drop Experiments, R. A. Millikan" written on the covers.

<sup>127</sup>This is followed by an entry made later, a second determination "by L. J. Lassalle, 10/31/11." Millikan frequently credited his students with participating in experiments, and in the laboratory notebooks not all the notations are in Millikan's handwriting.

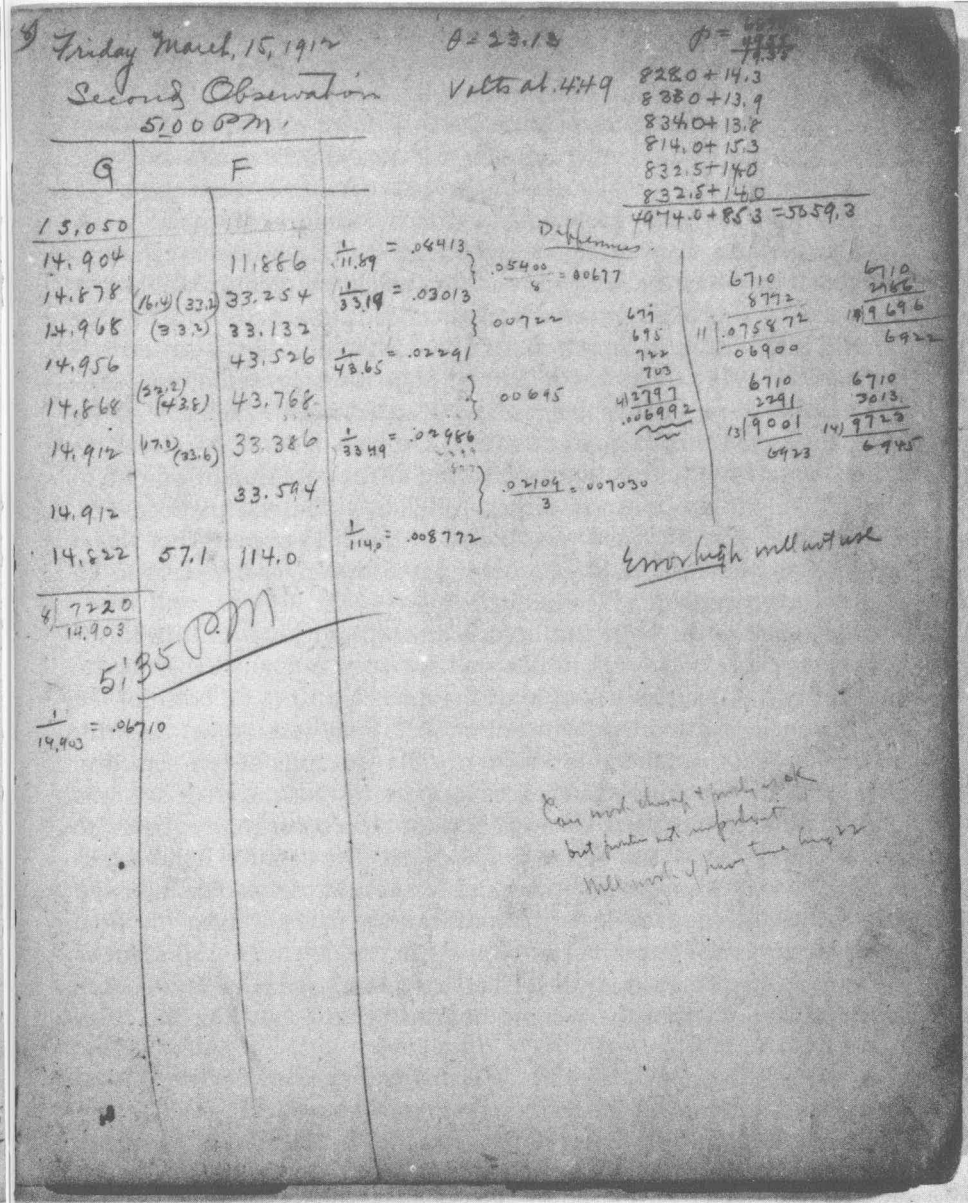
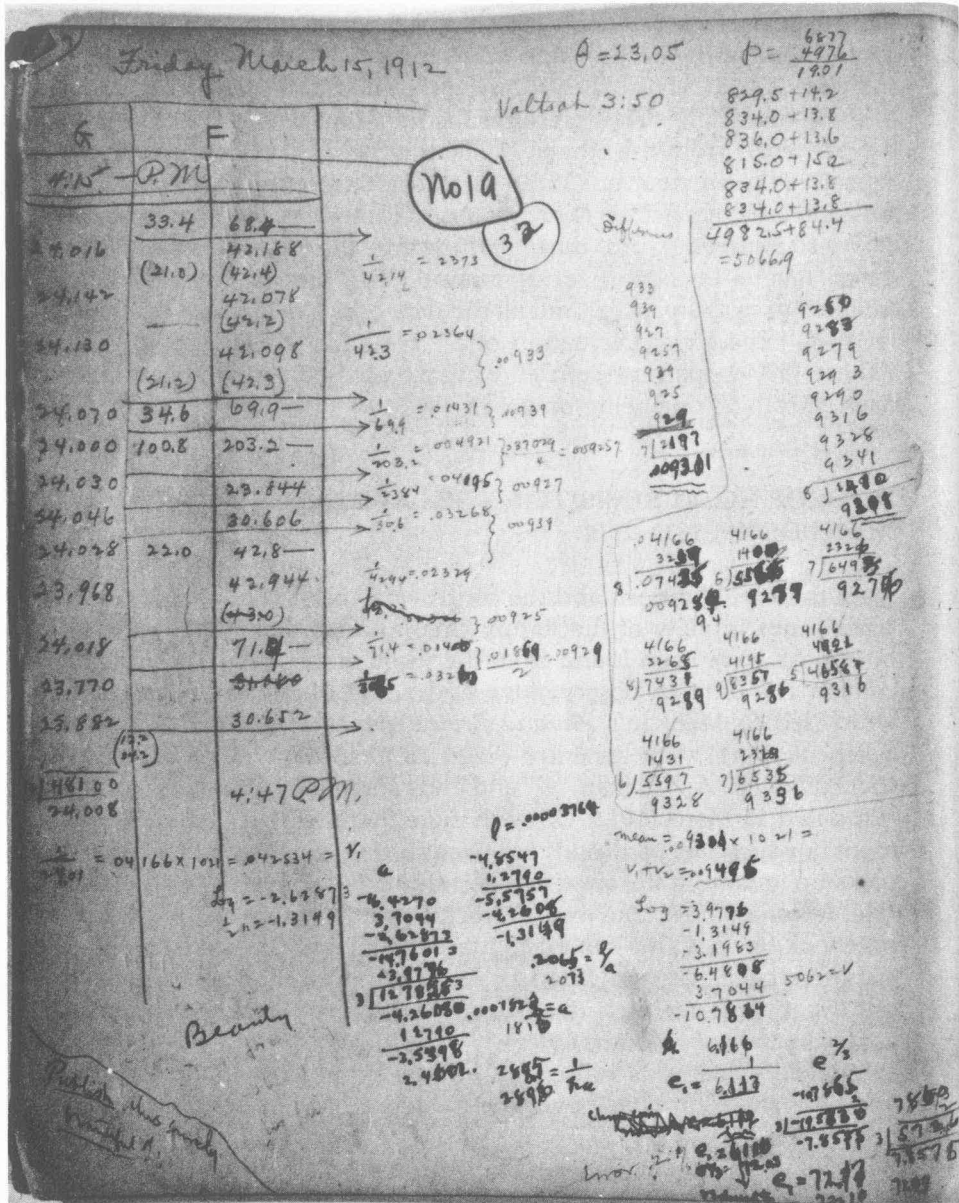


Figure 4. First and second observations in Millikan's laboratory notebook, 15 March 1912.

swing, and the stakes are high. While the work is still beset by difficulties, Millikan and his students are no longer novices. Millikan had been carrying out some form of droplet experiment for about five years, and the techniques used in 1911–1912 are not explorations on unfamiliar territory. Furthermore, as we trace details of the analysis, we encounter fine details and ingenious decisions made hour by hour. The work recalls Henry A. Murray's definition: "Science is the creative product of an engagement between the scientist's psyche and the event to which he is attentive." It was clearly this to Millikan.

Figure 4 refers to data taken at an advanced stage of the work in the second notebook and only a month before the end of the series that yielded the 1913 paper. The left-hand page is a representative example, chosen here because it is the raw protocol from which one of the published tables of data (in this case, Drop No. 41 as later renumbered) was drawn. Thus we are looking at the experiment on one of the fifty-eight drops upon which Millikan's final calculation of  $e$ ,  $4.774 \pm 0.009 \times 10^{-10}$  esu, was based in the 1913 paper. This was a value that Millikan could stay with for a dozen years, despite all further improvement of technique.

Every part of the page can be coordinated quickly with the corresponding published version that we have seen in Figure 3. Thus the first column (G) is equivalent to  $t_g$ ; the next column is  $t_F$ , both for the full distance of the drop's descent of 10.21 mm and, sometimes, for half that distance. At the top right are the readings of temperature, pressure, and potential differences (apparently for different sections of the battery, modified by a calibration correction that appears to have been revised later, before publication). The detailed hand calculations by logarithms in the lower right quadrant can also be followed up to the determination of  $e_1$ . Modifications made during the final computation prior to publication appear in the notebooks, sometimes several in different inks and pencil on the same page, and some pages carry indications that the recomputations occurred during the summer of 1912.

A key point in Millikan's work is his comparison of two sets of figures for each run. In the first run in Figure 4, one set is given under "Differences" (seven entries, starting with [0.00]933). The other is just to the right of it (eight entries, with the computed average of [0.00]9301). Each entry in the first of these two columns is a calculation of a quantity proportional to the elementary ionic charge,  $e_{\text{ionic}}$ , i.e., of  $1/n' [(1/t'_F) - (1/t_F)]$ , obtained exactly as in the table reproduced in Figure 3. For example, [0.00]9257 is one quarter of the difference between the reciprocal times of successive ascents ( $1/t'_F = 1/23.84$  sec,  $1/t_F = 1/203.2$  sec), on the assumption that  $n' = 4$ , that four integral charges were picked up between the measurement of  $t_F$  and  $t'_F$ .

In the same way the entries in the other column refer to the calculation of a quantity proportional to the elementary frictional charge,  $e_{\text{frict}}$ , i.e., of  $1/n [(1/t_g) + (1/t_F)]$ , again as in Figure 3. For example,  $1/t_g$  is here  $1/24.01$  sec (or  $0.04166 \text{ sec}^{-1}$ ) for all parts of the experiment. To this are added successive values of  $1/t_F$ , as previously calculated—but again the *assumption* is made for each entry that some integral multiple (8, 6, 7, 8 . . .) of charge is present.

Both assumptions become plausible when the scatter of data in each of the two columns is shown to be small, and when the mean values obtained in each of the two columns, so differently based, are nevertheless nearly equal. This is just what happens here: 0.009311 and 0.009301 are only about 0.1 percent apart. (Figure 3 indicates that recalculations prior to publication changed the first of these values to 0.009314; but it is still a good agreement.) Millikan expresses his pleasure at these results in the lower left corner. He writes: "Beauty. Publish this surely, beautiful!"

These readings took about half an hour. Thirteen minutes later Millikan was ready for another run, as indicated on the upper right-hand page (Figure 4). Considering his energy and long experience, Millikan may have used these minutes between runs to make the first rough calculations from his data (although occasionally with small arithmetical errors), subject to later reexamination.

We can look briefly at the right-hand page to see how the next run went—evidently not well. This was now a heavier drop, as seen from the fact that  $t_g$  is shorter. It did not change its charge drastically between ascents, and it appears to have been lost sooner than Millikan would have liked, leaving only four "Differences." Worst of all, the average values indicating  $e_{\text{ionic}}$  (0.006992) and  $e_{\text{frict}}$  (about 0.00692) are a full one percent apart. Millikan notes frankly: "Error high will not use"; and he adds (probably later): "Can work this up & probably is ok but point is [?] not important. Will work if have time Aug. 22." It was a failed run—or, *effectively, no run at all*. Instead of wasting time investigating it further, he simply went on to make another set of readings with a new drop, recorded on the next page of the notebook. Again it was a heavy drop, and for a while it was touch and go whether the data could be considered meaningful. He noted on the margin: "Might omit because discrepancy . . ."; but then he crossed that note out. Ultimately these data, christened Drop No. 39, made it into the final, published set.

The second set of observations on 15 March 1912 was by no means on the worst drop, nor was the first one (No. 41) the best in the series. But it is clear what objections Ehrenhaft would have raised against this procedure if he had had access to this notebook. If Ehrenhaft had obtained such data, he would probably not have neglected the second

observations and many others like it in these two notebooks that shared the same fate; he would very likely have used them all. For example, the entries on the right-hand page, which Millikan abandoned, make excellent sense if one assumes that the smallest charge involved is not  $e$  but, say, one tenth  $e$ . Thus, in the top right-hand corner of the page, if the sums given for  $(1/t_g) + (1/t_F)$  such as 0.075872, 0.09001, and 0.09723 are divided *not* by the integers 11, 13, and 14, but by 10.9, 12.9, and 13.9, a value proportional to  $e_{\text{frict}}$  results which matches almost exactly the mean of 0.006992 obtained earlier for the ionic charge under "Differences." 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  $0.1e$ .

Support for the conception of subelectrons would not fit with the rest of the physics of the time. From Ehrenhaft's point of view it was, for just this reason, to be regarded as an exciting opportunity and challenge. 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. Admittedly the integral character did not come through in every one of these runs, but that was to be expected. In real life, observations of this sort are beset by a number of difficulties, some more obscure than others; but one feels sure that eventually they can be explained and removed or dealt with by plausibility agreements. Millikan's notebooks record many different observations and hypotheses explaining "failed" runs: the battery voltages have dropped, the manometer is air-locked, convection often interferes, the distance measurement may have to be recalibrated, the temperature of the room must be kept more constant, stopwatch errors occur, the atomizer is out of order.

In the meantime Millikan had quite enough observational material left—58 drops out of about 140—to make a sound case, the more so as the integral value of  $e$  fit very well with other secure and unchallenged facts such as Rutherford's measurement of the charge of the alpha particle. Indeed, Millikan would have warned Ehrenhaft that using *all* readings equally, just as they come in, would be defensible only in a completely routinized situation where the chances for artifacts entering by the "open window" have become negligible. This was by no means the case in their experiments. Thus, at the end of a long run on 20 December 1911, Millikan was puzzled by a value of  $e$  far outside the expected limits of error. Aware that occasionally some material such as dust might still intrude in the observation chamber,

he calmly explained the discordant result to himself by a marginal note: " $e = 4.98$  which means that this could not have been an oil drop."

This remark illustrates again that the results of Millikan and of Ehrenhaft were quite sensitive to the treatment of data, and, before that, to the decision about what is the relevant or even crucial aspect of the experimental design, which data are discordant or suspicious, and which may be dismissed on grounds of plausibility. It is generally true that prior to the absorption of research results into canonical knowledge, the selection of the relevant portion of a range of experience that is in principle infinite is guided by a hypothesis. That hypothesis in turn is stabilized chiefly by its success in handling that "relevant" portion and by the thematic predisposition which helps focus attention on it.<sup>128</sup>

Of course, Millikan did not need to worry that Ehrenhaft might use these discordant results. His notebooks belonged to the realm of private science, with many decisions to be made before the work was fully done. Therefore he evaluated his data and assigned qualitative indications on their prospective use, guided both by a theory about the nature of electric charge and by a sense of the quality or weight of the particular run. It is exactly what he had done in his first major paper, before he had learned not to assign stars to data in public. Nor is this unfamiliar to anyone who has done basic experimental research; one does respond to small clues in the midst of a run to discern the extent to which the numbers one duly notes down do in fact stem from the phenomena to which one is trying to attend.

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. Here are some of Millikan's exclamations as the work proceeds, as recorded in the notebooks next to the data and calculations:

<sup>128</sup>Another example of this sort, based on the analysis by R. Bär, *Naturwissenschaften*, 10 (1922), 344–345, shows how crucial it was to discover when the measurement of potential differences was vitiated by changing voltages in the battery, changing calibration, and so forth. Thus, the ratio of successive charges on a droplet,  $(n_1/n_2)$ , will be given by the inverse ratio of the corresponding potential differences needed to suspend it against the pull of gravity,  $(1/v_1):(1/v_2)$ . Thus,  $(n_1/n_2) = (v_2/v_1)$ . For example, if by experiment  $V_1 = 47.5$  volts and  $V_2 = 71.1$  volts, then  $n_1/n_2::71.1:47.5$  or (to about two parts in one thousand) 3:2. Such a result would strongly support the hypothesis that droplets are charged in whole multiples of one basic charge. But if errors produce a difference in the measurement of the relative value of  $V_1$  or  $V_2$  of only one percent, the case looks very different. Thus if  $V_1$  were thought to be 47.0 volts, the ratio of  $n_1$  and  $n_2$  would be 71.1:47.0 or, to the nearest integers, 711:470—by no means a convincing proof of the quantization of charge and, conversely, evidence for unit charges much smaller than the charge of electron.

Very low Something wrong [11/18/1911]. Very low Something wrong [11/20/11]. This is almost exactly *right* & the best one I ever had!!! [12/20/11]. Possibly a double drop [1/26/12]. This seems to show clearly that the field is not exactly uniform, being stronger at the ends than in the middle [1/27/12]. Good one for very small one [2/3/12]. Exactly right [2/3/12]. Something the matter. . . [2/13/12]. Agreement poor. Will not work out [2/17/12]. Publish this Beautiful one. . . [2/24/12]. *BEAUTY* one of the very best [2/27/12]. Perhaps Publish [2/27/12]. *Excellent* [3/1/12]. *This drop flickered* as the unsymmetrical [3/2/12].

This continues, with beauty appearing more consistently as the work progresses, and ends thus during the last week or so:

Can't get differences [4/8/12]. Beauty. Tem & cond's perfect. no convection. Publish [4/8/12]. Publish Beauty [4/10/12]. Beauty Publish [crossed out and replaced by] . . . Brownian came in [4/10/12]. Perfect Publish [4/11/12]. Among the very best [4/12/12]. Best one yet for all purposes [4/13/12]. Beauty to show agreement between the two methods of getting  $v_1 + v_2$  Publish surely [4/15/12]. Publish. Fine for showing two methods of getting  $v$  . . . No. Something wrong with the therm.

## 15. SUSPENSION OF DISBELIEF

Two rather contrary tendencies are visible as we watch Millikan at work. One is the scientist's standard behavior of obtaining information in as depersonalized or objective a manner as possible. As every novice is taught, the graveyard of science is littered with those who did not suspend belief while the data were pouring in. But there is the other side of the coin, a strategy without which new scientific work could not get past the hurdles whose exact nature can be identified in detail only after the fact. To understand this side of the researcher's behavior, I introduce the notion of the "Suspension of Disbelief"; that is, the scientist's ability during the early period of theory construction and theory confirmation to hold in abeyance final judgments concerning the validity of apparent falsifications of a promising hypothesis.<sup>129</sup>

<sup>129</sup>Despite some differences, I am using this phrase as Coleridge, in *Biographia Literaria* (1817), applied it to the operation of the literary imagination. See also the letters of John Keats, 28 December 1817 and 19 March 1819. For an example of the literature of the place of "belief" in scientific work, see Max Born, *Natural Philosophy of Cause and Chance* (Oxford, 1951), pp. 123, 290.

This aspect of the operation of the scientific imagination is one of its key features and one that does not contradict the notion that falsification can be a useful tool in science. In Popper's well-known formulation: "I arrived . . . at the conclusion that the scientific attitude was the critical attitude which looked not for verifications but for crucial tests; tests which could *refute* the theory tested. . ."<sup>130</sup> The criterion of falsification may or may not be adequate for the analysis of scientific work in its later stages, when it has become part of a public dialogue; but the "Suspension of Disbelief" exemplified in Millikan's work in progress clearly exhibits a mechanism necessary for stabilizing belief in the efficacy of a hypothesis long enough to help it survive to the later stage of testing in public discussion, whether that testing be by falsification or other criteria.

If Millikan's only scientific achievement were the oil drop experiment, he might be open to the charge that he was lucky in guessing at the usable data or fortunate in his obstinacy. Such a charge, however, would collapse in the face of his next and perhaps most influential work, his resumed investigation of the photoelectric effect.<sup>131</sup> Here he found himself working with the wrong presupposition, but he knew how to rid himself of it eventually. Millikan launched into that work with the same energy and obstinacy as he had into his earlier work on the quantization of the charge on the electron, yet with the opposite assumption. As easy as it had been for him to adopt quantization as a thematic hypothesis for electricity, secure in the belief that it was an ancient and sensible idea, for a long time he regarded the application of the quantum hypothesis to the energy of light as an unacceptable novelty. Millikan wrote that Einstein's "bold, not to say reckless," hypothesis "seems a violation of the very conception of an electromagnetic disturbance"; it "flies in the face of the thoroughly established facts of interference."<sup>132</sup> On accepting the Nobel prize, Milli-

<sup>130</sup>K. Popper, "Autobiography," *The Philosophy of Karl Popper*, ed. P. A. Schilpp (LaSalle, Ill., 1974), p. 29. Among other challenges to the falsification theory, see E. Nagel, "What Is True and False in Science?" *Encounter*, 29 (1967), 68-70.

<sup>131</sup>In the presentation of the Nobel prize for physics for 1923, Millikan's measurement of elementary charge was described in considerable detail, whereas the investigation of the photoelectric effect was relegated to the last paragraph. However, the chairman of the Nobel Prize Committee for Physics revealed that the photoelectric work was relevant for two other Nobel prize decisions: "Without going into details I will only state that, if these researches of Millikan had given a different result, the law of Einstein would have been without value, and the theory of Bohr without support. After Millikan's results, both were awarded the Nobel prize for physics last year." E. Gullstrand, *Nobel Lectures: Physics 1922-1941* (Amsterdam, 1965), p. 53.

<sup>132</sup>R. A. Millikan, "A Direct Photoelectric Determination of Planck's 'h'," *Physical Review* 7 (1916), 384. A good, brief discussion of this episode is given in Roger H. Stuewer, *The Compton Effect: Turning Point in Physics* (New York, 1975), pp. 72-77. For

kan reported: "After ten years of testing and changing and learning and sometimes blundering . . . this work resulted, contrary to my own expectation, in the first direct experimental proof in 1914 of the exact validity . . . of the Einstein equation. . . ."<sup>133</sup>

## 16. TOWARD EHRENHAFT'S PRESUPPOSITIONS

In their ability to exploit and, if necessary, transcend their presuppositions, Millikan and Ehrenhaft differed greatly. I turn once more to Ehrenhaft to try to understand his presuppositions and motivations. The notebooks of his laboratory group did not survive; this impedes the fuller study which he deserves, but much can be retrieved from the published materials. Out of the wealth of papers issuing from the Vienna laboratories, one that Ehrenhaft published in the *Physikalische Zeitschrift* in 1910 provides important clues.<sup>134</sup> The key data in this instance again support his contention that if an indivisible atom of electricity existed, "it would seem to have to be smaller than  $1 \times 10^{-10}$  esu," if it can exist at all. Ehrenhaft presents a set of one thousand individual measurements on fog droplets, created by blowing moist air over white phosphorus. The measurements were taken from a previous publication of Karl Przibram, who apparently had undertaken these measurements at the request of Ehrenhaft, using a method proposed to him by Ehrenhaft.

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interesting material on the attitude of Millikan and others in the United States toward the quantum theory, see K. R. Sopka, *Quantum Physics in America, 1920-1935* (diss. Harvard Univ., 1976).

<sup>133</sup>Millikan, *op. cit.* (note 34), pp. 61-62. As late as 1920, Millikan was still not convinced: "The emission of electromagnetic radiation may or may not take place quantum-wise" (*Science*, 51 [1920], 505). In an address in December 1912, in which he declared that "the atomistic conception of matter has silenced the last of its enemies," he was struggling for some compromise that would avoid the photon. "That we shall ever return to a corpuscular theory of radiation I hold to be quite unthinkable." Similarly with the ether: "To deny the existence of this vehicle . . . is a bit of sophistry . . ." (R. A. Millikan, "Atomic Theories of Radiation," *Science*, 37 [1913], 133). Millikan was evidently able to adopt antithetical themata in different parts of his research, and he could overcome a deeply held thematic hypothesis when the experimental material would not coordinate with it.

<sup>134</sup>Ehrenhaft, *Phys. Zs.*, *op. cit.* (note 112). The paper contains a transcript of a discussion on the paper, with questions raised by various physicists. From April 1910 to March 1911, Ehrenhaft and Przibram turned out numerous publications, albeit there was considerable overlap in each case. For Ehrenhaft's publications during this period see *op. cit.* (notes 108, 110, 112) and the additional articles in *Physikalische Zeitschrift*, 12 (1911), 94-104 and 261-268. For Przibram's articles see *op. cit.* (note 108); *Anzeiger Akad. Wiss. (Vienna)*, no. 17 (30 June 1910), p. 262; *Sitzungsber. Akad. Wiss. (Vienna)*, 119 (1910), 869-935 and 1719-1753; and *Physikalische Zeitschrift*, 11 (1910), 630-632, and 12 (1911), 260-261.

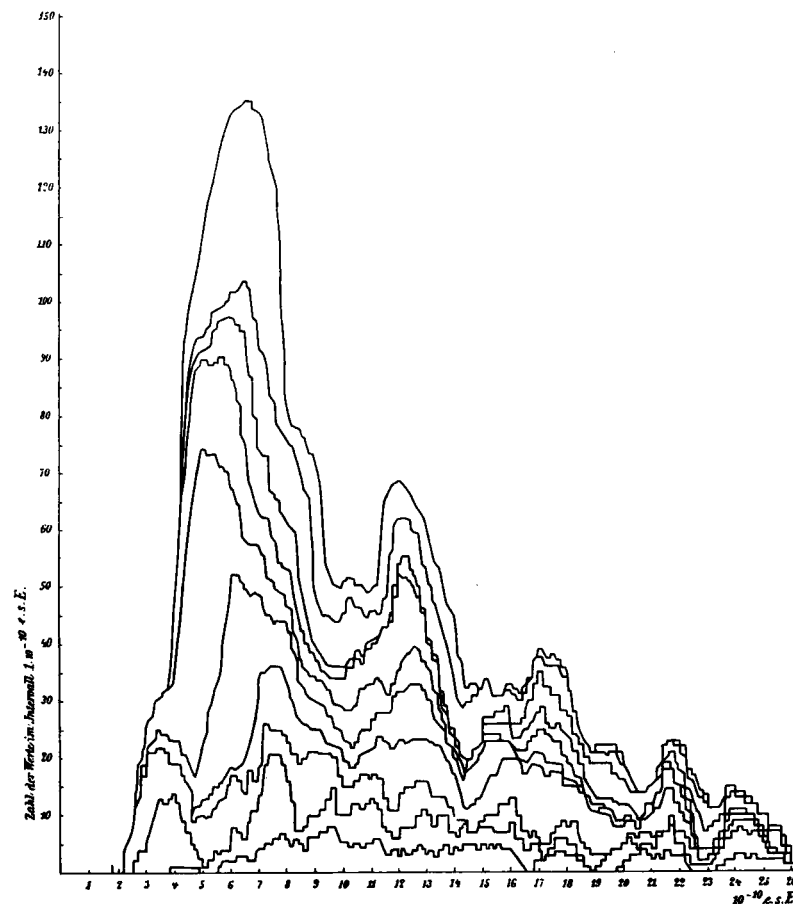


Figure 5.

Figure 5 presents the results. Along the abscissa are the observed charges, in units of  $10^{-10}$  esu; along the ordinate, the number of observed cases. The graph displays the first hundred data as the histogram with the lowest profile. To this the next hundred data are added to make the second histogram, and so forth. The striking fluctuation of the daily maxima was acknowledged to be mysterious but was felt not to undermine the essential conclusion: the peaks are not separated by simple integral relations, nor is there any reason to believe that a continuation of this process should not yield charges even smaller than those found. The statement in the title of the article is certainly borne out by the results displayed.<sup>135</sup>

<sup>135</sup>Ehrenhaft, *op. cit.* (note 108).

For many years to come, those who read Ehrenhaft's papers or heard his talks were disturbed, puzzled, and unable to propose definite explanations for his seemingly anomalous results. In retrospect, it is clear that at least one methodological difficulty had entered the experiment, and it is significant that the remedy for it would not normally be suggested in a public scientific meeting. Ehrenhaft and his colleagues appear to have used all their assiduously collected readings, good, bad, and indifferent; they did not apply the kind of discrimination we saw at work in Millikan's private analysis of his data. On the contrary, the bias now was in the opposite direction. The "window" was opened and all "measurements" were admitted. Ehrenhaft's method was not altogether different from what students do today when they repeat a well-established experiment, nor were the results he obtained. Figure 6 (a and b) illustrates this point by showing the widely scattered results recorded in some recent student experiments on the electric charges on oil drops.

Another ironic possibility for explaining Ehrenhaft's results is that the equipment in Vienna was rather more sophisticated than necessary. Millikan's equipment and procedure, at least in the crucial early phase, appear to have been much more primitive than Ehrenhaft's. Millikan's simple apparatus was put together in a rather homespun way. The atomizer was originally a perfume sprayer bought at a drug store; and the telescope was a short focus tube set up two feet from the 1.6 cm gap in the horizontal (22 cm diameter) air condenser.<sup>136</sup> Ehrenhaft's equipment was far more sophisticated and tended to involve the new ultramicroscope (with which Siedentopf and Zsigmondy had caused a sensation in 1902) which permitted observation of objects down to a limit about five hundred times below the resolving power of an ordinary microscope. Ehrenhaft himself had perfected its use in the observation of Brownian Movement. The condenser system he used was about an order of magnitude smaller than Millikan's in each dimension, and the range of size of charged objects he could follow was far wider. Thus, it permitted measurements on much smaller objects, a procedure which fitted with his conception that in looking for the smallest charges one should look at the smallest available objects. As to fears that Stokes' law would break down in that realm, Ehrenhaft had two responses. First, any correction needed should be derived by empirical methods rather than by building the conception of a unitary electron into the method of correction, as he believed Millikan to be doing. Second, corrections to Stokes'

<sup>136</sup>Millikan was by no means sloppy or careless. On the contrary, he insisted on precision where it counted—for example, that accurate potential differences be measured. See R. Bär, *op. cit.* (note 8), pp. 344–345.

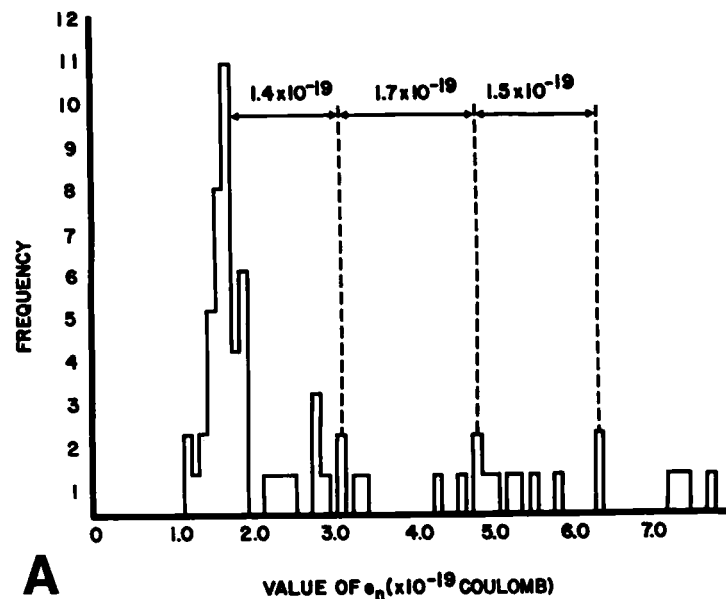


Figure 6a. A histogram of 74 drop charges determined by seven student pairs. Redrawn from the original by student R. Williams, Western Michigan University, Fall 1969. Graph and caption from H. Kruglak (note 10).

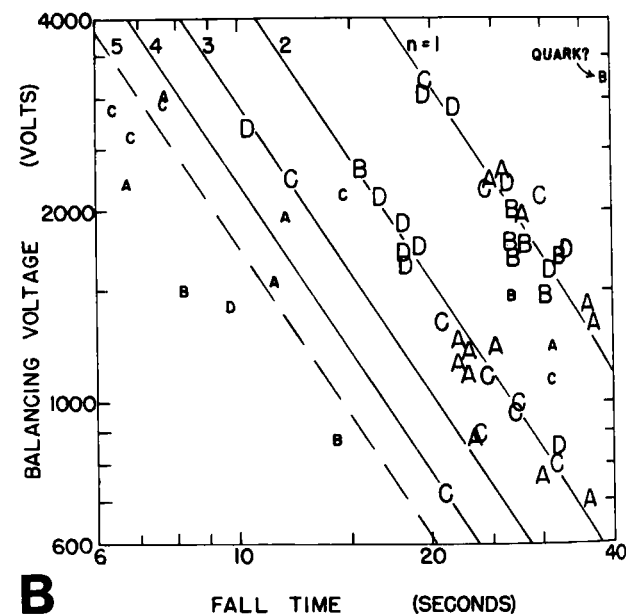


Figure 6b. Raw data of balancing voltage and fall time obtained by students in the 1971 class (four laboratory sections of approximately 15 students each are identified by letter symbols). Data points for  $n > 4$  were discarded as were a few apparent blunders (designated by small symbols). Graph and caption from M. Heald (note 10).

law would tend to make the small charges he was finding even smaller, so that the corrections could in no way affect the essential conclusion.

While Millikan may have appeared to be looking at the world of charged particles through a curiously primitive device, that device was in fact a key to Millikan's success. The particular dimensions of the apparatus he initially chose and the voltage of the battery available were

the element which turned possible failure into success. Indeed, Nature here was very kind. She left only a narrow range of field strengths within which such experiments as these are at all possible. They demand that the droplets be large enough so that the 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.<sup>137</sup>

Nature is not kind to everyone. Relatively few scientists know how to find or seize upon a "device of choice" that becomes the tool for opening up an area of research. Galileo fastened on the pendulum and the rolling ball as keys to dynamics. Fermi used the slow neutron, and Einstein the thought experiment of a freely falling experimenter noticing the seeming absence of gravitational effects. Ehrenhaft refused to see any resemblance between such cases and Millikan's device. On the contrary, Millikan's work seemed to him unacceptable on epistemological and methodological grounds. That Millikan restricted his measurements to a small region of mass, to droplets that are relatively large, and that he did not allow the use of arbitrarily large and small droplets, Ehrenhaft regarded as a detrimental feature. Valid findings, he felt, should exhibit themselves over a large range rather than within a relatively small sanctuary.

## 17. EHRENHAFT ABANDONS THE ELECTRON

Ehrenhaft's conversion from his original, expressed belief in the elementary quantum of electricity was so rapid and fervent that we can specify the period when it seems to have occurred. His last paper devoted to measuring the charge on the electron, and hence implying

<sup>137</sup>Millikan, *op. cit.* (note 34), pp. 57-58.

his acceptance of the electron hypothesis, was received by the *Physikalischen Zeitschrift* for publication on 10 April 1909.<sup>138</sup> Slightly over a year later, by 21 April 1910, the date of the short note in the *Anzeiger*, he had begun to change his mind: "An indivisible quantum of electricity, which theory presupposes to exist, would have a value considerably below the one previously accepted."<sup>139</sup> By 12 May 1910 the atom of electricity had shrunk to below  $1 \times 10^{-10}$  esu, and the question "whether it can exist at all" was proposed as the subject of forthcoming research.<sup>140</sup> When the first full-scale paper appeared in May 1910, the words "elektrisches Elementarquantum," which had been in the titles of the 1909 papers and had slipped to the subtitles of the short notes of 21 April and 12 May 1910, had disappeared from the title entirely.<sup>141</sup>

While Ehrenhaft made some gestures to connect the new work with that of 1909, which had used similar experimental equipment, it is clear that by the third week of April 1910 Ehrenhaft had at least very serious doubts about the electron of which there were no hints in 1909. By mid May 1910 he was quite confident about the need for subelectrons that, in principle, might have no lower limit of charge at all. He drew attention to the diversity of values reported in the literature for  $e$  (from 1 to  $6 \times 10^{-10}$  esu), values obtained both by different methods and by different observers using the same method. If one wants to avoid a style of science which piles up "hypotheses and corrections," he wrote, one is led to the recognition that the apparent variations of charge are grounded in nature.<sup>142</sup> The interpretation of the experiments has to be modified correspondingly. A few months later<sup>143</sup> these conclusions had become "certain beyond doubt"; one needed only to look at what nature herself made directly accessible to the senses of the assiduous experimenter, such as the data in Figure 5, to see the truth of the conclusions.

As Ehrenhaft's publications continued, there was increasingly an epistemological component in his work, that is, the use of his experiments to attack the credibility or necessity of atomism itself. In a long paper of 1914 summarizing his work and defending it against his critics, he still uses some of his older arguments.<sup>144</sup> He now believes that quanta of electricity, if they exist, should be at most on the order

<sup>138</sup>Ehrenhaft, paper no. 9 (note 76).

<sup>139</sup>Ehrenhaft, *op. cit.* (note 108).

<sup>140</sup>Ehrenhaft, *Anzeiger*, *op. cit.* (note 110).

<sup>141</sup>Ehrenhaft, *Sitzungsberichte*, *op. cit.* (note 110).

<sup>142</sup>Ehrenhaft, *Phys. Zs.*, *op. cit.* (note 110), p. 619.

<sup>143</sup>Ehrenhaft, *op. cit.* (note 134), p. 946.

<sup>144</sup>Ehrenhaft, *Sitzungsberichte*, *op. cit.* (note 98).



of  $10^{-11}$  esu. With this he can turn the tables on Millikan, for now the puzzle that needs explanation is why in the experiments of Millikan and others a specific value of  $e$  is found again and again. He hints at a theory that might explain why his smallest particles exhibit the smallest charges; this is to be expected, he explains, because the smallest quantities of electricity should be on bodies of smallest capacity.

But Ehrenhaft's attention is not chiefly on physical arguments. He deplores that while Ludwig Boltzmann, a few years earlier, still had to argue for the necessity for atomistics in the natural sciences, current views now accept this conception. "In recent years the atomistic theories of matter, electricity and radiation have gained more ground in physics than ever before."<sup>145</sup> Everyone in physics is convinced of the heuristic value of these theories; but if such a theory is more than a pure speculation, it must be solidly based on experiments that can withstand critical examination. Ehrenhaft notes that his study provides such an examination of the foundations of a portion of those hypotheses, the atomistics of electricity, and that his style is to proceed "from the *direct facts*."

Of course, there was never a direct laboratory disproof of Ehrenhaft's claims. In the 1916 edition of *The Theory of the Electron*, H. A. Lorentz still had to confess that "the question cannot be said to be wholly elucidated." In his review of the case, R. Bär noted in 1922 that "the experiments [of Ehrenhaft] left, at the very least, an uncomfortable feeling."<sup>146</sup> Like most such controversies, this one also faded into obscurity without anything as dramatic as a specific, generally agreed upon falsification taking place at all. Indeed, Ehrenhaft continued to publish on subelectrons into the 1940s, long after everyone else had lost interest in the matter.

## 18. "A BATTLE OF TWO WORLDS"

In his Nobel prize acceptance speech of 1923, Millikan had put an end to his side of the debate with a careful review of his work. A year after its publication in 1925, Ehrenhaft also gave a public address which signalled his realization that the controversy had ended for all practical purposes. As it happened, that address was also part of a ceremony, one held in a public park on a Saturday in Vienna. The occasion was the unveiling of a bust in honor of Mach to commemorate the tenth anniversary of Mach's death. Moritz Schlick delivered a

<sup>145</sup>*Ibid.*, p. 55.

<sup>146</sup>Bär, *op. cit.* (note 128).

eulogy.<sup>147</sup> Another contribution came from Einstein, who had admired Mach and had once specially sought him out during a visit to Vienna in 1911, a meeting apparently arranged by Ehrenhaft. Ehrenhaft's own presentation was brief but revealing.<sup>148</sup> Perhaps for the first time he there brought into the open one reason for his long fight against the atom of electricity. It had to do with his relationship with Mach.

Ehrenhaft saw Mach as a lonely fighter. Even the bust of Mach, which the authorities did not want in the arcade of the university building, stood there in the park "alone and isolated." Accepting Mach's own habitual underestimation of his influence, Ehrenhaft thought that Mach had "remained not understood and had so few followers, and those not among physicists. . . .":

I only want to draw attention to this: the great difference between Mach and most physicists arises from the fact that through the further development of physics each of the two opposing views shows itself to be ever more fundamental, ever more contrary and unbridgeable, like two professions of faith. Mach [appears] as an advocate of the much more modest, phenomenological point of view which finds satisfaction merely with the description of the phenomena and despairs of other possibilities. The others are advocates of views that through statistical methods and speculative discussions concerning the constitution of matter, are reflected in atomism; they believe themselves able to get down to the true Being of things.

Ehrenhaft's talk then ended with a Wagnerian crescendo:

Mach had the courage to set himself with mighty arguments against the current of the atomistic world view that was sweeping along nearly all others—against the very same atomistics that, in the smallest, supposedly indivisible constituents of matter and, recently, also of electricity, is supposed to have attained the magic keys for opening at last all doors of natural knowledge. But the world develops quite remarkably. On the one hand, daring researchers storm farther into the realm of atomistics, undaunted by such powerful thinkers as Mach; on the other hand, one must admit that the great man whom we celebrate today may be victorious in the end. Who dares to render judgment in this

<sup>147</sup>Schlick, *op. cit.* (note 23).

<sup>148</sup>Felix Ehrenhaft. "Ernst Mach's Stellung im wissenschaftlichen Leben," *Neue Freie Presse* (Vienna), Supplement, 12 June 1926, p. 12.

battle of two worlds? (*Wer wagt es, in diesem Kampfe zweier Welten das Urteil zu fällen?*)<sup>149</sup>

Ehrenhaft had indeed touched a key point. Whatever else the controversy was about, it was also about two ancient sets of thematically antithetical positions: the concepts of atomism and of the continuum as basic explanatory tools in electrical phenomena, and the use of methodological pragmatism versus an ideological phenomenology.

This is as far as one can safely go on the basis of the documents now available. Some tantalizing questions remain. At some point after his early, striking success in a physics based on atomism, Ehrenhaft evidently had been converted to antiatomism and to "antihypothetical" theorizing. Both of these positions were commonly identified with Mach, although Ehrenhaft was not a Machist in the positive and productive sense of the term. As we saw, the first indications of his change of mind appeared in the papers of late April and May 1910.<sup>150</sup> But to switch from one thema to its opposite is rarely done in science, and we naturally wonder what external influences may have helped Ehrenhaft reach his new point of view. The rebuff by Millikan published in February 1910 may well have played a role, although it is not likely to have been the primary influence.

We do not, and perhaps never will, know the reasons. But there is another unpublished letter in the Mach-Lampa correspondence that concerns Ehrenhaft, and it falls in the critical period when he was making the switch. It may contain a clue. The two-page letter from Lampa in Prague to Mach is dated 1 May 1910, just after Ehrenhaft's first, rather cautious announcement of 21 April and before his more detailed presentations of mid May.<sup>151</sup> Lampa first tells Mach about an attack on the philosophy they both shared, an attack coming from Planck—the last major physicist who still dared to attack Mach openly, although Mach and his circle saw themselves as a small, beleaguered group. Lampa notes that Planck has published a book "in which he maintains *in extenso* the views of his that you have been fighting against." Planck has embroiled himself hopelessly in con-

<sup>149</sup>Ehrenhaft and his remaining followers occasionally revived the discussion. As late as 1934, Alfred Stein, docent at the University in Vienna, reviewed the case, together with yet another set of Ehrenhaft's experiments. He concluded: "At any rate, the fight over the charge of the electron is still not decided—it is a war with heavy consequences on whose outcome depends the existence of today's physics . . ." ("Das Ende des Atomismus? Ehrenhaft erschüttert den Aufbau der Welt," *Wiener Zeitung*, Beilage, 19 August 1934, p. 3).

<sup>150</sup>Ehrenhaft, *op. cit.* (notes 108 and 110).

<sup>151</sup>Letter of Lampa to Mach in the Ernst Mach archives, Freiburg.

traditions; hence "reading it will give you much pleasure." While interesting from the point of view of physics, the book is "epistemologically childish."<sup>152</sup>

Then Lampa turns to the results of a recent trip to Vienna. Perhaps this was the occasion announced in his earlier letter of 9 February 1910 in which he had written to Mach: "I look forward with pleasure to be able to greet you personally in a few weeks, and to report to you then on the further developments of the case [the physics appointment, still pending in Prague]."<sup>153</sup> Without preliminaries, as if it were familiar territory to both, Lampa turns to the work of Ehrenhaft:

If the provisional measurements should be confirmed that Ehrenhaft, when I was in Vienna, carried out as part of his continuing research on the charges on colloidal particles, then the electron would be divisible. Even then Ehrenhaft had found particles with half electrons—in the meantime, Lang [whose assistant Ehrenhaft had been in 1903] has told me, he appears to have observed some with 1/3, 1/5 electrons.

It would be just too beautiful [*Es wäre doch zu schön*] if the electron were now to undergo the same fate as the atom did as a result of cathode rays. . . .

From the viewpoint of the more enthusiastic followers of Mach, such findings would have been, indeed, long awaited support for a cause battered by recent events. Perrin was conducting a successful crusade on behalf of molecular reality. Worse, Ostwald had defected from the cause, for in the 1908 edition of his text *Allgemeine Chemie*, he had recanted his antiatomism. "I am now convinced," he had written in the preface dated November 1908, "that we have recently acquired experimental evidence of the discrete or grained nature of matter, which the atomic hypothesis sought in vain for hundreds and thousands of years. [Experiments such as those of Thomson and Perrin] justify the most cautious scientist in speaking now of the experimental proof of the atomic nature of matter. The atomic hypothesis is thus raised to the position of a scientifically well-founded theory."<sup>154</sup>

<sup>152</sup>The reference is to M. Planck, *Acht Vorlesungen über theoretische Physik* (Leipzig, 1910).

<sup>153</sup>The appointment process continued to drag on. In the mid-June issue of the *Physikalische Zeitschrift*, 11 (1910), 552, there appeared a note that Einstein had been proposed to fill the vacancy at Prague.

<sup>154</sup>The "Monists," in particular, were jubilant about Perrin's work; thus, Jacques Loeb linked his famous essay "The Mechanistic Conception of Life" (1911) explicitly with Perrin's proof of the existence of molecules as the final vindication of the mechanistic philosophy.

In this dark period for the Machists, Ehrenhaft must have appeared to them as a bright new star.<sup>155</sup> He, in turn, can hardly have been oblivious to the favorable impression his preliminary new findings were making on them just in that provisional stage of his work and just when they were looking for new ideas—and for new men.

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<sup>155</sup>Lampa also undertook to describe Ehrenhaft's work in the popular semimonthly *Das Wissen für Alle*, 11 (1911), 45–47.