Personal Reminiscences

JEAN WYART

How did I come to crystallography, in 1925, when I had still to learn everything about this science? Certainly not by special inclination. Chance, casual circumstances which lead human destiny, made me a scientist and a crystallographer.

Living in a coal mining country, in the north of France, where only one primary school existed, I would have been at the end of my studies on reaching my twelfth birthday, and would have become a workman, like my father, had not war and the fire which burnt down our house, driven us out of our village, in October 1914. Finally we came to settle at Abbeville where there was a secondary school. I obtained a railway scholarship and thus studied for becoming a railway engineer. Consequently I prepared the competitive examinations of admission to the highest schools which, in France, play a most important part besides the Universities. Having passed in 1923 both the entrance examinations of the 'Ecole Polytechnique' and the 'Ecole Normale Supérieure', I chose the second one which leads to educational careers, and thus put a definitive end to the railways. The 'Ecole Normale Supérieure' is in the Latin quarter of Paris and the students, free of all worries for the necessities of life, attend the courses of the Sorbonne and some additional lectures at the school; most of all, they enjoy the advantage of fine laboratories and of the daily contact with their professors. I spent there four marvellous years.

The third year of the studies at the Ecole Normale is used for prepararing some laboratory research work, the conclusion of which must be a memoir called 'Diplôme d'Etudes Supérieures', which has to be maintained in an oral examination at the Faculty of Sciences. At the end of my second year, my professors of physics, Henri Abraham and Eugène Bloch, had offered me to work at the laboratory of the School. I had accepted when Eugène Bloch asked me to go and see a professor of mineralogy of the Sorbonne, Charles Mauguin, who

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wished somebody to help him out with some calculations. This is how, with one of my friends, a mathematician, I entered for the first time, on a spring day of 1925, a laboratory where I was to spend my whole life. Charles Mauguin wanted to draw up numerical tables in order to interpret his rotating-crystal patterns. He used a cylindrical camera and applied formulas of spherical trigonometry to infer the indices of the reflecting reticular planes from the position of the X-ray spots. The calculations required by Charles Mauguin were tedious, long and laborious, and a month later I went back to his laboratory to tell him that we had neither means nor time to finish his numerical tables. He then gave me the news that these tables had become obsolete. It had occurred to him to bring in the reciprocal lattice and this made the graphical interpretation of X-ray patterns immediate. He has described in a detailed memoir, published early in 1926, the methods of interpretation of X-ray patterns which are still in use nowadays. I think that this idea had occurred to him without having brought in Ewald's work. He used for a long time, as most of the crystallographers do, the polar lattice introduced by Bravais in 1848, in order to solve the crystallographic computations based on goniometer measurements. It was at the time of writing his memoir that he noticed that the construction well known now by every student under the name of 'Ewald construction' appeared in fact in an article by Ewald, published in 1921 in *Zeitschrift für Kristallographie*, as well as in his book, *Kristalle und Röntgenstrahlen* published in Berlin in 1923. Like all other students of physics and chemistry at that time, I was absolutely ignorant even of the elements of crystallography. That day, Charles Mauguin showed me his X-ray equipment and his X-ray rotating-crystal patterns, but most of all I was amazed by the Laue diagrams. Mauguin possessed an incomparable charm and had a wonderful talent to make difficult relations perfectly clear. He explained to me the connections between the crystal lattice and the reciprocal lattice, its application to the interpretation of X-ray patterns, and the rose-like arrangement of spots on the Laue diagrams. On that day I spent three hours with him and came back to the school enthusiastic for crystallography. It was rather difficult to explain to Henri Abraham and Eugène Bloch that I preferred to spend my year of research work under the direction of Mauguin. When arriving in October 1925 at the Sorbonne mineralogy laboratory, I first was somewhat bewildered. Compared to the physical and chemical laboratories of the Ecole Normale, this one was extraordinarily clean and silent. Only Frédéric Wallerant and Charles Mauguin worked there, and two aged laboratory servants who spent all their time at cleaning. Mauguin had asked me to make the synthesis of a basic zinc acetate which had just been discovered, in order to study it with X-rays. I had to distil acetic acid anhydride under vacuum; I soiled a very clean room and the servants, upset by this unusual trouble, were not at all pleased. I hastened back to the Ecole Normale to prepare there my fine almost perfect octahedra of basic zinc acetate. Above all, I found there my friends, the young teachers who were training us and renewed our brisk discussions. I had read the works of Laue and of the Braggs; I was enraged to see that none of my friends believed in the reality of the arrangement of atoms such as the Braggs proposed it. The Bragg's structure, they thought, was only a clever hypothesis to explain the X-ray diffraction, just as if two sorts of atoms existed: the atom of the chemists and the atom of the Braggs. The majority of the chemists don't think much of such research and one of them asked me if I would really enjoy playing at cup-and-ball with atoms.

By stressing this point, I wish to underline the state of mind which prevailed in our laboratories in 1925. Our country, terribly ravaged by the war (almost all the young scientists enrolled at the Ecole Normale during the years 1911 to 1915 were killed) was in this field very far behind the others. Mauguin was almost the only one in France who wanted to use X-rays for chemical aims. Meanwhile, amongst the students of the Ecole Normale, Ponte had just come back from the Royal Institution in London where he had spent a year with Sir William Bragg; my friend Marcel Mathieu had followed him and was still there in that year.

When I came back to the Sorbonne in January 1926, I took my first photograph of an oscillating crystal. I had chosen a too wide oscillation angle for this complex chemical compound and I still remember the thousand diffraction spots on the film.

Wallerant and Mauguin had asked me to attend the lectures on mineralogy. I was not at all prepared for natural sciences, but I thoroughly enjoyed listening to these two remarkable teachers who mainly stressed the crystallographic aspect of mineralogy. We were only about thirty students, most of them geologists or chemists; and yet it was the only teaching of crystallography that existed in Paris. It is not suprising that almost all the students in France were completely unaware of the importance of the discoveries of Laue and the Braggs.

These few months spent in the mineralogy laboratory have determined my scientific career. I was the only research student and Charles Mauguin gave me a great part of his time. Rapidly I learnt all the elements of crystallography and the X-ray techniques; Mauguin, with his wide-spread learning, explained to me also the current event of that time, atomic physics, which occupied his mind. When I left him in July 1926, in order to prepare the competitive examination of 'agrégation' of physical sciences for the next year, and after that to do my military service, I had firmly made up my mind to come back to work with him. This I did as early as October 1928. I came back to this laboratory of the Sorbonne which I never left since. The following year Stanislas Goldsztaub and Jean Laval also came to work under the direction of Mauguin. It was Frédéric Wallerant who proposed to me, as subject for my Ph.D. thesis, the study of zeolites. He was a short, skinny, limping elderly man; with a severe and stern countenance, yet he was great-hearted and I was very fond of him. Just as he arrived, at nine in the morning, I used to hear the noise of his walking-stick. Monsieur Wallerant was coming in for a few minutes talk with me. This would often concern some matter near to his heart. He told me how impressed he had been by the discovery of Laue, how he had unsuccessfully tried to repeat it with his friend, the well known physicist Villard. He showed me the insufficiently powerful tube and chiefly the diaphragm with a too small aperture for an X-ray beam of sufficient intensity. He used to say rather bitterly that he had spent much work on crystallography, but had come or too late or too soon, for the main discoveries had already been made by Haüy, Bravais, Sohncke, Schoenflies, Fedorov. He was deeply convinced that X-rays were going completely to transform this science; but he did not believe in atomic structures. He saw in X-rays only a particularly powerful goniometer, which disclosed the lattice by a kind of statistical effect. He urged me not to persist on childish attempts of determining the atomic structure of chabazite but rather to use X-rays as a convenient goniometer for studying the modifications of the lattice brought about by the replacement of calcium atoms by other cations and by the diffusion of zeolitic water by temperature action.

When my thesis was finished, I sent a copy to Georges Friedel, professor at Strasbourg who some years earlier had done excellent work on zeolites. He wrote me a long letter with some compliments and some criticism. He congratulated me for a point which I had considered as secondary and of lesser importance. I had shown that the silica skeleton left by an acid attack on heulandite, though it retained some optical anisotropy, is amorphous and is not at all a silicic acid characteristic for heulandite as some chemists and some mineralogists asserted. Regarding the arrangement of atoms in chabazite which had cost me a tremendous work, he let me understand the uselessness of such an effort. Georges Friedel, like E. Mallard, regarded the crystal as more complex, and the crystal symmetry as having only a statistical significance and as resulting from twins of very small individuals, most of triclinic symmetry.

While I was working on my thesis, I was led to collaborate for the first time in an international enterprise of crystallography. Charles Mauguin was a member of a Committee of crystallographers who, under the chairmanship of von Laue and Sir William Bragg, had been commissioned to draw up the *International Tables for the Determination of Crystal Structures* published in 1935. Mauguin entrusted me with the drawing of the symmetry groups other than the quadratic groups which had been taken over by Astbury in Leeds. It was an excellent exercise for me; in the evenings, after supper, I used to work with the help of a designer, in a quiet and silent Sorbonne. Everything went very well up to the cubic groups and the same drawings, hardly modified, are still to be found in the new edition of the *International Tables*. The representation of the cubic groups was more difficult. Mauguin had long discussions with me and at last I gave up this work which had given me the opportunity of meeting at the laboratory Ewald, Bernal and C. Hermann.

Frédéric Wallerant retired in 1933 and I became lecturer attached to the chair held by Mauguin. Being fond of teaching, I was very sorry to see that our lectures were so little attended; therefore, at the beginning of each academic year, we began sending a letter to all the teachers in physics and chemistry calling their attention to the physico-chemical importance of crystallography and asking them to advise their students to attend our lectures. This propaganda was soon effective and the number of our students started to increase steadily.

I wish to say some words about crystallography at the International Exhibition of Paris of 1937. The idea had occurred to Jean Perrin to create a 'Palais de la Découverte' in order to display the great strides of science to the general public. There was a special room for crystallography; we had promised some fine minerals which would be displayed next to big models representing their atomic structures and the corresponding X-ray photographs; we had also offered some fine experiments on liquid crystals. We were late, particularly for the large models. I remember having spent many a night in supervising the work of solderers from the inland water transports in order to prepare, from thousands of brass balls, the models of sodium chloride, of left and right quartz, mica, felspars, etc.... As the 'Palais de la Découverte' still exists, one can still look at these models which gave me so much trouble.

The war years, from 1940 to 1945, were very difficult at the Sorbonne. The laboratories were not heated and the only electric heating was for the mercury-arc rectifiers which we had on our high voltages. The only way for us to carry on our work was to organize a mechanical workshop where we built ourselves most of our devices, X-ray tubes, autoclaves for hydrothermal synthesis, etc.

In Paris we were actually cut off from the rest of the world and one must have experienced such an isolation to realize how necessary in research work is a minimum of information. The South of France was more favoured and some scientific periodicals arrived there. We organized a secret passing on of these periodicals and I undertook the responsibility, at the National Centre of Scientific Research (CNRS), to create and run a Documentation Center which published an Abstract Bulletin of the received periodicals and supplied microfilms of the articles to the scientists who asked for them; since then the Documentation Centre has spread out to a considerable extent and it has cost me a great part of my time.

The greatest event as soon as the war was over, was the meeting of the X-ray Analysis Group of the Institute of Physics which was held at the Royal Institution, London, in July 1946. Crystallographers form a large family and we were happy to meet again. Sir Lawrence Bragg was the chairman, Laue was present. On this occasion the foundation of the International Union of Crystallography was laid, and I was a member of the small group who met little later at the Cavendish Laboratory in order to work out the first statutes. The Union has much expanded since and the number of the participants to the General Assemblies is ever increasing. At the laboratory of mineralogy of the Sorbonne of which I became the director on the retirement of Mauguin in 1948, the number of research workers has become too large. We are far beyond the stage when we were obliged to ask our colleagues to send students to our lectures. More than six hundred of them regularly attended our teaching and passed the examinations in 1960. Laue and the Braggs have made of crystallography a flourishing science.

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