Autobiography

Albert W. Hull

In the spring of 1912, I had been teaching physics at the Worcester Polytechnic Institute for four years, and was beginning to worry about the future. My midnight photoelectric research had yielded one or two scientific papers, but I was aiming at such perfection that I hesitated to report more. The meeting of the American Physical Society was coming in June, and my wife persuaded me that it was time to give a paper. So I went to the meeting in New Haven, Connecticut, and there I met Dr. Irving Langmuir and Dr. William D. Coolidge, of the General Electric Research Laboratory. There followed soon an invitation to speak at the weekly Laboratory colloquium. I spent the evening with Langmuir, who told me about his space-charge experiments.

Then came an invitation to spend the summer at the Laboratory, and following that a letter from Dr. Willis R. Whitney, Director of the Laboratory, inviting me to join the staff. I hesitated, saying that I didn't think I was capable of doing anything practical. His reply was wonderful: 'I like you all the better for your hesitation. Don't worry about the practical part, that is my job.'

When I came to the Laboratory in 1914, Langmuir had discovered the law of electron space-charge, and Coolidge, following closely Langmuir's discoveries, had utilized the unique electron emission of tungsten to invent his hot-cathode 'Coolidge' X-ray tube. It was appropriate that Coolidge should make this invention, for he was one of the first in this country, while at M.I.T., to experiment with the original Crookes' X-ray tube, and he still bears the scars of the burns from those pioneer experiments.

Langmuir continued his electronic research, with his many well known brilliant contributions, the reprints of which, currently being published by the Pergamon Press, fill twelve volumes.

Coolidge continued for many years the development of his X-ray

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tube; first with the copper-backed tungsten target tube, for field use during World War I; next the oil-immersed dental tube which is standard today in all dentists' offices; then the line of highpower oil-immersed tubes, culminating in the multi-section, million volt, gas-insulated, transformer-enclosed tube which is standard high power radiographic equipment today; and finally the 100 million volt betatron.

I began working under Dr. Langmuir's direction, and soon discovered the negative resistance 'dynatron.' [1] As with Coolidge, this was a 'natural' because of my experiments at Worcester with secondary electron emission.

At this point something fortuitous happened. Sir William Bragg visited our laboratory and spoke at our colloquium, telling us about the X-ray crystal analysis work which he and his son were doing. In the discussion I asked if he had found the crystal structure of iron, which I though might be a clue to its magnetism. He might have answered, 'no, but I think we shall have it soon', and that would have ended it. But he replied, 'no, we have tried but haven't succeeded.' That was a challenge, and I decided to find the crystal structure of iron.

It was a rash decision, for I was totally unfamiliar with both X-rays and crystallography. But I had the Coolidge X-ray tube, and the new Kenotron rectifiers, which Dr. Saul Dushman of our laboratory had just developed. With these rectifiers I constructed a 100,000 volt d.c. power equipment, filtering the rectified current by a pair of condensers with an inductance between them. One of our young patent attorneys, Mr. W. G. Gartner, noticed this filter and patented it for me. Ten years later I was surprised to learn that all the manufacturers of radio receivers were licensed under my patent.

From the start I had planned to use powder for my X-ray crystal analysis, since it was common knowledge that single crystals of iron had not been produced. I visualized that all the Bragg reflections would be recorded simultaneously, and might be unscrambled.

With iron filings, which were rotated continuously in order to produce randomness, I soon had some good powder patterns. These I gave to an assistant, a very able young lady, to compare with Bragg values for the three cubic systems. She reported that none of them fitted.

The reason that I entrusted these calculations to an assistant, rather than making them myself, was that I was still 'holding on' to the dynatron project, studying applications—a lesson on what not to do,

from which I was able to profit later. One of the outstanding qualities of Dr. Langmuir's research which was partly responsible for his tremendous accomplishment, was his habit of stopping when he had made a discovery or invention, and going on to the next job, leaving applications to others.

I spent the next few months studying the X-ray spectrum of tungsten, and the law of absorption of X-rays at high frequencies.

At this point I was fortunate in having a two-week's visit from Dr. Fred E. Wright, well-known mineralogist of the Geophysical Laboratory in Washington, D.C. With his help I mounted a single crystal of 3.5 percent silicon iron, and determined its structure by Bragg reflections. It turned out to be body-centered cubic. Immediately I became suspicious about the interpretation of my iron diffraction patterns, and while riding home on my bicycle at noon I made the calculations and found that the patterns agreed perfectly with a body-centered lattice.

I proceeded then to work out the theory of powder crystal analysis—at home, nights and Sundays, for I never did any writing on 'laboratory time'—and published my paper on 'A New Method of X-Ray Crystal Analysis,' [2] in 1917; and in 1919 'A New Method of Chemical Analysis.' [3]

The First World War interrupted this work, the Research Laboratory working as a team on submarine detection.

We got very few foreign journals during the war, and when it was over I was surprised to learn that Debye and Scherrer, in Switzerland, had independently discovered the X-ray powder method of crystal analysis, and had published it nearly a year ahead of me. Hence it is very properly known as the Debye-Scherrer method, although Sir William Bragg, with true Anglo-Saxon loyalty, continued for some years to call it the Hull method.

At the end of the war I went back to X-ray crystal analysis, and soon had analysed nearly all the common metals. [4,5] Dr. Wyckoff very properly characterized my analyses as non-rigorous, for I am no crystallographer. But I believe that all my results are correct.

In the meantime, Dr. Wheeler P. Davey of our laboratory had suggested the use of logarithmic plots for the solution of cubic, hexagonal and tetragonal structures, and had superintended the construction of these plots. They were widely used. With them it was not necessary to know the axial ratio of the crystal; one simply marked off on a strip of paper the positions of all the lines in the experimental diffraction pattern, to the same scale as the plots, and moved the strip

over the plot until an exact correspondence was found for *all* the lines. (Some experimental lines might be lacking but there could be no extra ones, unless the sample contained an impurity.

At the time these plots were finished I had just obtained diffraction patterns of zinc and cadmium, but had not analysed them. With the plots the correct axial ratio, quite different from the published metallurgical value, was found quickly and easily and was routine. The use of these two examples, as illustrations of the use of the charts, was responsible for my writing our joint paper as senior author, which I later regretted out of consideration for Dr. Davey.

I might have gone on analysing more structures indefinitely, but I sensed from Dr. Whitney's attitude (he never 'directed' me) that I had gone far enough; and Langmuir advised me that I could continue for a lifetime, since there was no lack of materials to be analysed; but that there were more interesting problems in our laboratory. Therefore, I went back to electronics, and proceeded to invent the magnetron,[6] the screen-grid tube,[7] and the thyratron.[8]

I call them inventions for want of a better term; but they are so simple that they seem scarcely to deserve that austere designation.

The magnetron was simply an extension of J. J. Thomson's crossed-field calculations for electron orbits. J. J. had shown that electrons, moving from a plane cathode toward a parallel plane anode, would be bent into cycloid paths by a magnetic field parallel to the planes, and would fail to reach the anode. But of course this analysis ceased to apply when they reached the edge of the plates, and they all went to the anode.

I was curious about what would happen if the plates were bent into cylinders, so that there was no edge. The electrons might be permanently prevented from reaching the anode. I tried it soon after coming to the Laboratory, but failed. Presumably I was not sufficiently careful about vacuum and symmetry.

After deciding to terminate crystal analysis work, I came back to the 'magnetron' problem, and made the calculation of the paths of the electrons. It proved to be quite simple. It showed that they really could not reach the anode below a certain critical voltage. After this calculation the experimental proof was easy.

The screen-grid tube was a by-product. Our radio department had appealed to the Laboratory for an explanation of the 'noise' in their super-heterodyne receivers. Langmuir and I discussed it, and he suggested that it might be the 'shot-effect' of the individual impacts of electrons on the anode, which had been predicted by Schottky. I decided to measure the shot-effect.

The obvious method was straight radio frequency amplification. A voltage amplification of 100,000 fold was required at a frequency of one megacycle. I knew that the maximum total amplification that had ever been obtained by series triodes was about 200 fold; but I also know that the reason for the limitation was 'feedback' due to the capacity-coupling of plate and grid.

Faced with the problem, it seemed obvious that this coupling could be completely eliminated by thorough screening of the grid from the plate, both internally and externally. We had excellent construction facilities in the laboratory, and such 'screen-grid' tubes were quickly made. Of course they were completely successful.

I still can't understand how such a simple solution of the feed-back problem had eluded engineers for ten years—including myself, for I had worked on the problem and had constructed special tubes in an effort to *reduce* the capacity.

The principle that made possible the 'hot-cathode thyratron' was a chance observation of a very simple kind. I was studying the characteristics of a discharge from a 'thoriated' tungsten filament to a concentric anode in low pressure argon, and my assistant, Mr. W. F. Winter, showed me an unusual volt-ampere characteristic. As the anode voltage was increased the current increased quickly to a maximum at 20 volts, and then decreased rapidly to almost zero at 100 volts. I recognized at once that the bombardment of the filament by argon ions was knocking off the thorium atoms, which gave the filament its large electron emission, faster than new ones could diffuse to the surface. That was to be expected. But I also noticed the important fact that the thorium was *not* knocked off below 20 volts.

Such a simple observation was the solution of a problem that had bothered engineers, including myself, for more than 10 years; namely that when low pressure gases were introduced into hot-cathode rectifiers, the filament coatings were completely stripped off in from 10 to 100 hours, and even pure tungsten filaments were reduced in diameter sufficiently to materially change their resistance. This problem was completely solved by the specification of a 'disintegration voltage' of 20 volts, above which the anode voltage should not be allowed to rise. This simple precaution made possible the development of hot-cathode gas rectifiers and thyratrons that followed.

My only other major research problem was an analysis of the stresses in glass-metal seals. Large seals could be made only with very thin copper, which yielded to the stresses before they broke the glass.

With the help of Mr. E. E. Burger, and analytical assistance from

Dr. Hillel Poritsky, I made a thorough study of these stresses, including methods of measuring them; and developed a special alloy called Fernico which made seals to a special glass, developed by Dr. Louis Navias of our Laboratory, that were completely stress-free. The fact that a somewhat similar alloy had been discovered by Westinghouse scientists some two years earlier, so that we now buy our Fernico from Westinghouse, is relatively unimportant. Our analysis, and the development of Fernico, made possible the large power rectifiers and thyratrons of today, including Coolidge's multi-section X-ray tube. This 2 million volt X-ray tube has 20 metal sections, separated by glass sections; and the metal is Fernico.

References

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