Personal Reminiscences

R. W. James

I was born in London on 9 January 1891. My father, who had had no formal scientific education, had nevertheless a deep and lively interest in scientific matters, particularly in natural history. I was brought up to believe that one took an interest in such things as a matter of course, and I certainly owe a very great deal to this early training. I went first to the local elementary school, then to the Polytechnic School in Regent Street, and finally to the City of London School, where I had the good fortune to be taught chemistry by Henry Durham, one of the finest teachers I have known, who was able to foster in his senior boys an enthusiastic attitude of inquiry.

From the City of London School I won a scholarship at St John's College, Cambridge, where I read for the Natural Sciences Tripos, specializing in physics. In my year there were only about a dozen taking the second part, one of them being W. L. Bragg. Our practical work was supervised by C. T. R. Wilson, who expected from us the standards of accuracy and patience he had set for himself, and if we fell far short of this ideal, the attempt to reach it was good training.

I took my degree in the summer of 1912, and started work in the Cavendish Laboratory under Professor Sir J. J. Thomson, very shortly after the publication of the paper announcing the discovery at Munich of the diffraction of X-rays by crystals by Friedrich, Knipping and v. Laue. My own attention was first drawn to this by an account given by J. J. Thomson to the research students' colloquium, and I think I am correct in saying that W. L. Bragg was also present on this occasion. Not very long afterwards his first paper, embodying the reflection idea, appeared in the *Transactions of the Cambridge Philosophical Society*. I was very interested in diffraction problems, and had been stimulated by C. T. R. Wilson's lectures on the subject for the second part of the Tripos, which, if they left something to be desired in their delivery, yet formed an excellent basis for a serious student to build on; but I had

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already started experiments in a different field and did no work on diffraction at Cambridge.

In the summer of 1914, just before the outbreak of war, I had joined Sir Ernest Shackleton's expedition to the Antarctic as physicist, and sailed in the *Endurance*. The ship was ultimately crushed in the ice and sank, and the party were fortunate to reach land in small boats, so that much less scientific work was possible than had been hoped for. My own work consisted largely of magnetic observations, and of work on the movement and physics of the pack-ice done in conjunction with J. M. (now Sir James) Wordie.

On my return to England at the end of 1916 I joined the army, and early in 1917 went to France to do sound-ranging; and there I renewed contact with W. L. Bragg, who was in charge of the technical side of its development. There too I met for the first time C. G. Darwin and C. H. Bosanquet, both of whom were sound-rangers. When in 1919 Bragg succeeded Rutherford he offered me a post in his department, and it was then that I started to work on the diffraction of X-rays.

While still in the army, but knowing that I was going to Manchester, I had bought a copy of the first book by W. H. and W. L. Bragg, *X-rays and Crystal Structure*, and I have still a vivid recollection o- starting to read it one afternoon, and becoming so completely imf mersed in it that I appeared at Mess that evening an hour late. I think my Colonel found my excuse a little difficult to understand; but the first reading of that book was one of my really exciting scientific experiences.

Although my first two papers were on the structure of antimony and bismuth, both very inadequate by modern standards, although they gave the correct structure, my main interest in Manchester was not in structure determination, but in the physical and optical basis of the subject. I worked for a time with Bragg and Bosanquet on the absolute intensities of the spectra from rocksalt, on secondary extinction, and on the measurement of the atomic scattering factors for sodium and chlorine, work that is discussed elsewhere in this volume. I was particularly impressed with the importance of the scattering factor as an aid in structure determination, and used it in an analysis of the structure of barytes. Then, in 1925, I started to make more accurate observations of the effect of temperature on the intensities of X-ray spectra, taking measurements from the temperature of liquid air up to about 600°C with rocksalt, and later extending the low-temperature measurements to aluminium and potassium chloride. In this work I was associated with Miss E. M. Firth, R. G. Wood, and G. W.

Brindley. In collaboration with D. R. Hartree and I. Waller we were able to confirm experimentally the existence of zero-point energy in the thermal vibrations of the rocksalt lattice, and to estimate their amplitude.

The work on rocksalt was communicated to the Royal Society in 1927, and I was asked to read the papers. I have never forgotten the way in which Lord Rutherford, then President, put me at my ease before the meeting. Taking me by the arm, he said 'You haven't read a paper here before, have you? Do you mind if I give you some advice?' I said I should be most grateful if he would do so. 'Well' he said, pointing at the same time to the presidential throne, 'For Heaven's sake don't be too difficult. If you knew what I have to put up with, sitting in that Chair!' Very typical of Rutherford, entirely human, and very sensible.

In 1931 and 1932 I spent about ten months in Debye's laboratory in Leipzig, working on the influence of temperature on the scattering of X-rays from silicon-tetrachloride vapour. Debye encouraged me to tackle the problem theoretically at the same time. The theory confirmed the absence of appreciable effect which the experiments showed. I have always been very grateful to Professor Debye for helping to turn my interests, which had been almost entirely experimental, towards the theoretical side of the subject, and for giving me confidence that I found very useful later.

At about this time the series of volumes to be called the *Crystalline State* that were to replace *X-rays and Crystal Structure* were being planned. The first volume, by W. L. Bragg, appeared in 1934, and it was proposed that I should be responsible for the optical part of the subject in a second volume. Rather less than half of this had been written when in 1937 I was appointed to the Chair of Physics in the University of Cape Town, in succession to B. F. J. Schonland, and my eighteen years in the Manchester laboratory came to an end. It had been a very happy period, and I should like to say here how much I owe to Professor Bragg for the inspiration of his friendship and leadership during that time. It is a debt I cannot sufficiently acknowledge.

One of my predecessors in the Cape Town Chair, Professor Alexander Ogg, who still had a room in the department, from which he directed the Union Magnetic Observatory, had carried out work on crystal structure, publishing papers on the structure of antimony and bismuth at about the same time that my own two papers had appeared; and he had also done pioneer work on the structure of potassium sulphate. Crystal work had however stopped at the time of my arrival,

and had to be revived. I decided to continue work on the aromatic nitro-compounds in which I had become interested in my last years at Manchester. In 1943 the first structure that of 4 : 4′ dinitrodiphenyl was published by J. N. van Niekerk, later to become head of the X-ray Division of the National Physical Laboratory at Pretoria. Some account of the work at Cape Town is given elsewhere in this volume. My load of teaching and administration became too heavy for me to do much work myself, but I was fortunate in having from time to time a number of able assistants, among them Dr. D. H. Saunder, Miss E. M. Archer (Mrs. D. H. Saunder), and Dr. Aaron Klug, who did his first X-ray work in Cape Town.

In 1945 the second volume of the *Crystalline State* was completed at Cape Town, and it was published in 1948. I retired from the Chair in 1956, and during 1956 and 1957 was Vice-Chancellor and Acting Principal of the University. X-ray work continues under my successor, Professor Walter Schaffer.