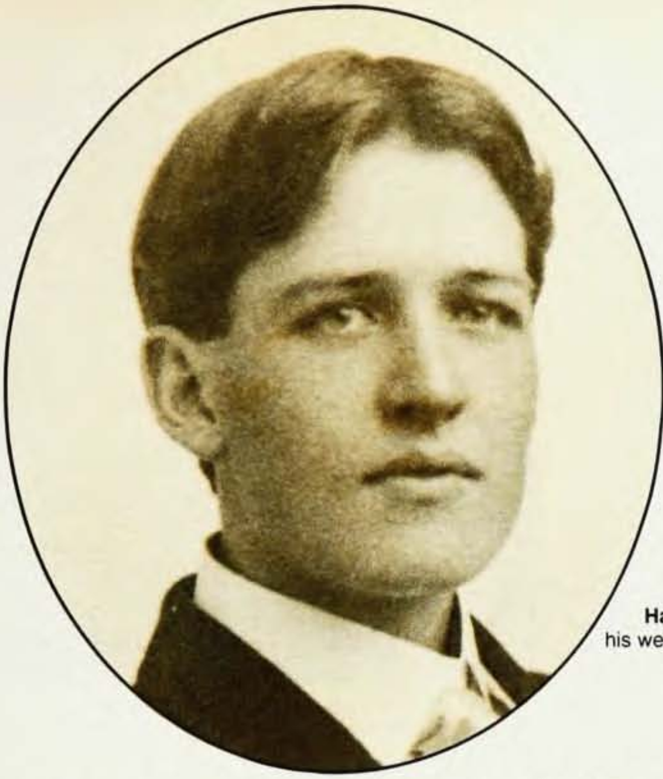
My work with Millikan on the oildrop experiment

Harvey Fletcher

Citation: Phys. Today **35**(6), 43 (1982); doi: 10.1063/1.2915126
View online: http://dx.doi.org/10.1063/1.2915126
View Table of Contents: http://www.physicstoday.org/resource/1/PHTOAD/v35/i6
Published by the American Institute of Physics.



Harvey Fletcher soon after his wedding in September 1908. (Photograph provided by Stephen Fletcher.)

My work with Millikan on the oil-drop experiment

In this personal reminiscence the late author recounts his experiences as a graduate student in the Ryerson laboratory in Chicago and his contribution to the determination of the electron's charge.

Harvey Fletcher

Lorena (Chipman) and I were married on 9 September 1908. Soon after we left by train for Chicago. On arrival there, we found a small apartment near the University.

My first problem was to get admitted and registered in the graduate school. I went to the admission authorities and presented my credits. [Fletcher had taken three years of college work at Brigham Young University, which was at that time sufficient for a BS degree.] They glanced at them and said it would take a little time before they could give me a definite answer. They made an appointment for four or five days later when I should come back. In the meantime I had become acquainted with Professor Millikan and others of the faculty of the physics and mathematics departments.

When I went back to the admission group I got the sad news that I must do four years of college work at Chicago before I could enter the graduate school. This was a great blow to me. After a sleepless night I decided to talk to Millikan about admissions. At that time he had just been made an assistant professor and seemed to be a very likeable fellow.

He indicated a way out for me. He said I could enter as a special student and select the courses a first-year graduate student usually takes. If I passed them successfully, the admissions committee might reconsider my entrance into the graduate school. I told him I was sure that I could. As a matter of fact, I had already taken courses similar to some of these at Brigham Young. So through his help I was able to enter as a special student.

The courses were not difficult, and I passed them all with high grades among the top in the classes. With this record I went back to the admissions committee, and they decided to let me enter the graduate school as a candidate for the doctorate with the condition that I make up one year of undergraduate college work at Chicago, preferably in those lines in which I was deficient, such as history, English, foreign languages, sociology.... I thus spent three full school years and two summers at Chicago and graduated in 1911. I was as well, if not better, prepared in physics and mathematics than any of my classmates who had graduated from the College at Chicago, but I was below them in my knowledge of subjects in the general educational field.

I had to borrow some money to complete my first year of graduate work. After that, through the influence of Millikan, I was able to get work in the University that paid enough to defray my school and living expenses for the remaining two years. During the second year I was given a job teaching science to high school students in the College of Education. I cooperated with other members of the faculty to map a general science course that

Harvey Fletcher (1884–1981) directed acoustical and, later, physical research at Bell Laboratories from 1925 to 1952, developing hearing aids and stereophonic equipment. He also taught at Columbia University and headed research at Brigham Young University.

0031-9228 / 82 / 0600 43-05 / $01.00

would be suitable for boys and girls of that age....

Also, that year I took charge of lantern projectors for various classes. I received a dollar for each lecture. This too helped out my finances. It was at the beginning of this second year [1909] that I went to Millikan to see if he could suggest a problem upon which I could work for a doctor's thesis in physics. He was a busy man, and I had a hard time making an appointment with him. Finally, he told me to come down to one of the research laboratories where he and Professor [Louis] Begeman were working and he would talk to me. First he and Begeman showed me the research work that they were doing on the electronic charge, and reviewed the work that J. J. Thompson and E. Regener had been doing along this line in Cambridge, England.

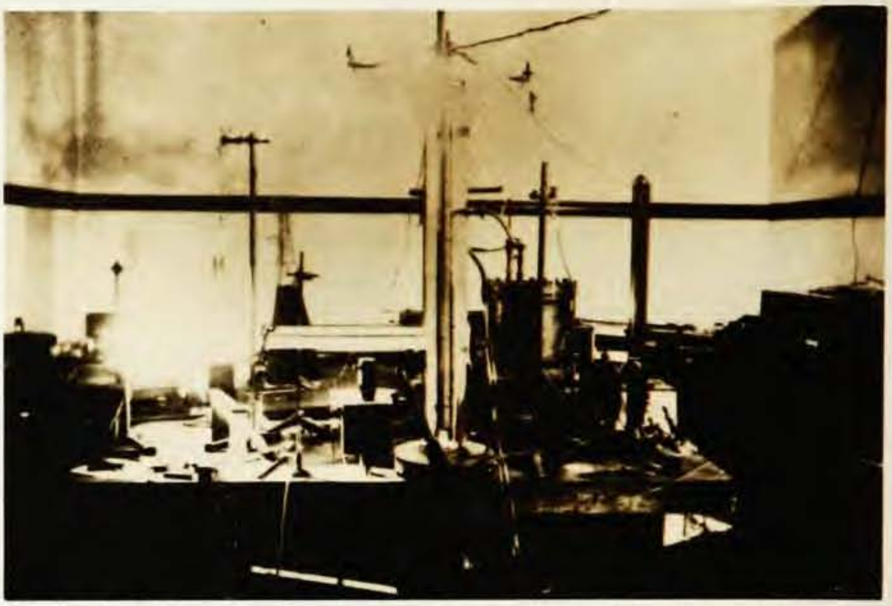
They had arranged a little box having a content of 2 or 3 cubic centimeters that was fastened to the end of a microscope. A tube was attached from an expansion chamber to the box. By opening suddenly a petcock, a sudden expansion of the air in the box caused a cloud of water vapor to form. When viewed through a microscope this cloud was seen to be composed of a large number of tiny water drops. The droplets would soon fall from the top to the bottom of the box under the influence of gravity. A conducting plate was arranged at the top and another one at the bottom of the box so that an electric field could be imposed.

When an electric field was turned on, it would retard the fall of some droplets. They were trying to make the field just right so that a selected droplet would be suspended in the air between the plates. From the speed of the droplet, that is the fall speed, and the intensity of the field to stop its fall, one could calculate the electrical charge on the droplet. This was essentially repeating the experiment that Regener did in England. However, the water forming the droplet evaporated so fast that it would only stay in view for about 2 seconds, so it was difficult to get more than a rough estimate of the charge.

We discussed ways and means of getting around the difficulty, and I think we all agreed that we should have a droplet that did not evaporate if we could get it small enough and could control it. Mercury, oil, and two or three other substances were suggested. In a discussion of that kind, it is rather difficult to be sure who suggested what. I left with the impression that I had suggested oil for it was easy to get and to handle. However, in his memoirs Millikan said he had been thinking of this before this conference. Of course, I cannot say yes or no to that, but I do know what happened after this conference.

Professor Millikan said to me, "There is your thesis; go try one of these substances which will not evaporate."

To build an apparatus like they were using would take considerable time. So I decided to make a crude setup in the laboratory and try it before designing an elaborate one. I went out to the



Apparatus for the oil-drop experiment at Caltech in the early 1920s. (Photograph courtesy California Institute of Technology Archives and AIP Niels Bohr Library.)

Source of the story

Last year Mark B. Gardner, of Spanish Fork, Utah, wrote an obituary of his long-time friend and co-worker, Harvey Fletcher, for PHYSICS TODAY (October 1981, page 116). In the course of correspondence with Gardner, we learned that Fletcher had left him a manuscript autobiography that included an account of Fletcher's work in the celebrated oil-drop experiment for which his thesis adviser, Robert A. Millikan, won the Nobel Prize in 1923. Fletcher had instructed Gardner to publish the manuscript only posthumously, so it would be clear that Fletcher had no personal interest motivating its publication. In fact, Gardner told us that Fletcher was deeply grateful to Millikan for the many kindnesses he accorded him and for the friendship that lasted throughout their lifetimes. He did not want in the least to tarnish Millikan's reputation. At our request, Gardner sent us the manuscript and obtained the consent of Fletcher's family to have it published.

Fletcher's account fills a gap in Millikan's otherwise extensive decriptions, in his books and his Nobel Prize Lecture, of the sequence of experiments he undertook to determine the magnitude of the charge of the electron. It relates how and by whom the apparatus for the final phase of the experiments, that using oil drops, was devised. The matter is all the more significant because of the importance that Millikan himself saw in the details and mechanism of the experiment. In his Nobel Lecture he said that "my own work has been that of the mere experimentalist whose main motive has been to devise, if possible, certain crucial experiments for testing the validity or invalidity of conceptions advanced by others regarding the unitary nature of electricity." Shortly afterwards came the remark, "The success of the experiments first performed in 1909 was wholly due to design of the apparatus, i.e., to the relation of the parts.... Scarcely any other combinations of dimensions, field strengths, and material could have yielded the results obtained."

Fletcher came to Chicago and to Millikan at a time when the existence of the electron was becoming widely accepted by experimentalists as more than a heuristic device. Only two years before, J. J. Thompson had published a paper reporting measurements of the constant charge-to-mass ratio of cathode rays, which, in Millikan's words, "put together, in a matchless manner, the evidence for the view that the cathode rays consist not of ether waves... but rather of material particles carrying electric charges, each particle possessing a mass of about $1/1000$ of that of the lightest known atom." Values were sought for the magnitude of the electron's charge. Early determinations were averages of very many hypothetical individual charges; they were indirect measurements at best, according to Gerald Holton in his essay on Millikan in *The Scientific Imagination.*

Millikan and his student Louis Begeman initially used such a method, one devised by H. A. Wilson, in which clouds of water droplets were produced in an expansion chamber between parallel horizontal plates of a charge condenser. This method assumed that Stokes's law held for the droplets, presupposed that each droplet formed on a singly charged ion, and ignored the effects of evaporation. The results that Millikan and Begeman produced, falling within a smaller range of values of e than those of Wilson, were only tentative.

Millikan attempted to improve his results by eliminating the error from evaporation. He drug store that afternoon and bought an atomizer and some watch oil. Then I came back to the laboratory and set up the following apparatus:

First, an arc light with two condensing lenses in front of it was set up. The combination made a bright beam of light. The experience I had with projection lanterns for lectures made it possible to get this together very quickly. I then used the atomizer and squirted some oil spray so that it fell through the beam of light. The light made these tiny drops of oil look like tiny stars. This indicated this part of the experiment would probably work. Next, I went down to the student shop and found some brass sheets about one-eighth of an inch thick. From them I cut two circular plates about 20 centimeters in diameter. I soldered a stem onto each one so that they could be held by an ordinary laboratory stand with clamps. A small hole was then bored in the center of the top plate. Next, the plates were set up horizontally about 2 centimeters apart. In this first setup the air between the plates was not enclosed. So I moved the stands holding the two plates over into the beam of light. I then put a large cardboard between the light and the plates and cut a hole just large enough to permit a beam of light to go between the plates without touching them. Next, I found a cathetometer, an instrument commonly used around a physics laboratory, and placed it so the telescope on it could be turned and raised or lowered until its line of sight went between the two plates at about 120° from the direction of the light beam. The distance from the telescope to the plates was about 1 meter. I then tried out the apparatus. I turned on the light; focused the telescope; sprayed oil over the top of the plate; then came back to look through the telescope. I saw a most beautiful sight. The field was full of little starlets, having all colors of the rainbow. The larger drops soon fell to the bottom, but the smaller ones seemed to hang in the air for nearly a minute. They executed the most fascinating dance. I had never seen Brownian movement before. Here was a spectacular view of it. The tiny droplets were being pushed first that way and then this way by the actual molecules in the air surrounding them. I could hardly wait until I could try an electrical field upon them to see if they were charged. I knew there were two or three banks of small storage cells in the laboratory. A large number of these had been connected in series and mounted in compartments on a small truck. Each one

The oil-drop experiment. Filtered air, into which an atomizer (A) blows oil droplets, is admitted into chamber (C). Droplets of oil find their way through pinhole (p) into an air condenser bounded by plates (M) and (N) held apart by ebonite posts (a); the plates are charged by the battery (B), controlled by switch (S). The oil drops are illuminated and seen through the window (c). (From Millikan's *The Electron* published in 1917.)

I. ISOLATION OF INDIVIDUAL IONS AND MEASUREMENT OF THEIR RELATIVE CHARGES

In order to compare the charges on different ions, the procedure adopted was to blow with an ordinary commercial atomizer an oil spray into the chamber *C* (Fig. 3).

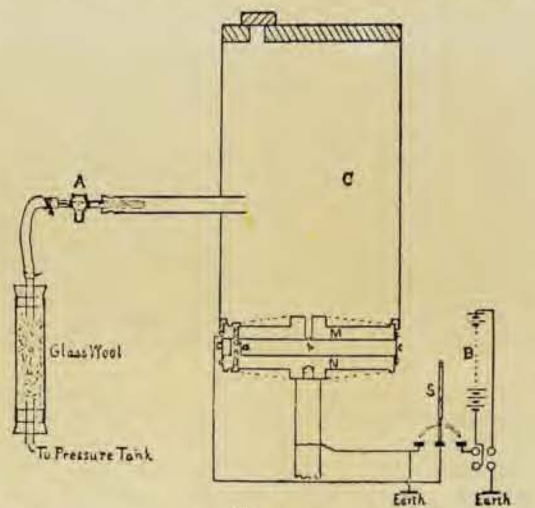


FIG. 3

The air with which this spray was blown was first rendered dust-free by passage through a tube containing gl[illegible]s wool. The minute droplets of oil con[illegible]ting the [illegible]

hoped to hold the cloud steady so that he could study its rate of evaporation. To do this, he increased the strength of the electric field, which actually had the effect of dispersing the particles by acting differently on differently charged particles. Millikan wrote, "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 remained in view." When he saw that by turning off the field, drops fell at different rates, he realized that their different weights had been balanced by their different charges or numbers of ions. Timing the descent of a droplet whose weight he could find by means of Stokes's law, he discovered the magnitude of its charge needed to balance its weight. Comparing droplets falling at different rates, he was able to eliminate most of the effect of evaporation. The results he obtained always came out, within the limits of his measurements, to 1, 2, 3, 4, or some other exact multiple of the smallest charge on a droplet he obtained. "Here then [from this balanced-droplet method] was the first definite, sharp, unambiguous proof that electricity was definitely unitary in structure." He obtained a value of e as 4.65×10^{-10} electrostatic units.

On 31 August 1909 Millikan presented his results at a meeting in Winnepeg, Canada, of the British Association for the Advancement of Science. Delivering his paper, he had the good fortune to be able to contradict a statement that Lord Rutherford had made a few days earlier: "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."

Soon after Millikan's return from Winnepeg, Fletcher came to him asking for help in choosing a thesis topic. Millikan showed him the apparatus that had been used for the balanced-drop experiment (which Fletcher describes in the early part of this excerpt) and suggested he work on using less quickly evaporating substances to find more accurate values of e. While the general features of Millikan's experiments had by this time—early fall 1909—been set, they did undergo a "process of significant maturation" according to Holton, while Fletcher worked with Millikan. Until then, values of different "balanced" water drops were compared. In the new procedure using oil drops, sets of data were obtained on the risings and fallings of a single oil drop. Droplets, often charged by friction, were introduced into an electric field between two charged plates. The charges on the droplets were changed by irradiating them. The speeds of descent of a single droplet with different charges were compared and found all to yield multiples of a smallest value, that value being e. Millikan had to make adjustments for the viscosity of the air (or other medium used) in using Stokes's law, because the particles observed were so minute that the medium could no longer be treated as entirely continuous. (When larger particles were observed, their behavior did conform to the law, but the advantage of having long times of descent—sometimes over a minute—was lost because the heavier particles fell much more quickly.)

Millikan and Fletcher continued the work for years, with many variations, and finally obtained a value of e at $4.774\ (\pm 0.005) \times 10^{-10}$ electrostatic units. —DG

Millikan (photo below, in the center) in 1908 flanked by A. A. Michelson (at left), Henry G. Gale (right) and Carl Kinsley (front). (Photograph by Crowe, courtesy AIP Niels Bohr Library.) **Fletcher in 1936 with Millikan** (to his left) and Leopold Stokowski, with whom he worked on recording equipment. (Photograph provided by Stephen Fletcher.)

of these units would produce 1000 volts dc at its terminals. I soon rolled one of them into place near my crude apparatus. Insulated wires were attached through a switch to the two terminals of the 1000-volt dc battery. I finished most of this that first afternoon. The next morning I spent some time adjusting it and installing a meter to read the voltages applied to the plates. I was then ready to try the battery on these tiny oil drops.

Once more the atomizer was used to spray some of the oil across the top plate. As I looked through the telescope I could see the tiny stream of oil droplets coming through the hole. Again I saw beautiful stars in constant agitation. As soon as I turned on the switch some of them went slowly up and some went faster down. I was about to scream as I knew then that some were charged negatively and others positively. By switching the field off and on with the right timing one could keep a selected droplet in the field of view for a long time. I went immediately to find Millikan, but could not find him so I spent the rest of the day playing with these oil droplets and got a fairly reasonable value of e before the day ended. The next day I found him. He was very much surprised to learn that I had a setup that was working. He came down to the laboratory and looked through the telescope and saw the same beautiful sight of the starlets jumping around that I had already seen and have described above. He was very much excited, especially after turning on the field. After watching for some time he was sure we could get an accurate value of e by this method. He stopped working with Begeman and started to work with me. We were together nearly every afternoon for the next two years. He called the mechanic who worked in our physics shop and we outlined a new design for our apparatus and asked him to build it. The principal changes were to make the plates more accurate and to enclose the air between the plates to prevent air drafts. Also, we obtained a radium source or x-ray source that we could shoot at the chamber to produce a greater ionization. The actual design is described in the first paper published about this work. I want to say more about this first paper later.

Making the principal changes took about a week. Afterwards we started in earnest on this research work, which was later to become so famous. After working five or six weeks we had the press come into our laboratory and see and hear our results. We also made a popular presentation. The papers were full of this wonderful discovery. It was the first real publicity that I had ever received. My name ran right along with Professor Millikan's in the newspapers. I spent considerable time showing these experiments to various VIPs from all over the country.

I remember one of them was the great Charles Steinmetz from the General Electric Company. He was one who did not believe in electrons. He could explain all the electrical phenomena in terms of a strain in the Ether. After watching these little oil droplets most of one afternoon, he came and shook my hand and said, shaking his head, "I never would have believed it. I never would have believed it" and then left.

This was all great publicity, but I began to wonder if this work was to be my thesis as Millikan had promised at that first conference in December 1909. However, during the spring of 1910 we started together writing a paper to be published about the new research.

I wrote more of it than he did, particularly about the modification of Stokes's law and the arrangements of the data. He went over it all and changed the phrasing somewhat to make it read better. All the time I thought we were to be joint authors.

Before going further let me quote some from that paper. If you want to read the whole paper, it is available in the library.

> "The Isolation of an Ion, a Precision Measurement of Its Charge and the Correction of Stokes's Law."
> *Science*, 30 September 1910
> . . . Mr. Harvey Fletcher and myself, who have worked together on these experiments since December 1909 have studied in this way between December and May from one to two hundred drops which had initial charges from 1 to 150 and made from oil, mercury and glycerine and 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.

Throughout the paper such statements as this occur:

> Mr. Fletcher and my own mean times on a given drop generally differ from each other by less than $\frac{1}{100}$ second.

Phyllis was born 21 May 1910, and as you will see, that is about the time we finished this first paper. When she was about one month old, I was babysitting with her as Lorena had gone out somewhere with some of her friends. Answering a knock, I went to the door and was surprised to see Millikan. I wondered why he had come to our humble apartment. I soon found it was to decide who was to be the author of the paper referred to above. There were four other papers in the formative stage that were coming out of these oil-drop experiments and I had expected they would all be joint papers.

He said that if I used a published paper for my doctor's thesis that I must be its sole author. The five papers on which we did the experimental work together were

▶ "The Isolation of an Ion, a Precision

Measurement of Its Charge, and the Correction of Stokes's Law." *Science*, 30 September 1910—Millikan

▶ "Causes of Apparent Discrepancies and Recent Work on the Elementary Electrical Charge." *Phys. Z.*, January 1911—Millikan and Fletcher

▶ "Some Contributions to the Theory of Brownian Movements, with Experimental Applications." *Phys. Z.*, January 1911—Fletcher

▶ "The Question of Valency in Gaseous Ionization." *Phil. Mag.*, June 1911—Millikan and Fletcher.

▶ "A Verification of the Theory of Brownian Movements and a Direct Determination of the Value of *Ne* for Gaseous Ionization." *Phys. Rev.*, August 1911, and *Le Radium*, 1 July 1911—Fletcher.

It was obvious that he wanted to be the sole author on the first paper. I did not like this, but I could see no other way out, so I agreed to use the fifth paper listed above as my thesis.

As you will note from the above, I was also sole author on the third and joint author with Millikan on the second and fourth.

Thus the authorship of these papers was settled in our humble apartment about one month after Phyllis was born.

People have frequently asked me if I had bad feelings toward Millikan for not letting me be a joint author with him on this first paper, which really led to his getting the Nobel Prize. My answer has always been no. It is obvious that I was disappointed as I had done considerable work on it, and had expected to be a joint author. But Millikan was very good to me while I was at Chicago. It was through his influence that I got into the graduate school. He also found remunerative jobs for me to defray all my personal and school expenses for the last two years. Above this was the friendship created by working intimately together for more than two years. This lasted throughout our lifetime. When he wrote his memoirs shortly before he died he had probably forgotten some of these early experiences.

I graduated with a PhD in physics in 1911 *summa cum laude*. This was the first such high honor that was given to a physics student at the University of Chicago. At this graduation I was also elected an honorary member of Phi Beta Kappa. I received very warm praise from my classmates.

It was from these classmates that the rumors arose that I had been unfairly treated by Professor Millikan, and these rumors persisted at the Ryerson Physical Laboratories for many years after I left there. This is one of the reasons that I have outlined in some detail my connection and contribution to the famous oil drop experiment. □