

character was in some ways rather anomalous. He was a mathematician, a very good mathematician, who yet liked his theories concrete. All his life he was attracted by the idea of tubes of force, Faraday's tubes of force, and always tried to ascribe to them some kind of actual physical reality. He liked something he could picture and he entirely distrusted metaphysics. He preferred the wave atom, the wave atom with the wave electron, to the Bohr atom, at least as long as the waves could be allowed to remain pictorial. He was a great experimentalist who was liable to break any apparatus he got near. He was singularly clumsy with his hands and my mother, who was good at that kind of thing, never dreamed of allowing him to knock a nail in.

He had most of the actual preparing of the experiments done by his personal assistant Everett; my father just took the readings, which very often took the form of examining a photographic record, for example of positive rays, which he would measure. But he had an uncanny power of diagnosing the reasons why apparatus, his own or other people's, would not work, and suggesting what had to be done to make it work. He was a man who was normally silent, but he was a witty

and amusing host at any sort of party, including the daily teas held in his room in the Cavendish, which he introduced. He loved flowers, wild and cultivated, and knew a very great deal about them, but he seldom gardened. He was fond of watching cricket, tennis, and football, and could recall the names and achievements of most of the leading people at Cambridge for the last 30 or 40 years in those sports. But in fact he had played little himself. He was a man of exceptionally wide sympathies. He could enjoy talking to almost anybody, and had the knack of making other people talk well about their own particular subject. He founded, and these sympathies helped him to found, the first school of physics, in a modern sense, at least outside Germany, and at one time his pupils, Cavendish men, held a very large fraction of the professorships throughout the world. Though he had a strong sense of humour, physics was too important to be funny, certainly too important to be laughed at. For him the two great qualities of a physicist, the two that really mattered, were originality and enthusiasm; and though he rated originality extremely high, it was enthusiasm which stood at the top.



Electron Physics in America

By Karl K. Darrow

The address by Dr. Darrow, a physicist at Bell Telephone Laboratories and Secretary of the American Physical Society, was also an after-dinner talk at the Electron Physics Conference Banquet.

DR. MARTON said I was going to speak about the history of electron physics in America. I think you are very fortunate that he did not make this request of someone competent to fulfill it, for if he had this person might have done it; and I can imagine nothing less appropriate for this hour of the evening of a busy day and particularly after so brilliant a speech as you have just heard.

I did go so far as to try to figure out what electron physics is, and I concluded that it is all of physics except part of nuclear physics and the general laws of thermodynamics and relativity, which in principle are independent of whatever hypothesis you make about

nature. It is somewhat devastating to reflect that the blacksmith at his forge, the cook in her kitchen, and the distiller in his distillery, are all practicing electron physics; but I really see no way of making a definition which leaves them out. So I shall not cover so vast a field. I shall just tell about some of the figures in the history in the United States, beginning quite a long way back.

This year contains not only the 100th anniversary of J. J. Thomson but the 250th of Benjamin Franklin. If one were to omit mentioning Benjamin Franklin this year at any speech in Philadelphia, one would be considered to have committed a crime—the crime of *lèse-*

Franklin, much more serious than that of *lèse-majesté*; and as I don't want to stay away from Philadelphia for the rest of my days, I will avoid it. One of the most remarkable things about Franklin, I think, was the way in which he financed his experiments. He did not go to the National Science Foundation, or the Atomic Energy Commission, or the Office of Naval Research, or the Office of Aerodynamic Research, or the Office of Ordnance Research. He couldn't because they are all in Washington—and Washington didn't exist. He didn't get his money from an endowed university either, nor from the taxpayers.

He came to Philadelphia as a young and penniless boy. He started a printing business. At the age of 42 it had thrived so well that he was able to sell it for a competence and he retired, and in his own words he said he was going to devote himself to reading, to study, to performing experiments, and to discussion with ingenious people. This program he continued for four or five years, and then he got swept up into politics and finally into statesmanship, and that is why he is greatly remembered now. But the work of those few years really constitutes some sort of an epic in the history of electrical science.

I pass over Joseph Henry just with the mention of his name. He was a man who when he got anywhere was likely to find that Faraday had just got there before him. But he did get to self-induction first.

I pass over a young man named Hall because I am going to speak of him later. Next I recall to you the very famous man who discovered the thermionic effect and left it for others to explore. This was Thomas A. Edison. He had developed the old-fashioned lamp with a carbon filament of the shape of a hairpin. The inside of the lamp grew black with sublimated carbon, and Edison noticed that there were white lines where the glass was shadowed from one leg of the filament by the other leg. He thought that the evaporating carbon atoms might be charged, so he made a tube with an auxiliary electrode to attract them. When the auxiliary electrode was positive it drew a current, when it was negative it drew none; but the blackening was unaffected.* At this juncture Edison turned his attention to something else, I do not know what. He had made enough of an impression on people's minds so that for a while the thermionic effect was called by some the "Edison effect", but this usage has died out. It is interesting to speculate on what might have happened if Edison had had the training of a physicist. Actually, he had no academic training at all.

Next I introduce you to the first President of the American Physical Society. You have heard of him as the perfecter of the diffraction grating and as the man who discovered the magnetic field of a convection current, that is to say of a moving static charge. You probably have not heard of him as the man who killed electricity. But listen to this: "It is not uncommon for electricians to be asked whether or not science has yet determined the nature of electricity, and we often find difficulty in answering the question. When it comes from

a student of science, anxious and able to bear the truth, we can now answer with certainty that electricity no longer exists, for the name electricity as used up to the present time signifies at once that a substance is meant, and there is nothing more certain than that electricity is not a substance." This is something that H. A. Rowland published in 1895. Now, of course, one could get all tangled up in semantic discussions as to the meaning of the word "substance"; but from the context, which I haven't brought along, I deduce that Rowland believed in an ether and in tubes of force in an ether, but he thought electricity was just a name for the ends of the tubes of force—no more significant than it would be to have a name for the end of the rainbow where it is supposed that a pot of gold is to be found; and he didn't want people to put any faith or belief in the existence of anything real or substantial at the ends of the tubes of force. This at least is all that I can make of it, and the coincidence of dates is such as to suggest that Sir George's father might have read this and might have set out with exemplary skill and success to prove our Rowland wrong. But I have no evidence to sustain this idea and unless Sir George has some, I think we must just give it up as one of those things that ought to be true but isn't.

Now I will go on to someone whom I do remember, and that is Millikan. The last elementary course that he ever gave at the University of Chicago was also the first that I ever took; and consequently in this sense my career begins where Millikan's teaching career ended, though of Millikan's research career there was still a full forty years to go. It occurred to me the other day that I could still remember the value that he published for the charge of the electron. Now this is not so trivial a fact, I believe, as it appears. To me it suggests, and I believe, that 30, 40, or 45 years ago nine-tenths of all the physicists in this country knew that Millikan had measured the charge of the electron as 4.774×10^{-10} , so that nine-tenths of them if they had heard a new value given for the electron charge would have had a standard of comparison for it, and if the new value had been 4.25 they would have felt there must be something queer, and that if the value was 4.77 that it must be quite right. I doubt whether this can be said now or can ever be said again of such things as the value of h , the value of k , or the value of the mass of the tau meson. My impression is that if anyone were to give a new value for any one of these quantities, practically all of you would have to look up the old values to see how the new value agreed with them. This is partly, but not exclusively, because the numerical values of things like h/e are now given to seven significant figures; it is also because physics has become too much compartmented. This in turn made me think what a towering figure Millikan was, say 30 years ago; a figure such as can hardly be imagined by the young generation because now it is rare for a man

* This is the story as graciously provided to me by Mr. N. R. Speiden, from the files of the laboratory of Thomas A. Edison.

to tower unless he be Enrico Fermi or Niels Bohr. Most solid-state physicists don't know the eminent nuclear physicists except by hearsay and *vice versa*. But 30 years ago this was not yet the case. Neither was Millikan so restricted as many of our contemporaries have to be, for his range of research extended (not to speak of his thesis which was on something having to do with textiles) over the measurement of the electron charge, over the photoelectric effect, over spectroscopy of ionized atoms, and finally for some 25 years over the cosmic rays; so that he was electron physicist and nuclear physicist at once—something not easy to become nowadays.

He was a man of tremendous energy, one of these lucky people who live on the short-sleep basis and can sleep five or six hours a night and work the remaining hours of the night and day; and one of the few, in fact I think of only one other, who have succeeded in combining the career of research physicist with the career of university president. That is a very little to say about a very great man, but perhaps it is worth saying. And another thing that I recall about him is that he joined for about three years in the crusade of J. J. Thomson to revise the terminology and make "electron" mean the value of the unit electric charge and "corpuscle" mean what we now call electron. It is evident that they didn't succeed, and evident also that any enterprise in which J. J. Thomson and R. A. Millikan together failed was an enterprise in which no one could succeed. But if that terminology had persisted, then I suppose your father, Sir George, as discoverer of the electron in that sense, would also have been the discoverer of all the mesons.

Millikan was also associated, sometimes slightly, sometimes closely, with several of the other figures who ought to be mentioned. Davisson, for example, was one of his early students, but only as an undergraduate and briefly, so that it would not be reasonable to connect Davisson's work with Millikan's. Davisson, as you know, shared the Nobel Prize with Sir George, for the experimental verification of what we loosely call the wave nature of the electron. Lately I heard Sir George relate the story of his discovery; and I was impressed by the difference between the two. For Sir George was acquainted with the work of Louis de Broglie, and he was looking for what he found. With Davisson the phenomenon came first and the interpretation came after.

It was just a fortunate chance that he had taken up the study of the reflection of very slow electrons from metal surfaces, for it was in the course of this study that he discovered that the reflected electrons grouped themselves into clearly-defined beams. Accident played a dramatic part. Davisson's first observations were made upon polycrystalline masses of metal; then one day the tube broke and the target got oxidized, and in the course of the prolonged heating necessary to undo the harm, the metal was changed from an aggregate of a large number of small crystals to an aggregate of a few large crystals. The system of beams was radically changed. Davisson trained the incident electrons against

the surface of a large single crystal, and the key was in his hand.

I think it probable that no discovery has ever been made simultaneously in two such different ways as this discovery; the one with very slow electrons, the other with fast; the one with an analogue of the Laue method, the other with the Debye-Scherrer-Hull powder method. It was Sir George's method that had the flat-tery of speedy imitation and application; whereas Davisson's method has been cultivated by very few, Farnsworth at Providence, one or two elsewhere in the world, and otherwise remains in the state where he left it.

Another person with whom Millikan was intimately associated and this time definitely in the role of teacher to pupil was the discoverer of the twin, or I guess I should say the anti-twin, of the negative electron. This was superficially like the discovery of the anti-proton which has just made the headlines, but only superficially, for the anti-proton was the object of a long and tenacious search achieved finally only by new instrumentation, whereas the positive electron just dropped out of nowhere into Anderson's bag. This is another instance of a discovery being made quite independently and almost simultaneously in Britain and America, and just the hazard of chance determining the order, and the rectitude of the Nobel Committee distributing the Prize evenly between the two. At this point, I mention something else pertaining to the electron. This is the phenomenon loosely called paramagnetic resonance and better named electron spin resonance: the turning over of an electron in a strong magnetic field by an applied radio frequency field. You will find this credited everywhere to a Russian named Zavoisky; and after naming Zavoisky, some but by no means all writers will go on to say that the next to publish the phenomenon were David E. Halliday of Pittsburgh and his collaborators. But this also was a case of independent and nearly-simultaneous discovery, though Halliday was too modest to make his claim.

Now I turn back to E. H. Hall. Hall was a remarkable figure and there are remarkable features about his story. For instance, he was still a graduate student when he sought and found an effect of such importance that within a few years it became widely known and it took its name from him, so that such terms as "Hall effect" and "Hall EMF" and "Hall voltage" are now part of the everyday language of physicists. I feel sure that there must be other such cases, but I cannot think of any; perhaps someone else can.* Hall thus made his discovery while he was very young, so that he lived long to enjoy its fruits and also to experience the ludicrous event of which L. Brillouin has told me. He went as an honored guest to a Solvay Congress held after World War I—the date, it seems to me, was 1924—and person after person came up to him, each

* Someone else could and did, and I have verified it at first hand. The contributions made by E. U. Condon to the "Franck-Condon principle" important in molecular spectroscopy were made while he was still a graduate student.



Robert A. Millikan



C. J. Davisson



Edwin H. Hall

asking, "Are you related to the old Hall?" and getting the reply, "I *am* the old Hall". Evidently these inquirers thought that such a discovery could have been made only by a man already middle-aged.

Another remarkable thing about Hall was this. Sometimes a man makes a discovery while looking for something else, sometimes he makes one while looking for nothing in particular, and sometimes he makes one by verifying some great man's theory. Hall however made his discovery by defying a great man's theory—a very great man's theory, that of none other than James Clerk Maxwell. Maxwell in his *Treatise on Electricity and Magnetism* said that what we now call the Hall effect could not occur. I have indeed heard people say that Maxwell's words can be construed otherwise, but this is of no moment, for they were construed as I have described by Hall himself and also by the young Oliver Lodge, who started and then gave up an experiment of which you can read the account in a speech that Sir Oliver delivered when he was an old man—you will find it in the section of that speech which he entitled "How I Failed to Discover the Hall Effect". Hall rushed in where others feared to tread, or rather, where others thought it useless to tread: and he got his reward.

All the stranger is it therefore, that having taken this great and courageous step and taken it with success, Hall did not take the next one. It is very easy (once somebody shows you how) to derive an equation which gives you the speed of the flowing charge, or in more modern language the mobility of the carriers, in terms of the Hall EMF and other measurable things. This seems but a small step onward, and yet it was not Hall who took it. It was another man equally young and destined to even greater subsequent fame—the Austrian, Ludwig Boltzmann. There is another equation, or really the same one transformed just a little, that enables you to go from the measured Hall

EMF to the density of the flowing charge. This is indeed a small step, but Boltzmann himself did not make it, not at least in his first paper on the subject: I do not know who made it first. One hates to think how difficult it would be to analyze convincingly the behavior of semiconductors, were it not for the Hall effect and for these equations that lead from it to the density and the mobility of the flowing charges. Hall laid the groundwork, but others found the equations. On the other hand Hall did clearly see that the sign of his effect gives the sign of the preponderating carriers, and since he observed in some metals the sign appropriate to flowing positive charge, he has something of a case for being regarded as the discoverer of conduction by holes.

I admit that I can scarcely claim that Hall was the discoverer of holes. He couldn't have formed the concept of holes, for this is derived from the concept of electrons, and since Hall made his discovery before 1880 he didn't even have the concept of electrons. I cannot claim that the first to publish the concept of holes were Americans, nor that all of the important discoveries in the semiconductor field were made in the United States. Yet I think that we do not vaunt ourselves unduly if we say that quite a big share—well over half—of the work on semiconductors published since World War II was done in American laboratories. Most especially is this true of work on germanium and silicon, those elements that almost seem to have been designed by Nature for giving vivid demonstrations of simple and clearcut ideas regarding conduction by electrons and conduction by holes. I remember well a time when metals were considered simple and intelligible, semiconductors odd and mystifying; now the situation is almost reversed, and I think that if I were trying to lead a group of beginners into the lore of conduction, I should commence with germanium and silicon both pure and impure, and go over to the metals at the end.

I think that in this hypothetical case I should find it hard to explain why for so long a time physicists assumed that all of the flowing charge in a conductor must be of one sign either positive or negative, and did not take into account the possibility that now is seen to be often a fact—the possibility that charges of both signs are flowing at once. Perhaps this was due to the discredit in which the two-fluid theory languished for so long after the one-fluid theory was accepted. Also I am sure that I should find it hard to produce a simple explanation of holes. The articles that have been written on this subject have often reminded me of something that appeared in *The New York Times* some thirty years ago, at the time of the spate of popular books about relativity. Simeon Strunsky, then of the staff of *The Times*, wrote a column about it. I no longer remember Strunsky's exact words, but I can paraphrase them nearly enough. In effect he said, "All of these books have one feature in common. They are all very lucid and fascinating until just before they get to the point, and then all of a sudden they become unintelligible." To my ears this sounds sadly like the explanations of holes that I have read. Nevertheless the language of holes and electrons, the language of bands and forbidden gaps, excitations and impurity-levels—this has turned out to be quite a useful language for describing vast numbers of phenomena, and indeed phenomena in more than one field, and indeed phenomena outside of physics altogether. Let me give a couple of examples.

First, here is the example of photoconductivity in an insulator. You have a great crowd of electrons which are fitted together and compensate one another in such a way that even though they are right there inside the insulator, the outer world doesn't know anything about them and they don't know anything about the outer world. You may consider them as being all paired off

and holding little conversations tête-à-tête, the world forgetting, by the world forgot. They are said to form a valence-band, also known as a filled band. Now comes along a photon and expels one of these electrons out of the valence-band and into the conductivity-band. The evicted electron has to go to work, and so do all of the other electrons have to go to work, their activity being described by speaking of a hole. Some day the electron will go back into the valence-band, and things will be as they were before. The time of this return will not be decided by the exciting photon. The photon has no control over it whatsoever. It is entirely up to the electron to decide when to go back to the valence-band.

Next consider a group of people all sitting together in a dining-hall after a banquet—indeed it could be this very group right here. They constitute a filled band, in more senses of the adjective than one. They are paired off or else they are grouped into clusters of not more than eight, carrying on their conversations within their own group. They have forgotten about the outside world, and the outside world has forgotten about them. But this peace is rudely shattered when the Chairman arises and excites one of the people into the oratory-band. Then all of the nice balancing is undone, and everyone has to go to work, the speaker on the one hand and the listeners on the other. Some day the speaker will stop talking, but the time will not be decided by the exciting Chairman. It will be entirely up to the speaker to decide when to go back into the silence-band. The speaker is all too likely to make a mistake in judgment on this important matter, and in fact two such mistakes have already been made this evening. Sir George Thomson subsided too early into the silence-band, and I have stayed out of it too long. I can do nothing about the former error, but at least I can refrain from compounding the latter.

International Conference on

Quantum Interactions of the Free Electron

A summary report by Harold Mendlowitz, National Bureau of Standards

THE electron has been a bona fide member of the family of elementary particles for over a half of a century and a great deal is now known about its properties. As is usual in scientific endeavor, the more one learns about something the more one finds further questions which need to be answered. The International Conference on the Quantum Interactions of the Free Electron served somewhat as a pause to recapitulate what has been learned and understood and to reformulate the pertinent questions that we would like to have answered.

The conference was held in commemoration of the one hundredth anniversary of the birth of J. J. Thomson, the "father" of the electron. The University of Maryland, which is celebrating its centennial and sesquicentennial, acted as the host institution.

A very nice feature of the conference was that there were only nine invited comprehensive review papers, and no short ten minute papers, in order to ensure and facilitate adequate discussion and contributions from those attending. For the most part this worked out as planned, and many people were able to participate ac-