For auld lang syne

J. D. H. Donnay

I came to Crystallography through Mining Engineering, Geology, and Mineralogy. By training I thus belong to the Old School. At the University of Liège, my first encounter was with a crystallographer, G. Cesàro, born in 1849 but still in charge of the entrance examination. When I entered my third year and time came to take the required courses in crystallography and mineralogy, Prof. Buttgenbach had just been appointed. My first reminiscence is a sobering one—flunking the first quiz! This failure may well have been the turning point of my career: in order to make up for my ignominious showing, I had to take a good second look at symmetry, and the derivation of crystal forms, by the traditional method of truncations, gave me a thrill.

My next professor of Crystallography was A. F. Rogers, at Stanford University. Although an old-time mineralogist, he recognized the importance of X-ray diffraction and was conducting a joint course with M. L. Huggins, then in the Chemistry department. He also emphasized the theory of groups and taught the derivation of forms by Gadolin's stereographic method. His enthusiasm for Geometrical Crystallography was boundless. At a meeting of the Mineralogical Society of America, a (then) young and up-and-coming 'X-ray crystallographer', M. J. Buerger, had remarked that he did not understand how people could still maintain any interest in the old geometrical crystallography, that once the crystal structure was unraveled everything worth knowing about the crystal was known. Rogers got up, expressed his disagreement, and concluded in a vibrating voice (I still hear him), 'Geometrical Crystallography has had a glorious past, and it will have a glorious future!'

My chief debt to Rogers is perhaps that he introduced me to Georges Friedel's admirable *Leçons de Cristallographie*, the second edition of which had just appeared (1926), for no other book has had on me as strong an influence. After 35 years I still find that it makes profitable and challenging reading.

Contents

The man who really showed me the shear pleasure of research was H. W. Morse. After six months of prospecting for oil in Morocco, I had come back to U.S.A. and, as 'Research Associate in Geology and Teaching Fellow in Mineralogy', I was spending another year at Stanford. Morse had prepared artificial three-dimensional spherulites of hundreds of compounds and had observed their interference effects between crossed nicols, in parallel light. I was told to go and see the beautiful phenomenon that simulated the conoscopic uniaxial figure. It was my good luck to derive the equation of the retardation curve of the spherulitic figure — though not until J. V. Uspensky (the Stanford mathematician) and H. A. Kramers (the visiting physicist from Utrecht) had shown me how to solve an elliptic integral. How elated I felt! My Ph.D. thesis, *The Genesis of the Engels Copper Deposit*, paled into insignificance—it had been a *job*, but *this* was pure joy! How well I remember the many happy nights that followed, during which I was working with my old friend Dr. Morse in the little house that was his laboratory, under the eucalyptus trees at the far end of his garden... (And how often the milkman's arrival in the early morning reminded me of bedtime!)

In the summer of 1931, I visited G. Friedel in Strasbourg. It was a short visit, for he was not well, but it left a deep impression on me. I had shown him our work on spherulites and other aggregates, and he had said, 'You have gathered a large number of very interesting facts.' I had been disappointed, 'Perhaps, yes, but—the interpretation?' With a faint smile and a gesture of powerlessness, he had answered, 'Ah, cela....' It was not until much later that I came to realize that his disappointing comment had *not* been disparaging and that, to his way of thinking, uncovering facts that could not have been predicted was worthwhile, was indeed the very foundation of scientific research.

In September I joined the Johns Hopkins University as 'Associate in Mineralogy and Petrography'. From the start I found friendly advice and help in the Chemistry Department, where Emil Ott, then M. L. Huggins, and in 1936 David Harker taught X-ray diffraction; at the U.S. Geological Survey, where W. T. Schaller showed me how to use a 2-circle goniometer; at the Geophysical Laboratory of the Carnegie Institution of Washington, where G. Tunell (who had just succeeded R. W. G. Wyckoff) and Tom F. W. Barth were taking great interest in structural crystallography. My friendship with M. A. Peacock, who was then with Charles Palache at Harvard, also began at that time. Somehow I got deeply immersed in the problem of crystal habit.

Henri Ungemach, of Strasbourg, had been using *multiple indices* to designate the forms of a trigonal crystal with a rhombohedral lattice: he would write $(30\bar{3}0)$ for the prism $(10\bar{1}0)$, reserving the latter symbol for crystals with a hexagonal lattice. As $(10\bar{1}1)$ would have been valid in either lattice, he even proposed to drop the minus sign over the third index in the rhombohedral case. He therefore expressed the rhombohedral criterion as '$(hkil)$ with $(h + i + l)$ a multiple of 3'. I had asked Huggins, at lunch time, whether Ungemach's criterion was the same as that used in X-ray diffraction. Huggins had been slightly baffled by the unfamiliar formulation, but he had answered that, off-hand, yes, he thought it *was* the same. This had led me to use multiple indices to express lattice criteria in other systems: (200) for the cube in I and F lattices, (222) for the octahedron in the I lattice, and so on. I had also been intrigued by Baumhauer's *zonal series*, of which Ungemach had found many striking examples, but I thought (erroneously, as it turned out) that they were strict consequences of the law of Bravais.

In June 1936 I went to see Ungemach, who was very ill at the time. He had insisted that I come immediately after landing, 'otherwise you might never meet me', he had written. As I entered his room, I found him studying my paper on calaverite. For three days he talked to me of the morphology of minerals, and I marveled at his knowledge. I do not think it is much of an exaggeration to say that he knew all the forms of all the minerals! He reeled off zonal series after zonal series. And he also said, 'Remember the base: why is it absent in so many species?'... Friedel's 'unpredictable facts'—he had them all! As I was about to leave, that Wednesday afternoon, I suddenly felt deeply moved, and I told him that, as long as I would live, I would remember the Strasbourg crystallographers who had taught us so much: Friedel and Ungemach. And he replied, modestly, 'Ah, ça, c'est beau! Car Friedel, voyez-vous, il était grand... comme cela (and he lifted his emaciated hand as high as he could), tandis qu'Ungemach, il est grand (and he let his hand drop to a few inches of the bed cover)... comme ceci.' The next day I was to give a paper on the form birefringence of chalcedony to the French Society of Mineralogy, in Paris. As he called the meeting to order, the chairman broke the news: Ungemach had passed away during the night. He had left me all his measured crystals and all his notebooks.

On my return to Hopkins in the fall, I went to salute David Harker who, heralded by the newly discovered Harker Section, had just arrived from Cal Tech to replace Huggins. It was friendship at first sight. He sat in my course, and during the second semester I would attend his. Little by little it was finally dawning on me that the Baumhauer-Ungemach series might well, in some cases, contain *more* than the law of Bravais. Knowing very little about space groups, I asked Dave whether there did not exist space-group criteria similar to the lattice criteria. So he told me about the existing tables. Then I showed him the morphology of orthorhombic sulfur in Friedel's *Leçons*: the law of Bravais unmistakably pointing to an F lattice, and the many anomalies in the list of forms arranged according to decreasing frequency of occurrence (the pinacoids and most forms whose symbols contained a zero appeared too high in the list). Would he look up the space group of sulfur and find its systematic extinctions for me?

Days passed, then weeks. I was beginning to wonder whether Dave was taking me seriously, but one late afternoon he appeared with a broad grin, 'Every symbol with a zero in it must have the sum of the indices divisible by 4.' Quickly I grab my Friedel, fling it open— Eagerly we pour over it—All the anomalous symbols violated the rule! We had it! And feverishly we started looking for more, and more, examples—The abstract on the Baumhauer-Ungemach series that I had sent to the Mineralogical Society of America for its coming Christmas meeting was already printed, but the paper was never given: instead, I was able to announce our generalization of the law of Bravais, which appeared in the *Comptes Rendus* in February 1937.

It was at that same Christmas 1936 meeting that Peacock presented his 'Harmonic-arithmetic rule', the powerful tool that enables one to recognize the dominant face in any *simple zone* (one governed by a primitive reciprocal-lattice net). He had concentrated his efforts on the triclinic system, which he thought would be the most general, but in which all zones are simple. We spent the summer of 1937 together at Harvard, working on the 'new Dana' between memorable discussions on the relationships between crystal morphology and crystal structure. I wrote my own paper on the development of crystal zones in the summer of 1938, which, like many other happy summers, was spent in the company of J. Mélon at the Institute of Crystallography and Mineralogy at the University of Liège. By taking the zonal extinction criteria into account, Peacock's Harmonic-arithmetic rule could be generalized to give a perfect explanation of the series of Baumhauer and Ungemach.

In 1939 I was called to Laval University in Quebec, where I got my first X-ray unit (a Baird gas tube and a Buerger Weissenberg camera). The three years I spent in Canada were highlighted by the visits to Peacock in Toronto. I would take my students to see him, in the hope that some of his perfectionism would rub off on them. He would show us how to plot a gnomonic projection, pricking the face poles half-way through the thickness of the drawing paper with a fine needle mounted on a chuck. Duly impressed we watched in reverend awe. Then he would open the drawer and produce a honing stone—to sharpen the needles ('they *do* get blunt')!—What exquisite figures he could draw! What simple and beautiful English he could write to record his highly conscientious observations!

After the war—in the wake of thousands of powder patterns taken at the Hercules Powder Company— I blissfully returned to Academe and to Hopkins. One of the first jobs of 'reconversion' was to create an International Union and a Journal. This happened in London in July 1946. I attended this first postwar meeting on my way to Belgium (where I had just been appointed a professor at Liège). The British participants wished to have a journal devoted to 'X-ray analysis', but they finally compromised and settled for the name 'Journal of Structural Crystallography'. A minority objected to the word 'structural' as unduly limiting the scope of the journal. The spokesman for the majority argued that the word 'Crystallography', alone, would be too restrictive! Hoping to save the unity of Crystallography, I countered that, since it was the function of an adjective to restrict the meaning of the noun, 'Crystallography' without any adjective would be more general. But the motion to delete 'structural' was defeated, and Dave Harker, who was presiding, concluded, 'Donnay has fired his last cartridge!'—What is in a word! It was obvious that, to most people present, the term Crystallography did not evoke the study of crystals in its broadest meaning, but connoted only 'hemihedry, holohedry, and all that sort of things' (as W. T. Astbury astutely put it). Two days later the Russian delegation arrived, who had been delayed en route. The discussion was not re-opened, but the name of the journal was changed, behind the scenes, to *Acta Crystallographica*. Single handed Academician Shubnikov had turned our rout to victory by remarking that, in Moscow, the Institute of Crystallography of the Academy of Sciences of the USSR comprised many sections besides that of Structural Crystallography; he had also recommended Latin as a good language for titles of international journals.

In 1912 Laue presented us with a magnificent tool. Thanks to him, Crystallography has reached undreamed-of heights. But the study of crystals did *not* begin in 1912: it started with Kepler—as Laue himself told us. The results of the past remain the foundation on which we build. Throughout my career I have striven for the rapprochement of classical crystallographers and diffractionists—teaching crystal structure to geologists and crystal morphology to chemists, pushing the amalgamation of CSA (Crystallographic Society of America) with ASXRED (American Society for X-Ray and Electron Diffraction) to get ACA (American Crystallographic Association). Today, in admiring the majestic edifice of crystallography, I would like to think that, if others have brought in the freestones, I have contributed some of the mortar.

Epilogue

In 1949 J. D. H. D. married a crystallographer. Together Donnay and Donnay worked happily ever after. Their latest papers (1961) deal with a Second Generalization of the Law of Bravais.

Next Section