Personal Experiences of a Crystallographer

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My first contact with Crystallography took place in 1921 in a small room in Peterhouse, Cambridge, when I was a young freshman. I was being interviewed by a physicist with a view to deciding what subjects I ought to study during the first two years at the University. Having consulted my record he said, 'Well, of course, you will take Physics and Chemistry and then as a third subject I should suggest Mineralogy. It is a nice small subject.' As I had no knowledge of any science subjects apart from Physics and Chemistry, I gladly took my supervisor's advice. From the first lecture I found great interest in the subject. Hutchinson had the view that a lecture ought to be entertaining as well as being informative. We were taught classical crystallography with many practical exercises involving the drawing of accurate stereograms and the calculations of angles between faces and axes in all crystal systems. The optical goniometer in use by the students was of the same design as that described by Wollaston in 1813. On these instruments, which were quite adequate for the purpose, we measured a number of crystals of natural minerals. Hutchinson also gave a course on Mineralogy in which he went through the whole of the types of the mineral species. This tended to be rather like a recitation from a text-book but the special points were well brought out. Some students found descriptive mineralogy very tedious but I was quite fascinated by the development shown by natural crystals. Lewis was Head of the Department and he was remarkable in that he refused to render accounts in the form required by the University. A consequence of this was that funds for running the Department were dependent on students fees. Lewis also maintained an Appointments Registry for placing graduates in schools. This was located in the Department itself though it had no official connection with the University.

In 1927 I finished my Ph.D. thesis, under Rutherford's supervision, on the natural radioactivity of Radium B, C and E. Hutchinson asked

me if I would become a University demonstrator in the Department of Mineralogy and regard as my special province the development of courses of lectures and practical demonstrations on Crystal Physics. This appealed to me very much and I gladly accepted. The salary was £125 per annum, though by supervising students it was possible to earn rather more. Hutchinson was a great raconteur and each day he gave tea to all working in the Department. He provided the madeira cake and made the tea and coffee himself and was the life and soul of the party. We all enjoyed his stories of life in Cambridge during his younger days. During one of the demonstrations in 1927 I was going out of the darkened room and one of the class, Nora Martin, was at the same time coming in. We collided and from that moment began the courtship which led to our marriage in 1928. My wife took up research in structural crystallography under Bernal who had just been appointed by Hutchinson to develop X-ray Crystallography in Cambridge. The necessary apparatus was just being developed and the X-ray tubes were continuously evacuated demountable gas-tubes of the design introduced by Müller at the Royal Institution where Bernal had worked before coming to Cambridge. My first piece of research in the Department of Mineralogy was occasioned by a visit of Mr. Alpheus Williams. He had an enormous enthusiasm for the study of diamonds and he normally carried in his pockets little sacks of stones which would be spread out on the table whenever he wished to illustrate a particular feature of a diamond. He had one large flat stone and I remarked that for the study of a possible piezoelectric effect in diamond this would be a good stone. He at once gave me the crystal and I did the measurement. Within the accuracy of my observation no piezoelectric effect could be observed. The courses in Crystal Physics and in X-ray Crystallography which were being built up occupied most of my time. There was not much technical assistance, either in making or running apparatus nor much money to buy materials. One small illustration serves to show how restricted funds were in those days. I wanted a reel of ordinary double-cotton-covered copper wire and I asked Hutchinson if I might get this. His reply was 'There are some odd pieces of wire in the laboratory, would these not do?'

At the beginning of the academic year 1930/1931 Hutchinson retired and Tilley was appointed Head of the Department. There had been an immense discussion going on since 1927 about the future of the Department of Mineralogy. A commission had been appointed by the University and this had recommended that two departments should be formed, namely, one of Crystallography and the other of Mineralogy

and Petrology. Largely on grounds connected with the political views of certain people the former was not created while the latter was. The scheme was originally intended to leave research in X-ray Crystallography in its original place and to transfer the teaching in Crystallography to the new Department of Mineralogy and Petrology which was completed in 1931. As time went on the arrangement broke down and a considerable amount of research in X-ray Crystallography had to be done in the Department of Mineralogy and Petrology. Eventually the old department of Mineralogy was demolished to make room for the new Cavendish Laboratory.

One of my principal activities during the period 1931–35 had been the development of an automatic recording X-ray diffractometer. In the design of the instrument I collaborated with my brother-in-law, A. J. P. Martin. This instrument worked with punched celluloid film and recorded photographically. The data for the determination of the structure of gypsum, $CaSO_4 \cdot 2H_2O$, were obtained on this diffractometer. From January to September 1936 I had sabbatical leave in Norway, Sweden and Finland. My wife and ten-month-old son, Tony, came with me. In Oslo, Nora and I worked in Professor O. Hassel's laboratory of Physical Chemistry in the newly-built block at Blindern. We had our own portable X-ray tube and X-ray goniometer and it proved possible to do the work necessary to finish off the determination of the structure of gypsum. Here also I did much of the writing of *A Textbook of Crystal Physics* though this was continued in Uppsala and Helsinki. My first experience of skiing was gained round Oslo and this I very much enjoyed. Siegbahn was head of the Physics Department in Uppsala and much of the work was concerned with the long wave-length X-ray spectra. In Helsinki Wasastjerna was in charge of Physics and actively working on the theory of the physical properties of alkali halides.

After returning to Cambridge I concentrated on finishing *Crystal Physics*, and it was published in 1938. Political developments in Germany at last convinced Paul Ewald that he ought to come to Cambridge and he arrived, with none too much time to spare, in our X-ray Crystallographic Department, housed at that time in the Old Anatomy School during the rebuilding of the Cavendish. Ewald founded the discussion group which for many years went by the name of 'The Space Group'.

There were a large number of students in Cambridge throughout World War II and our courses continued in much the same way as before. My wife and I felt that any scientific work we did ought to bear some relation to the war effort. But professors were not encouraging

and we sought out industrial crystallographic problems for ourselves. These were connected with diamonds, sapphire, coal and quartz. The diamond problems were all arising from the cessation of Dutch supplies of diamond dies for wire-drawing. Many problems arose concerning the texture of the crystals and we developed the topograph method of studying them. Sapphires had previously been obtained from Switzerland but now they had to be grown locally and problems of fracture and orientation of the crystals arose. Coal was being studied by the B.C.U.R.A.* research laboratories and on the practical and theoretical aspects of the measurement of the refractive and absorption indices of opaque substances they were glad of our help. The supply of quartz for piezoelectric oscillators had largely come from Brazil. During the war many cargoes of crystals never arrived and the supply position was at one time very bad. We were therefore asked by the General Electric Co. Ltd. to undertake the artificial growth of quartz crystals. We can claim to be the first to have produced quartz crystals by hydrothermal means in Great Britain. During the war our work was in the early stages but after the war a useful process was established. With the object of finding out how to make use of electrically twinned quartz we also undertook a research on the control of electrical twinning. This too was only completed after the end of the war. All this war-time work was done in the laboratory at our own home which we assembled in the early months of the war.

The institution known as 'Summer School' has been established a long time in many countries but I think the first Crystallographic Summer School was arranged in the Department of Mineralogy and Petrology in 1943. Together with Henry, Lipson and other members of the staff, we organized a course of lectures and practical exercises which in varying forms and in different places has continued until the present time. A consequence of this effort was the conviction that the material ought to appear in book form. In 1951 the *Interpretation of X-ray Diffraction Photographs*, which was based on the material used in the early crystallographic Summer Schools, saw the light of day.

International relations among scientists have always been a special concern of mine and I gladly supported the Society for Visiting Scientists when it was formed just after the war. One of its first actions was to invite a group of French scientists to come to this country and to give talks on the work they had been doing. At this meeting I heard Laval speak, for the first time, on the work involving diffuse reflection of X-rays from crystals. This subject appealed to me very much and I

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forthwith decided to make it a major part of my scientific work in the coming years. In this I was at a great advantage because of earlier experience with X-ray diffractometers. A succession of assistants and research students including, Macdonald, Ramachandran, Hargreaves Lang, Prince, Hoerni, Prasad and Sandor greatly assisted this work.

Two scientific instruments were indispensable for this work; one the X-ray diffractometer and the other the automatic recording microdensitometer. The 1936 design of diffractometer was not sufficiently well adapted for the purposes of diffuse reflection work and many changes had to be made. The production of these two instruments is connected with the development of the war-time work which Nora and I did together. While studying the control of electrical twinning in quartz we found it necessary to make ball-and-spoke models of the atomic arrangements. We made efforts to get other people to take up the manufacture of these models and the Company, called Crystal Structure Ltd., was formed to facilitate this transfer. In fact we found that it requires expert crystallographers to ensure that the models are properly assembled and we eventually took over the manufacture ourselves. Meanwhile I had had to construct a microdensitometer for my own work and we thought this would probably also be wanted by other crystallographers. The diffractometer has now been developed as an automatic setting and recording instrument in response to the invasion of crystallography by automation.

A separate chapter in my scientific work relates to the period 1956–58 when Dr. N. Joel was a research student of mine. It was again Laval who caused me to take an interest in the work on the breakdown of the classical theory of crystal elasticity. Joel and I studied the elastograms of ammonium di-hydrogen phosphate and found, as Zwicker had previously asserted, that the figures could only be interpreted on the assumption that velocities of transverse waves in the crystal, which must be equal on the classical theory, are in fact unequal.

I had originally been appointed to establish a course in Crystal Physics. The 1938 text book on the theoretical aspects was a consequence of this and so was *Experimental Crystal Physics* which appeared in 1957. This was subsequently translated into German, Russian, Polish and Chinese. After this I began to write *Diffuse X-ray Reflections from Crystals* based on the research work done since 1948. This book should appear early in 1962. I retired from my University post in 1960 because I wished to be able to devote a greater portion of my time to scientific work. This was only possible because of the laboratory Nora and I had built up during the war and because of the small factory making a limited number of crystallographic scientific instruments.