PART VII

Personal Reminiscences

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Some Personal Reminiscences

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After being at school at St. Dunstan's College I went, as a youth of seventeen, to University College, London, where I studied physics under F. T. Trouton. His name is known to all physical chemists from Trouton's Rule, giving a relation between latent heat, molecular weight and boiling point, which he discovered when a student as the result of an afternoon's manipulation of physical constants. He carried out, as a collaborator with that genius G. F. FitzGerald, fundamental experiments on electromagnetic waves just after their discovery by Heinrich Hertz, and was subsequently much occupied with experimental attempts to establish the ether drift. The negative results of these experiments helped to pave the way for the first theory of relativity. Trouton was a charming, sympathetic and humorous Irishman, with a great love for physics. When, at the age of nineteen, I took my B.Sc. with first-class honours in physics, he, with great kindness, encouraged me to do research and I carried out my first work on the creep of metals in his laboratory, work which established the $t^{1/3}$ law.

In 1910 I was awarded an 1851 Exhibition scholarship and went with it to Heidelberg, as I wanted to work in a German university and was attracted by the name of Philipp Lenard, Professor of Physics there, who had been awarded the Nobel Prize in 1905 for his fundamental work on the electron. I had hoped to carry out research on electron vacuum physics, but Lenard decided that I should work on the electrical properties of flames, in which he was particularly interested at the time. Accordingly I settled down to this subject and duly obtained my Ph. D. (summa cum laude) at the end of 1911, when Lenard, who had apparently, from his conduct at the time and after, taken a liking for me, offered me a post in the laboratory. This I declined, probably wisely. Lenard was an experimental physicist of the highest distinction, undoubtedly a genius, and he had treated me with great kindness and wrote me many friendly and flattering letters after I left Heidelberg. He was, however, a difficult man to work under, who failed to awaken the independence and sympathy of those working beneath his rule, as is shown by the fact that, with all his qualities and in spite of the fact that some brilliant Germans worked in his laboratory, he did not found a great school of physics. As Carl Ramsauer, who was closely associated with him, wrote 'Als Doktorvater hatte Lenard keine glückliche Hand'.

At Heidelberg I made some close friends with whom I remained on intimate terms until, alas, they all eventually died. Prominent among them were Wilhelm Hausser, Walther Kossel and Carl Ramsauer. The two first named took their doctorates in the same academic year as I took mine, Ramsauer was some years older and was already a Privatdozent when I came to know him. Hausser carried out fundamental work on the production of *Lichterythem* (sunburn) by ultraviolet light. After the First World War he became director of research at the Siemens and Halske works in Berlin, where he not only promoted the application of electron physics to practical ends, but played a great part in the discovery of rhenium and masurium,* for which X-ray crystal spectroscopy was essential. With typical modesty he withheld his name from the discovery. He died in 1933. Walther Kossel achieved fame for his work in explaining the excitation and absorption of the different series of X-ray lines, for making clear the relation between X-ray and optical spectra and for other important work in physics. He died in 1956. Carl Ramsauer, whose name is perpetuated in the Ramsauer effect and who carried out other important work on the passage of electrons through matter and on the ionization produced by ultraviolet light, became director of the great research institute of the Allgemeine Elektrizitätsgesellschaft. He died in 1955. All three were men of outstanding personality. From Hausser and Ramsauer I learnt much of German student lore, including the legends of Bonifacius Kiesewetter. From Ramsauer I learnt to admire and revel in Goethe's *Faust*, in which I have since continued to take delight. I hope that present day research students find such company. We spent many merry evenings together and worked hard in the laboratory.

After quitting Heidelberg I went to the Cavendish Laboratory at Cambridge, directed by the world-famous J. J. Thomson, as what was called an 'advanced student', a man who was allowed to work in a research laboratory but had no status whatever. There went up with

* Masurium was the name assigned to the element of atomic number 43 when its discovery was claimed. This element is now called technetium.

me at the same time my friend S. E. Sheppard, the famous photochemist, who was already a doctor of the Sorbonne. Needless to say, our degrees were not recognized and we were neither of us called doctor. I remember saying to Sheppard that the University of Heidelberg, having been founded in 1385, later than the oldest Cambridge colleges, was perhaps unworthy of recognition, but I thought that the Sorbonne, founded in 1256 and famous before Cambridge was heard of, might be accepted as a centre of learning. Having no recognized degrees we both had to ask permission of a tutor, unheard of outside Cambridge, if we wanted to be out after 10 o'clock at night, as we occasionally did. No doubt it was with this kind of thing in mind, although his status was higher than ours, that the physicist H. R. Robinson later remarked that he was not a Cambridge man, but had been an out-patient there for a year or two.

Finding that one had to work for a few years at Cambridge before one had the status that entitled one to apparatus, I returned to University College, London, while Sheppard at the same time went to the Kodak research laboratories in the U.S.A., where he made capital discoveries in photochemistry. I continued my work on the creep of metals and accidentally made the first single crystal metal wires. Then an opportunity arose for me to go to Manchester to work with Rutherford as John Harling research fellow. I embraced it with enthusiasm and accordingly started in Rutherford's laboratory in the autumn of 1913.

The analysis of the wave-lengths of X-rays by means of crystals had been initiated in the previous year by von Laue and the Braggs, and Rutherford chose me to collaborate with him in determining the wave-lengths of the gamma rays of radium. For this we used crystals of rocksalt, in the first place a rather dirty little slab of which the surfaces, although cleavage surfaces, were not very plane. With this we obtained lines in positions not to be anticipated with a perfect crystal, as everyone can well understand today. As was stated in our first paper (*Phil. Mag. 27*, 854, 1914) 'All our photographs showed similar peculiarities, but the outside lines which appear are very variable for different angles of the crystal. This behaviour of rocksalt led us to make many fruitless experiments to obtain a more perfect crystal, but all the crystals of rocksalt we have examined showed similar imperfections, though in varying degree.' The attempts to obtain more perfect crystals included a descent which I made into a salt mine in Cheshire. I well remember being lowered in a kind of great bucket into hacked-out corridors and halls with glistening walls of salt. I collected many pieces of salt with, apparently, beautiful smooth surfaces, and similar pieces in surface pools, but, as can well be understood today, they did not give any better results than our original little slab—in fact, if my recollection is correct, not such good results. C. G. Darwin, who was working in the laboratory at the time, wrote a classical paper on the theory of X-ray reflection in which the effect of imperfections of crystals was for the first time considered.

In this work with Rutherford the first divergent-beam transmission photographs, showing deficiency lines, were observed by passing the gamma rays through a slab of rocksalt.

I have been asked recently if, when we decided to try this method, we anticipated the dark and light lines which appeared. My recollection of those distant days is that we certainly expected the dark lines caused by the direct reflection and that it was to obtain these that the experiment was planned, but that we had overlooked the fact that this reflection must cause deficiency lines. As soon as we saw the white lines, however, we naturally recognized their origin.

Working in Rutherford's laboratory in 1913 was H. G. J. Moseley, who was one of the first to obtain results of fundamental importance with the newly-born method of X-ray analysis. With reflection by a crystal of potassium ferrocyanide he measured the wave-lengths of the K- and L-series of the elements and established the basic significance of the atomic number. Everybody knows this, but not everybody knows how definite Moseley was on the point, so I quote words that he used. 'We have here a proof that there is in the atom a fundamental quantity, which increases by regular steps as we pass from one element to the next. This quantity can only be the charge on the central positive nucleus, of the existence of which we already have definite proof.'

Moseley was an outstanding character, who would undoubtedly have accomplished further great things had he not been killed in the First World War. Others working in the laboratory when I went there were Ernest Marsden, Charles Darwin, Walter Makower, H. R. Robinson, E. J. Evans, D. C. H. Florance, Stanislaw Loria and Bohdan de Szyszkowski. Niels Bohr was not working there at the time, but was a visitor. Life was good fun—and hard work—in Rutherford's laboratory in those days. I occasionally did a bit of boxing with the undergraduate champion of my weight. I had won the (London) University College and Hospital light weight championship in 1910.

In the summer of 1914 I went to pass a few weeks in Sweden at the invitation of Svante Arrhenius, a great man of most kindly, amusing but incisive character. I had every intention of continuing to work in

Rutherford's laboratory, where my Fellowship was open to me. One of the things on which I intended to work was the investigation of the structures of my single-crystal metal wires by the new X-ray method. The outbreak of war, which took place while I was in Sweden, put a stop to all such projects. On my return to England I became a very junior artillery officer, spent two and a half years at the front in France and the rest of the war on trivial 'scientific' tasks supposed to be of significance for the conduct of the war.

At the end of the war, having recently married, I had to find a post which would enable me to keep a family and became Professor of Physics at the Artillery College, a staff college for artillery officers destined for technical posts, which later took the name of the Military College of Science. Here there was little opportunity for research. I wrote a book on *The Structure of he Atom*, which I dedicated to Rutherford. This came out in 1923 and was widely read at the time: a third much revised edition appeared in 1927. In this period I also published a volume of poems. In 1923 I visited Germany and had the great pleasure of staying with my old friend Wilhelm Hausser; we had long talks together of our old Heidelberg days. This was the period of the collapse of the value of the German mark and it was a strange experience to see the expense of posting a letter to England go up ten times, in German marks, in a day or two. Luckily I had English money with me, for which I could get anything I liked. Hausser and I met again at the Physikertag in Danzig in 1925. Being in Danzig at that time was another queer experience. I last saw Hausser when I visited Heidelberg in 1931. He foresaw already the rise to power of Hitler and told me what a disaster it would be. At the time I did not understand, as he did, the seriousness of the situation.

In 1928 I was appointed Professor of Physics at University College, London, the post carrying the title of Quain Professor of Physics in the University of London. The laboratories were very poorly equipped and funds were scanty, so that there was no question of experimental research requiring elaborate apparatus. I and my students worked on problems in sound, on the viscosity of liquids and on the mechanical properties of metals, devoting much attention to the preparation and behaviour of single crystals of metals. In sound I was able to investigate and explain the mechanism of the sensitive flame, the behaviour of small particles under the influence of air vibrations and to deal with other classical problems. In the field of viscosity I put forward the formula for the variation of the viscosity of a liquid with temperature which is generally accepted and with my collaborators measured, by special methods, the viscosity of certain molten metals, as being the simplest liquids. With Dr. Cyril Dodd I established the influence of an electric field on the viscosity of liquids, about which there had been considerable confusion. But these and other such matters have nothing to do with the X-ray diffraction in crystals.

In our work on metals, however, particularly that on metal single crystals, we naturally made considerable use of X-ray diffraction. The break-up of asterism which occurs with strained sodium and potassium single crystals was discovered and discussed by Dr. Tsien and myself. In the work of Dr. C. Henderson and myself on single crystals of face-centred cubic metals, in the course of which 'easy glide', a term now generally accepted, was established, much use was made of back reflected X-rays, asterism being again much in question. In fact it is natural that in all our investigations of the deformation of metals, whether single crystals, as in the work carried out with Dr. Aboav on copper single crystals, or polycrystalline metals, as in the work which I carried out with Dr. Jolliffe on the flow of metals under simple shear, the X-ray method is indispensable in studying the behaviour of the glide planes and the distortion of the crystals, or crystal grains, in general. I am glad to say that my collaboration with Dr. Jolliffe on the problems of metal flow continues.

It is strange to look back over fifty years to the days when von Laue made his great discovery and the Braggs, Moseley and Darwin carried out their fundamental work. In those days the physics research laboratory was directed by men in close contact with all their research students, men who gave all their time and thought to the problems of their choice. The students were mostly enthusiasts, for there was practically no career except the academic one open to them. Money was scarce: Rutherford, H. R. Robinson has written, ran his laboratory on about £400 a year, the equivalent of which, even allowing amply for the change in the value of money, can easily be spent on one student, finding out nothing in particular, in these days. Everything has changed—has, it need hardly be said, immensely improved. Possibly, however, to work in a research laboratory under one of the great leaders of the time, was a more exciting adventure, a gayer and more satisfying life, than to labour seven hours a day in one of the splendidly equipped and highly organized discovery factories of today.

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