Personal Recollections

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More than forty years ago, when I had finished my academic studies and had obtained my doctorate with a thesis on the Brownian movement I began to look for a new field of research. One of my best friends, a grandson of the famous spectroscopist Jonas Ångström, after whom the Å. U. has been named, advised me to take up X-ray investigations of crystals as a new line of research. 'This modern discipline,' he said, 'must be particularly profitable to a chemist. There is a tremendous lot to be done in chemistry with these new methods.'

At that time I was already married, and we had a son and I felt strongly the necessity of earning a livelihood. I was fortunate to get employment as metallographer at the recently founded, and already successful, ball-bearing company SKF in Gothenburg. In January 1918 on the long railway trip from Uppsala to Gothenburg I read a textbook on metallography but I also found time to study W. H. and W. L. Braggs' recently published book on *X-rays and Crystal Structure*, which I found to be extremely fascinating and stimulating. What I read there occupied my thoughts a great deal during the following year while I was learning the practice of steel microscopy.

I saw very clearly that the new X-ray methods could be used in metallography with great advantage and that the powder method of Debye and Scherrer was especially well adapted for this purpose. I had heard of this method first hand in 1916 in a lecture by Debye at Göttingen during the time when I was studying the Brownian movement and the coagulation of colloids at Zsigmondy's laboratory.

In the fall of 1919 I wrote a letter to Manne Siegbahn who at that time was still a young lecturer but who had taken over Professor J. Rydberg's duties as head of the physics department at the University of Lund during his illness. I asked Siegbahn if he would allow me to use his recently constructed metal X-ray tube for an investigation of steel and steel carbides. The permission was kindly granted.

An assistant of Siegbahn, a young student with the name of Axel Lindh, later Siegbahn's successor as professor of physics at Uppsala, introduced me to the use of the X-ray tube and together we produced X-ray powder patterns of iron, of steels heat-treated in different ways, and of cementite. The apparatus was primitive and the outcome of the investigations rather meagre. Unfortunately, we used a copper anticathode which, of course, gave highly blackened films the lines of which were hard to discern. Later, we changed to an iron anticathode and succeeded in obtaining better patterns. We made a camera with which it was possible to obtain photographs of an iron wire electrically heated to high temperatures. The structure of iron was determined at about 800°C (β-iron) and at about 1000°C (γ-iron). The former was found to be the same as that of α-iron; γ-iron was found face-centred cubic as was the iron of hardened austenitic steels.

At the beginning of 1921 I was appointed metallographer at the Metallographic Research Institute in Stockholm which had been organized by the steel and metal industries of Sweden. I met Gösta Phragmén there who was certainly a 'connaissance à faire'. He was a young student who had passed his first academic examination and wanted to deal with problems of technical interest during his further studies. Son of a prominent mathematician he had inherited much of his father's theoretical ability. Furthermore, he was an exceptionally able experimentalist, an ingenious constructor, a well-trained glass-blower, well versed in electrotechnics and handy in all kinds of mechanical work. Above all, he was a splendid character, being very modest and always ready to help. He was permanently surrounded by young research adepts whom he guided in their work with an inexhaustible benevolence. He was interested in thermodynamics and played an important role as adviser to metallurgists who wanted to apply its principles to the problems of steel production.

The mineralogical institute of the University, situated quite close to the Metallographic Institute, had a high-tension apparatus and equipment for taking Laue photographs which were used by G. Aminoff, at that time lecturer in mineralogy at the University. Phragmén had worked with him for some months and had found X-ray crystallography to be a most fascinating research field. We decided that together we should try to use its methods on metallurgical problems.

I was fortunate in obtaining grants from the university and some foundations, enabling me to buy another high-tension apparatus and instruments for taking powder and rotation photographs. The X-ray

tube and the cameras were built according to our designs (mainly Phragmén's) by an instrument maker in Stockholm and at the workshop of the Metallographic Institute. The X-ray source was a metal tube of the Siegbahn-Hadding type which was evacuated by means of mercury pumps. The gas pressure of the tube was kept constant by means of an excellent capillary tube leakage constructed by Phragmén. This tube worked very reliably. The apparatus was used, at times night and day, during twenty-five years and thousands of X-ray patterns were produced with it.

I gave a lecture on my investigations in Lund at a meeting of Jernkontoret (The Iron Masters' Association) in Stockholm in 1920 and afterwards I had the pleasure to receive from some members of my audience a selection of beautiful carbide and silicide crystals grown in blow-holes of ferrous alloys. They were of great value in the following investigations. To enable us to make well-defined alloys we wanted to have a good vacuum furnace. We experimented some time with a cathode ray furnace and obtained very pure alloys with it but the sudden evolutions of gas from the heated specimens caused such violent fluctuations of the current as to hazard the existence of the high-tension apparatus. So Phragmén constructed a vacuum furnace based on electrical carbon tube heating which functioned very well. We used it for purifying magnesium, manganese and other metals by distillation and produced alloy melts of some hundred grams. I suggested that Phragmén should publish a description of this furnace so that his construction could be of use to scientists in other laboratories but I got a reply that is very characteristic: 'Anybody wanting to make a vacuum furnace must, of course, understand that it should be made somewhat in this way. A description of it is not worth the trouble of writing and the printer's ink.'

A collaborator during the first investigations with the new apparatus was Eric Jette, a jovial student from U.S.A., at least 6'3″ tall (Jette is an old Swedish soldier name and means 'giant'). Together we attacked the structure problems of the copper-aluminium alloys which was hardly a happy first choice as this system is rather complicated. The structure of many of its phases is, in fact, still unknown. During this investigation we found, however, a highly symmetrical phase of a kind that we later came across in many other alloys and with an atomic arrangement that has its analogy in γ-brass.

In the autumn of 1926 A. J. Bradley came from W. L. Bragg's institute in Manchester to take part in our work. He was interested in our research on α-manganese and during his stay here he solved its

structure problem. When he returned to England at the end of the year he took with him a number of Laue, rotation and powder photographs of γ-brass and analogous phases and succeeded later in determining how the atoms are arranged in them. This was a real break-through in X-ray metallography and was a great stimulus for us in Stockholm to try to attack structure problems. Evidently, it was not impossible to solve them, even if there were many atoms present in the unit cell.

In 1927 Tr. Negresco of Bucarest who had lately studied in Paris visited our institute and took part in an investigation of the iron-chromium-carbon system. He spoke French which improved our knowledge of that beautiful language somewhat, but, alas, not sufficiently. He is now professor of metallurgy in his native city.

During the years 1921–25 the X-ray apparatus had found a place in the mineralogical institute of the University. Unfortunately, its dark-room could be put at our disposal only for a few years. Later, the loading of the cameras, the development of films and other photographic work, had to be performed in a very primitive, dusty and dirty dark room in a building belonging to the Metallographic Institute far away on the other side of the street. It was, however, always extremely exciting to develop the films and see what they had to tell us, so we willingly put up with this inconvenience. In 1926 all the equipment was moved into the Metallographic Institute. In 1927 I was, however, appointed professor of general and inorganic chemistry at the University and so we returned into its building but this time into its chemical department. A more rationally furnished X-ray laboratory was by and by fitted up there.

In other respects my department was very poorly equipped. If we had not had the benefit of collaborating with the Metallographic Institute the research possibilities would have been bad but, fortunately, the resources of that institute were kindly put at our disposal and my pupils thus had access to furnaces, microscopes and other facilities which they could use for their work. The research was therefore mainly orientated on metallographic problems, especially during the first years.

The number of students wanting to try their research abilities on X-ray crystallographic problems grew very rapidly and it soon became impossible for me alone to guide them in their attempts. I had, however, great help not only from Phragmén but also from several other collaborators who successively mastered the methods. One of them was Harry Arnfelt. Another was Gunnar Hägg who in 1929 got

his doctor's degree and was appointed lecturer in general and inorganic chemistry. He was a firm rock in the turmoil of the young people. He went about his task of teaching so thoroughly that he even married one of the most able (and charming) lady-students whom I had confided to his care. Great assistance was also rendered by Lars Gunnar Sillén and Cyrill Brosset. Although retarded by much military service the former was ready with his doctoral thesis in 1940 at an age of twenty-three years. The latter got his degree in 1942.

From 1940 on I had the pleasure of working together with Anders Byström, a very talented student with a great ability for crystal structure research. Already at that time he suffered from consumption but we all believed and hoped that he would conquer his illness. He had time to write a fine dissertation on manganese and lead oxides but in 1952 he was overcome by his disease and died. Another young doctor who died prematurely was Olof Nial who shortly after having written a dissertation on alloys of tin with transition metals in 1945 was killed in a motor car accident. Both these scientists had been appointed lecturers at the university. Their death is greatly to be deplored.

Already before the decease of these collaborators Phragmén died suddenly during an operation in 1944. That was a severe blow and a great sorrow to all of us. He had been nominated head of the Metallographic Institute that was going to be reorganized and modernized and for which a new building was going to be erected not far from the Swedish Academy of Sciences. I had rejoiced in the prospect that Phragmén and his new well-equipped institute would be near the office where from 1943 onwards I had to perform my duties as secretary of the Academy. It might have been possible for me to carry on at least some research work there even if my time was occupied with administrative work. The death of Phragmén was a great loss to metal research in Sweden and to international science. He would have been an ideal head of the new institute where certainly great scientific conquests would have been made. The steel and metal makers in Sweden greatly deplored their loss.

During my first years at the Academy I tried in leisure moments to solve some X-ray problems which I had been engaged on earlier, but my efforts were in vain. My time was too much split up by administrative work. I retired, however, from the secretaryship in 1959 and since then I have got fairly well into the recent development of X-ray crystallography and I have taken up those problems again and, as I am very reluctant to give in, I hope I will succeed in solving them.