Adventures of an Amateur Crystallographer by Marjorie Senechal

I wish I could report that I collected crystals as a child, but I didn't collect anything. A pastime I see now as proto-mathematics was many happy hours spent organizing and reorganizing the buttons in the tin box by my mother's much-used Singer sewing machine. There were dozens of buttons, of many kinds. Some were as large as a quarter, others smaller than a Dutch dubbeltje. Some were painted with flowers or faces. The materials differed too: wood, plastic, metal. I learned there are many ways to classify anything, and every organizing principle is quickly upended.

I entered the University of Chicago without finishing high school but I had no clear idea of what I wanted to study in college. In those days no one expected girls to aim for careers. I enjoyed mathematics. But I didn't know, or think to ask myself, what I enjoyed about it: which areas of math appealed to me and why; what stirred my imagination. After UC, I churned on and earned an MS and PhD in mathematics at the Illinois Institute of Technology.

My first post-PhD job was a one-year appointment (1966-67) in the mathematics department at Smith College, filling in for a woman who took the year off to have a baby. Though I had a two-year old daughter by then and was expecting another child in the spring, I persuaded the department chair to let me teach anyway. The baby would be born during spring vacation, I assured him. And indeed she was. My only problem, as the due date approached, was finding a gripping book to read in the hospital. Serendipity struck: in the science library I came across *Crystals: Their Role in Nature and Science*, by Charles Bunn. All I knew about crystals was that they were pretty. I checked it out and read it with increasing excitement in my few quiet moments the next week. I'd found the answer to my unasked questions. Crystals showed me what drew me to math: geometric forms; patterns, packings and tilings; and elegant puzzles like diffraction diagrams. The woman I'd replaced at Smith decided not to return. Job security gave me respite to think. I felt sure that math and crystallography could be combined in a way that pleased me, but I would have to find it myself.

Just about that time I found an article by the polymathic Arthur Loeb. I went to see him and came home with a box of symmetry workbooks and a bag of plexiglass polyhedra, some empty, others with a plastic ball at their centers. Arthur showed me the astonishing variety of simple crystal structures that can be modeled by fitting these occupied and unoccupied shapes together. He also showed me *Symmetry in Science and Art*, a translation from the Russian of a book his friend V. A. Koptsik had written with the great soviet crystallographer A. V. Shubnikov. This introduced me to color symmetry, which Shubnikov had pioneered; his 1951 *Symmetry and Anti-symmetry of Finite Figures* is a classic of the field. I found that color symmetry meant different things to different authors, and each had hatched his own notation. But on close examination all these different methods were exercises in group-subgroup relations. I wrote a short paper for *Zeitschrift fur Kristallographie* in which I pointed this out and this put me in touch with a wide circle of mathematicians and crystallographers.

Colored patterns became my new buttons. I spent hours in Smith's Art Library poring over a massive tome called *The Grammar of Ornament*, one hundred gilt-edged chromo-lithographed plates of ornamental patterns culled from cultures all over the world, and all times. Each plate showed dozens of intricate repeating patterns, a wealth of motifs: yet each pattern belonged to exactly one of seventeen symmetry types. One day a very senior professor of art noticed me browsing on his turf. He asked me (with curiosity, not hostility) what I, a mathematician, was doing there. I explained. "Do you know Dorothy Wrinch?" he asked me. I didn't. "You should," he said. "She is a crystallographer and she has a copy of that book." I went to see Dorothy and explained my new-found interest in crystals. She was, she told me, writing a book on crystal geometry, and I could help her by making models and illustrations. I saw this as an excellent way to fill in my very sketchy background. And so I became her informal, unpaid, post-doc. (We never finished the book.)

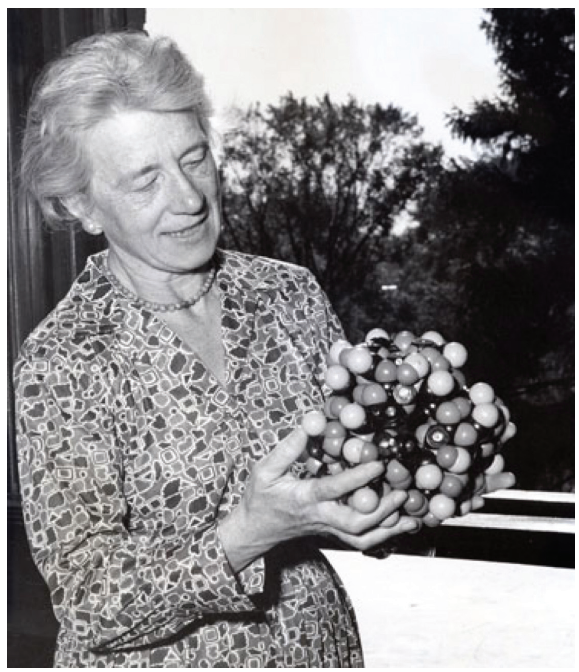

Photo of Dorothy Wrinch from the Smith College Sophia Smith Collection

Dorothy never told me she had been the epicenter of a controversy over the structures of proteins; she rarely talked about herself or her life. She taught me the basic notions of symmetry, not from a math text on group theory, but from F. M. Jaeger's *Lectures on the Principle of Symmetry and its Applications in all Natural Sciences*. I learned the importance of making models with my own hands, and studying them from every perspective. Dorothy stressed meaningful naming (hexahedra, not cubes!), exact diagrams, and succinct arguments. She was a demanding taskmaster; no vague or sloppy reasoning escaped her razor mind. More than any teacher I'd ever had (except my father) she held me to high standards. With two other colleagues I organized a Symmetry Festival in her honor, though she was by then too ill to attend. The proceedings were published as *Patterns of Symmetry*. At that time Dorothy was interested in twinned crystals. They became the driving force of my interest too.

Marjorie with H. S. M. Coxeter, British-born Canadian geometer (about 1985).

I spent my first sabbatical year in Holland with Piet Hartmann and Wiepko Perdok, authors of the Periodic Bond Chain (PBC) theory of crystal growth. I wrote a paper on "The mechanism of certain growth twins of the penetration type" and sent it to Martin Buerger, editor of *Neues Jarbuch für Mineralogie*. Buerger rejected it by return mail, mostly on the grounds that I hadn't quoted any of Buerger's many papers on twinning. And so I learned about turf wars in twin domains. In fact I hadn't read his papers but I quickly did. I added a reference to one of them, the paper was published, and we became friends.

When I returned to Massachusetts at the end of the year I learned that Dorothy Wrinch had had a stroke; she died a few months later. She had left her papers to the women's history archive at Smith College. Reading her letters, notebooks, and memos I glimpsed the outlines of her remarkable if contentious career. The Wrinch papers comprise some 30 boxes; she appeared to have saved every scrap, flattering and unflattering alike. "They'll never find stuff like that on me!" Martin Buerger exclaimed. (But, writing her biography thirty-five years later, I found the real story in other archives.) I organized a symposium about her papers: *Structure of Matter and Patterns in Science*. There I met Carolyn Cohen and David Harker for the first time. Both became my good friends and encouraged me professionally. Through Harker, I attended meetings of the ACA.

After reading an article by N. N. Sheftal' on tetrahedral penetration twins I wrote to him in Moscow. My year at the Shubnikov Institute for Crystallography, where Sheftal' worked, was invaluable for

Marjorie with N. N. Sheftal', Moscow (1987).

Ravil Galiulin with his wife and daughters, Moscow (1987).

an entirely new perspective on crystal geometry. I began reading papers on mathematical crystallography by B. N. Delone. In Delone I found my real teacher, though I never met him. But on the basis of his papers and through my friendship with Delone's students Ravil Galiulin, N.V. Dolbilin and M.I. Shtogrin, I became and remain his disciple, trying always to emulate his clear and simple approach to crystallographic problems and his informal, lucid writing style. Delone's work has been the starting point for all of mine since then. Not only is his approach simple and elegant, it has turned out to be useful. Quasicrystals show us that sharp diffraction patterns are not the sole province of lattice structures. Evidently "order/disorder" is a spectrum, not a dichotomy. Delone's perspective is a tool for exploring that spectrum.

Soon after I returned to the United States Jose Lima de Faria invited me to write the chapter on the history of geometric crystallography for the *Historical Atlas of Crystallography* he was then preparing. I snatched this chance to repair the gaps in my knowledge and threw myself into the history of science. Reading the original papers to prepare that chapter, I learned the wisdom of the adage "read the masters!" Not only because secondary sources sometimes get things wrong, but because they are necessarily selective. The masters said more than their followers reported. The nuggets left behind were sometimes just the ones I needed. And so I became an amateur historian of science too.

In 1981 I attended the IUCr meeting in Ottawa, my first. One lecture there was crucially important for me, though I didn't know that at the time: Alan Mackay's talk on the optical diffraction pattern produced by a Penrose tiling. Like the patterns in Harburn, Taylor, and Welberry's *Atlas of Optical Transforms*, he'd made a mask, a metal plate punched with tiny holes, and photographed the optical diffraction pattern it produced. But the holes were the vertices of

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Penrose tiles; the diffraction pattern should have been a blur. To the audience's astonishment, despite the apparent disorder of the mask, the diffraction pattern had ten-fold symmetry!

After Ottawa, I had a professional-identity problem on my hands. What was my field? Mathematics? Crystallography? History of Science? And who was I, a teacher in a liberal arts college or a member of the international research community? I decided not to choose, but to juggle instead. Smith College was an ideal setting for this juggling act. I could give courses on topics I wanted to learn, teaching myself along with bright students. There were no textbooks for these courses; I pulled the material together.

In January, 1985, when I arrived in Paris for a conference on mathematical crystallography, a colleague shoved the latest issue of *Physical Reviews Letters* in my face. “Have you seen this?” he shouted. The paper, *A metallic phase with long-range orientational order and with no translational symmetry*, by Shechtman, Blech, Gratias, and Cahn, was astonishing. The diffraction patterns Alan Mackay had manufactured occurred in nature too! The lattice paradigm whose history I had so carefully spelled out at the Hamburg IUCr in August had been toppled. By the time the invited mathematicians, physicists, and crystallographers arrived in Paris for the conference, the program was obsolete.

By a wonderful coincidence, Shechtman, Gratias, and Cahn were in Paris just then. We invited them to join us. The invited lectures were given as planned, but the rest of the time we discussed Penrose tilings and diffraction. Penrose tilings are fascinating objects. They can be studied through several mathematical lenses. First, they are self-similar: they repeat on all scales. Second, they are modular: they can be built by juxtaposing two simple shapes by following prescribed matching rules. And third, they can be obtained from an ordinary periodic cubic lattice by a technique called cut-and-project. Briefly - and I grossly oversimplify here - one takes a high-dimensional lattice, slices it with a plane, and projects the lattice points lying near the plane onto it. If the plane itself contains no lattice points, or only one lattice point, then the projected pattern is nonperiodic. This projected pattern will always have sharp bright diffraction spots. For the Penrose tilings, the lattice in question is cubic, its dimension is five, and the cutting plane is orthogonal to the cube diagonal (1,1,1,1,1). This cutting plane can have exactly one lattice point in it (e.g., if it passes through (0,0,0,0,0)) or none. It can't have more than one. The points of the five-dimensional lattice that are projected onto this plane don't form a lattice in that plane; they are a nonperiodic set. This point set diffracts as Mackay discovered. To get what Shechtman discovered, you do essentially the same thing, except that the high dimensional lattice is six dimensional instead of five, and instead of cutting it with a plane you cut with a three-dimensional subspace. And then you project. This is harder to visualize but the idea is identical. Again, you are guaranteed a nonperiodic pattern whose diffraction pattern shows bright sharp spots.

I gave a lecture on Penrose tilings at the Perth IUCr meeting in 1987. This led to an invitation from Dan Shechtman to visit the Technion. Members of his own department, the materials science department, didn't understand what his discovery was all about. Would I give a lecture course on crystal symmetry, and teach his colleagues to read the *International Tables*?

Now the field I had hoped to find when I knocked on Dorothy Wrinch's office door was burgeoning, with more conferences in more countries than anyone could possibly attend. I enjoyed my role as go-between, telling crystallographers about developments in mathematics and mathematicians about developments in crystallography. For example, "*Quasicrystals: the view from Les Houches*," which Jean Taylor and I wrote at a quasicrystal conference.

Jean Taylor (left) and Marjorie, Les Houches, France (1989).

The United States was a glaring exception to this international flurry. The influential Linus Pauling's very public disparagement ("there are no quasicrystals, only quasiscientists") discouraged young researchers.

I saw a need for a book on the basics of quasicrystal geometry, one that could supply a common background and vocabulary and introduce readers to Penrose tiles, diffraction geometry, the cut-and-project method, and more. In *Quasicrystals and Geometry* I explained, summarized and synthesized what seemed to me the most important questions that quasicrystals raised for mathematical crystallography and mathematics more generally.

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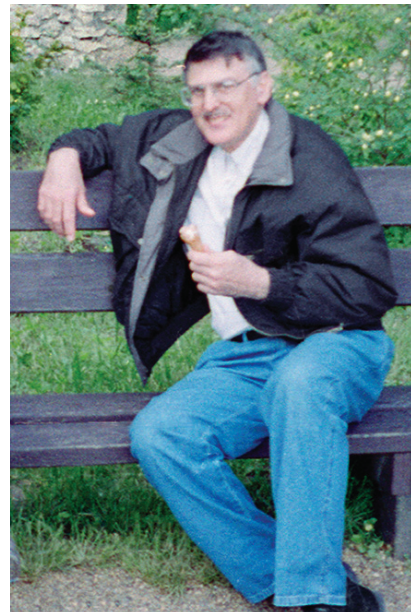
David and Deborah Harker in 1987 at the Perth IUCr.

In October, 2011, when the Nobel Prize in Chemistry was announced, I felt like dancing in the street. "Quasicrystals have fundamentally changed the way chemists think about solid matter," the Nobel Committee said.

After 42 years at Smith, I retired from teaching in 2007 to make time for two big projects. One was to edit *The Mathematical Intelligencer*, an international quarterly then in its 30th year. My other big project was a biography of Dorothy Wrinch. I had given a lecture on her papers soon after her death, and thought I had said all I had to say. Later quite a bit was written about Dorothy, some fiercely pro and more fiercely con, and most of it wrong. Wrong facts, wrong interpretation, and more than a few fictions. The errors propagated: writers who never knew her uncritically adopted the attitudes of people who had and added to the misinformation. Moreover, these writers saw her through the lens of chemistry. But Dorothy had been a trained as a mathematician, had studied logic with Bertrand Russell, and was a disciple of D'Arcy Thompson; *On Growth and Form* was her bible. There was more to her story and I set out to find it. I read letters to and from Bertrand Russell, D'Arcy Thompson, J. D. Bernal, Joseph Needham, Dorothy Hodgkin, Irving Langmuir, Isidor Fankuchen, both Braggs, and many more. Personalities and ambitions and who's right, who's wrong aside, I came to see Dorothy Wrinch's protein model as a lightning rod for a clash of scientific cultures. The clash is the eternal dialogue between truth and beauty, between complexity and simplicity, a dialogue both profound and productive. Indeed, it is an engine of science. And so I called my book *I Died for Beauty: Dorothy Wrinch and the Cultures of Science*. **Editor's note: See the book review of IDFB, p 36.**



Dan Shectman, Budapest (1993). Nobel Prize in Chemistry, 2011.

A crystallography banquet in Moscow in 1987. Marjorie is at left, B. K. Vainshtein, Director of the Shubnikov Institute of Crystallography is at far right and Herbert Hauptman is standing.

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