Personal Reminiscences

W. L. Bragg

I lived for the first eighteen years of my life in Adelaide, South Australia. My father had come to Adelaide in 1886 to succeed Sir Horace Lamb as Professor of Physics and Mathematics at Adelaide University; three years later he married my mother, who was the daughter of Sir Charles Todd, Postmaster General and Government Astronomer of South Australia. My parents sent me to St. Peter's College, a fine Adelaide school, and afterwards to Adelaide University. School education was then much less specialized than it is now. I had instruction in eight subjects to an equal standard, in fact in all the subjects which were taught at the school with the exception of German and Physics—and I have always regretted that I did not learn German. I did not have an entirely happy time at school, though no fault of the school. I was no good at games, not for physical reasons but because I had not the right temperament, lacking the drive and self-assurance necessary to a good games player. Being rather advanced in my school work I was a very young member of the sixth form, while at the same time making such a poor showing in games that I could only be put to play in the 'sets' with boys in much lower forms. Schoolboys only accept the normal boy into their full fellowship and, although they regarded me with kindly tolerance as a strange freak, I was very much cut off from my fellows, and driven into finding solitary interests of my own. One was the collecting of shells along the coasts and on the reefs in the neighbourhood of Adelaide. I had quite a fine collection for a youngster and amongst other rarities I discovered a new species of cuttlefish; I still feel proud that 'Sepia Braggi' has continued to be accepted by conchologists as a distinct species.

Chemistry was well taught at my school, and the chemistry master, who took a kindly interest in me, used to let me help him to set up the class experiments. It was this, I think, which gave me my first interest in the methods of science. My brother and I, also, were always making

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things in a shed in the garden which we used as a workshop. My father's University mechanic used to give us bits of scrap and apparatus which were being discarded and we rigged up crude motors, morse sounders, a telephone system and so forth. Much of the Post Office stores was kept at the Observatory and I am afraid we sometimes raided them for some coveted piece of insulated wire or zinc or chemicals. I did not get any formal instruction in Physics till I went at the age of fifteen to Adelaide University, and then it was only as a subsidiary course to my main subject of Honours Mathematics in which I took my degree at the age of eighteen. It was at that time, however, that my father started his research career. Up to the age of 43 he had devoted himself entirely to teaching and administration and had been one of the stalwarts who almost recreated the University and made it into a fine institution. He was one of the first in Australia to experiment with a Röntgen tube and I think I was one of the first patients to be X-rayed, my elbow having been smashed in an accident at the age of six. He and my grandfather put up the first Marconi set in South Australia, transmitting messages from the Observatory to the coast five miles away, and he also installed a seismograph at the Observatory. As a boy I saw a good deal of these ventures. When I was at the University my father began his investigations of the ranges of the α-rays and used to talk to me about his results, so I lived in an inspiring scientific atmosphere.

My father was appointed to the Physics chair at Leeds University in 1908 and the family came to England. I was entered at Trinity College, Cambridge, for a course in Mathematics and at the end of my first year was awarded a Major Scholarship in that subject. I was taken ill with pneumonia just before the examination and had to take it in bed with quite a high temperature. Our beloved Master, Butler, read all the essay papers himself and particularly commented on mine, which he said showed great dash and fire—I think the temperature helped very much towards the happy outcome. In fact, I was headed towards being a mathematician till my father urged me, in my second year, to change to Physics for my final examinations.

After getting my degree at Cambridge, I started research under J. J. Thomson at the Cavendish. He set me a problem of measuring the mobility of ions in gases. The great reputation of J. J. Thomson's school had attracted many young researchers to Cambridge, and the facilities in the way of apparatus were quite unequal to the strain put upon them. I remember, for instance, that there was only one foot bellows between the forty of us for our glass-blowing which we had to carry out for ourselves, and it was very hard to get hold of it. I managed to sneak it once from the room of a young lady researcher when she was temporarily absent, and passing her room somewhat later I saw her bowed over her desk in floods of tears. I did not give the foot pump back. Then again, when I first got a reflection of X-rays from a mica sheet, I kept the induction coil running so hard in my enthusiasm that the platinum contact on the hammer-break burnt out. Lincoln, our head mechanic, was very angry and told me I must wait two months before he could get me another one because it had cost ten shillings. I have still not got used to the cheerful and airy way in which young research students nowadays ask for some item of equipment costing many thousand pounds.

My first researches into the analyses of crystals by X-rays were made at Cambridge in 1912. My father had aroused my interest in Laue's work; it was the help which I got from Professor Pope, the Professor of Chemistry, and the inspiring influence of C. T. R. Wilson, which led to my analysis of sodium chloride and potassium chloride by the method of the Laue photograph. Pope and Barlow had developed a valency-volume theory of crystal structure, and when my first studies of Laue's diffraction patterns led me to postulate that zinc sulphide was based on a face-centred cubic lattice, Pope saw in it a justification of his theory and urged me to experiment with sodium chloride and potassium chloride crystals which he got for me from Steeg and Reuter in Germany. These experiments were made in the later part of 1912 and early in 1913. Simultaneously, my father seized on the conception of the reflection of X-rays by crystal planes to design his X-ray spectrometer, and discovered the X-ray spectra. I did not start to work with my father till I came home for the long vacation in the summer of 1913. He was still principally interested in X-ray spectra, but it was immediately clear that the spectrometer provided a far more powerful method of crystal analysis. After showing its power in the analysis of the diamond structure (a research in which I played a very minor part) he continued to establish the relations between the X-ray spectra and the absorption bands, and I concentrated on the interpretation of crystal structure. We had a thrilling time together in an intense exploitation of the new fields of research which were opening up, brought to an end by the start of World War I in 1914.

I volunteered for service when the war started. We were allocated to regiments in a very haphazard way and of all things I found myself thrown into a horse artillery battery. The other officers were all hunting men, and I was very much a fish out of water, though it was an interesting experience. We trained in England for a year, but just before the battery went abroad for active service I was called to the War Office and told that I was to go to France to test a new invention called Sound Ranging for the British forces. The French army had started this method of locating the enemy guns by sound and I was first sent to the Vosges to learn its methods. A small British section was then established on Kemmel Hill just south of Ypres and we set up our base of microphones and recording headquarters. At first the method had a very poor success, though we tried to gloss this over in our reports to headquarters. The trouble was that the loud crack made by a shell travelling faster than sound muffled the fainter report of the gun we were trying to locate, when recorded by the carbon microphone we were using. Then one of the men in my section, Corporal Tucker, had the brilliant idea of using a device which later came to be known as a 'hot-wire microphone'. The report of the gun, though it produces little effect on the ear, creates a powerful pressure wave. We bored a hole in the side of an ammunition box, and stretched a fine platinum wire across it. A current heated the wire a dull red, and the report of the gun created a rush of air through the hole, cooling the wire and reducing its electrical resistance. It worked like a charm. I shall never forget the thrill when we first saw the recording apparatus in our dugout paying almost no attention to the 'shell-wave' and other incidental noises, but giving a powerful response when the real report of the gun reached it. Sound Ranging then developed rapidly and we established sections to cover the whole Western Front. It could only work in conditions of calm or easterly wind, but it gave valuable counter-battery information because the shell-burst as well as the gun report was recorded, and so we were able to establish not only the gun position but also the calibre and the target on which the battery was registering.

It was in 1915, when we were setting up our first Sound Ranging base in Belgium, that I heard of the award of the Nobel Prize to my father and myself. I remember that the genial Curé in whose house we were billeted brought up a bottle of wine from his cellar to celebrate the occasion.

When the war ended, I had a brief time in Cambridge as a lecturer, and then went to Manchester as successor to Rutherford in the Physics professorship. It was a difficult time. Not only was I inexperienced, but also the war years had made us forget all our knowledge of physics and the classes, being largely composed of men returning from war service, were by no means easy to handle. My main helper in getting the laboratory into full activity again was R. W. James. He had been the physicist in the Shackleton expedition which was on the way to the South Polar regions when war broke out. The expedition lost its ship, which was crushed in the ice, but managed to make its way to the barren Elephant Island whence Shackleton set out on his famous boat journey to South Georgia to get help. James played a crucial role in the escape of the party. They of course had no radio in those days and were quite uncertain as to their longitude, but had saved a nautical almanac; James studied this and managed to determine the time by observation of stellar occultations. This information gave them their position and enabled the boats to reach Elephant Island. Soon after his return to England, James joined our Sound Ranging section in Belgium and we worked together for the greater part of the war. When we were demobilized he joined me at Manchester and together we built up a research school.

At first we concentrated on the quantitative measurement of the efficiency of X-ray diffraction, using the formulas which C. G. Darwin had developed before the war. This led to our examining more complex crystal structures, in which the values of a number of paramaters had to be determined in order to fix the positions of the atoms. Some of our more ambitious crystal analyses were at first viewed with deep suspicion by our colleagues in other laboratories, but I think that one of the chief contributions of the 'Manchester School' was the demonstration that these quantitative measurements enabled one to tackle such structures with confidence. I can only mention a few of the men who worked with us at Manchester. Hartree came to the University as Professor of Theoretical Physics and did his famous work on atomic scattering factors. Bradley started as a chemist, but then joined our group. He developed the powder method as a means of determining the structure of metals and alloys. Bradley was unique in his management of the powder method, a brilliant researcher. The great theoretical physicists Waller, Bethe, Peierls were amongst those who came to the Manchester laboratory. That volatile genius, E. J. Williams, whose early death was such a profound loss to science, was our Reader for a number of years. I remember that when Bradley and Sykes had got some interesting results with iron aluminium alloys, and I had given an account at a colloquium in which in a vague way I ascribed their findings to an order-disorder transformation, Williams came to me next morning with practically the whole thermodynamic treatment of the problem which he had worked out on the preceding evening.

Warren and Zachariasen played a great part in the elucidation of the silicate structures.

I first met Alice Hopkinson in Cambridge just after the War, and two years later we became engaged and were married in December 1921. My wife comes from a famous Manchester family of engineers, though her father, Albert Hopkinson, broke from the family tradition by becoming a doctor. Our elder boy, Stephen, is an engineer and chief scientist to Rolls Royce in Derby. Our second boy, David, is an artist. Our elder daughter, Margaret, married Mark Heath in the Foreign Office, and our younger daughter, Patience, is married to David Thomson, the son of my closest friend, Sir George Thomson, the Master of Corpus Christi College in Cambridge. My wife was Mayor of Cambridge in 1945, is a Justice of the Peace, and has served on many public bodies, including the Royal Commission on Marriage and Divorce. She is at present Chairman of the National Marriage Guidance Council.

I left Manchester to become Director of the National Physical Laboratory in 1938, and at about the same time James went to South Africa to become Physics Professor at the Cape University. My directorship of the National Physical Laboratory only lasted for a year, because soon after my going there Rutherford died and I was invited to succeed him at the Cavendish Laboratory in Cambridge. I remained based on Cambridge during the Second World War, though for eight months I was abroad in Canada as Scientific Liaison Officer between that country and Great Britain. I acted as scientific adviser to the Admiralty on underwater detection of submarines, and was also connected with the development of Sound Ranging during the war.

The reorganization of the Cavendish Laboratory after the war was not easy. Cockcroft, my fellow Professor of Experimental Physics, was not released by the Government when the war ended, and so we had not got his help, which would have been invaluable, and at the same time could make no alternative appointment. He was finally appointed head of Harwell. We had suffered a great loss by the death of R. H. Fowler, who directed the Theoretical Physics, and several of the most important members of the staff did not come back till some time after the war ended. Large sections of the main building were still occupied by Government departments which had been installed there during the war. Eventually Hartree succeeded Fowler in Theoretical Physics, and Frisch came to direct the nuclear research. W. H. Taylor was appointed Reader and built up a flourishing school for X-ray analysis. He has been a very active participant in all our international gatherings. Perutz was working in the Cavendish on protein structure and was later joined by Kendrew, and this created the starting point for the Medical Research Council's unit for Molecular Biology which has produced such spectacular results and become world-famous. I of course had a particular interest in the protein work. For a long time we only had the most meagre success, just sufficient to encourage us to continue. The Crick and Watson solution of the DNA structure was a notable triumph coming from the research group, but the analysis of the protein diffraction patterns was a formidable problem, eventually solved by Perutz' discovery that heavy atoms could be attached to the molecule at specific points without any alteration of the crystal framework. The successful analysis of molecular structures containing many thousands of atoms is an event which has given me the very greatest pleasure, after a lifetime of research devoted to X-ray analysis.

When I came to the Royal Institution in 1954, I was faced with the problem of starting research in its Davy-Faraday Laboratory practically afresh. All costs had risen so much that the income of the laboratory was insufficient to meet even the basic running expenses, and it was drawing heavily on its research fund which was almost depleted. The Davy-Faraday is unlike a university laboratory in that there is no steady stream of young people wanting to do research after taking their degrees, so we had to find both the men and the money before we could start work. I could not have done it but for the help of Perutz and Kendrew. We agreed to pool resources between the Cambridge unit and the D.F. Laboratory, and make a joint attack on protein molecules. Perutz and Kendrew were formally appointed as Readers in the Royal Institution, and the Medical Research Council appointed me as a general adviser on the research. We got going by transferring a living part, as it were, of the Cambridge research to Albemarle Street. The Rockefeller Foundation, the National Institute of Health, the Medical Research Council, and a number of industrial firms generously financed the research. The D.F. Laboratory has participated in the analysis of myoglobin and of haemoglobin, its particular contribution, by Philips, Arndt, Green, North and others, being the automatic collection of data and programmes for the electronic computer.

The Royal Institution receives no government support. It is maintained by its endowments, the subscriptions of its members, and donations by industrial and other bodies interested in its activities. It is primarily a centre for the diffusion of scientific knowledge, in particular by popular lectures to a non-scientific audience. Founded by Rumford in 1799, it has great traditions and a famous history. Davy, Faraday, Tyndall, Dewar and W. H. Bragg lived and worked in it, and it was the home of many of the outstanding advances in science in the nineteenth century. It also has a priceless asset in its building, which provides assembly rooms and a world-famous lecture room right in the centre of London. But when I came here seven years ago, a good deal of thought had to be put into its function in the modern world. The Friday Evening Discourses and the Christmas Lectures, which had been held for a century and a half, were as successful as ever, and it was a much sought after centre for meetings by other scientific bodies. However, other activities had ceased to be effective in the changed conditions of modern times. It used to be a main centre in the country for courses of lectures dealing with the latest advances in science, but this function has now been taken over by the Universities and by societies of many kinds. The afternoon lectures were still being held seven years ago, but their usefulness had almost ceased. At the same time, finance was a major problem; one had to reconsider the function of the Royal Institution both to take full advantage of its unique advantages and to form a basis for an appeal for support. The Christmas Lectures had always been so successful that we conceived the idea of lectures of the same kind all through the year to boys and girls in Greater London, and at the same time of making the Royal Institution a place where science teachers could hear about the latest developments in their subject. Schools lectures are held twice a week, when the young people are shown experiments illustrating scientific principles. These lectures are run like the standard plays of a repertory theatre. Each is repeated a number of times so that a large number of boys and girls can attend it, and a store of the most effective demonstration apparatus is kept up for the repeat of the programme every three years, when the school population has changed. We arrange occasions when the science teachers can meet representatives of famous research schools in the country and hear about their work, and we maintain a large collection of demonstration apparatus for the teachers to study. It has proved to be a very interesting and rewarding experiment which is much appreciated by teachers and by their pupils, and I have taken a deep interest in this work. Although some 20000 young people come each year to the Royal Institution to see the experimental demonstrations, they only represent a small fraction of the potential young scientists. The schemes on a far larger scale initiated by the British Association and by other bodies must meet requirements all over the country. The Royal Institution, however, with its traditions and its setting is an ideal place for maintaining the standards of lecturing and demonstration and for exploring new techniques. It is a school for the art of lecturing, the influence of which extends far beyond the area which it mainly serves.