Personal Reminiscences

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My interest in science was started by a volume entitled *The Complete Chemistry, a Text Book for High Schools and Academies* by Elroy M. Avery (Sheldon and Company, New York and Chicago, 1883). This was my father's high school chemistry text in 1893, and I discovered it in our family library about 1910, when I was seven years old. This explained a good deal about matter, physical and chemical changes, and of course, chemistry. This seemed to be just what my mind needed, for it explained the complicated things about me in terms of simpler units, so that it became obvious that the world was a matter of chemistry. I was soon well acquainted with the contents of the book, and astonished my parents and their friends with this curious knowledge.

In due time I studied chemistry in high school and found it, along with geometry, my most interesting study. My father, noting this interest, felt that M.I.T. was the place for me, and when I finished with high school, I continued my studies there.

Because of the uninspiring teaching of chemistry, I looked around for another related field. I found that mining engineering made use of chemistry through mineralogy, so I entered the study of mining. This was a fortunate choice, because I immediately came in contact with Mr. Walter H. Newhouse, at that time a graduate student studying for his doctor's degree under Professor Waldemar Lindgren. Every student needs a wise and inspiring teacher, and Newhouse became this to me. What I had missed in chemistry, Newhouse made good in mineralogy, for that was what he taught. But more than that, Newhouse had a feeling for relevance, and always stripped a matter of its extraneous wrappings and went directly to the core. I recognized this characteristic and tried to emulate him. Although I graduated with a degree of B.S. in mining engineering, I took my bachelor's thesis under him. At just about that time he was given funds for a research assistant and he offered this post to me. My job was to relieve him of

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teaching elementary mineralogy while he devoted the corresponding time to research.

This was a turning point in my career. During the last two years, I had had tution scholarships, but my mother had supported me. It had never occurred to me to go on with graduate study, since there was no money available for it, so this assistantship changed my career from that of a practising mining engineer to that of a teacher. In two years I took my master's degree in geology, and in two more my doctor's degree in mineralogy, all under Newhouse, who had been advanced to an assistant professor. Newhouse and I became close personal friends, and shared an office. I absorbed much from him in the philosophy of science. We both basked in the research atmosphere created by Professor Waldemar Lindgren, dean of geologists and head of our Department of Geology.

In 1927, before I obtained my doctor's degree, Bragg spent a term at M.I.T., and I attended his lectures. Bertram E. Warren, a fellow graduate student in physics, became a student of Bragg's at that time, and with him worked out the structure of diopside. After receiving his doctor's degree, Warren continued in crystal-structure analysis. I had no laboratory facilities, but also wished to try out crystal-structure analysis, so, with Warren's kind permission to use his equipment, I took a set of rotating-crystal and oscillating-crystal photographs of marcasite. I solved this simple two-parameter structure alone in 1930–31 by applying the techniques I read about in the papers appearing in the *Zeitschrift für Kristallographie.*

This marked the second, and perhaps the most important, turning point in my career. I had been teaching mineralogy, optical crystallography, and petrology, so that up to this point I had been a mineralogist. But the structure of marcasite represented my first excursion into pure crystallography. I was delighted with the certainty of the conclusion reached by crystal-structure analysis, as compared with the arguable results published by the geologists and mineralogists of that period. Just following my delightful experience with marcasite, the new president of M.I.T., Karl Taylor Compton, obtained a large grant for research from the Rockefeller Foundation, and eventually I was asked by Professor Henry Shimer, acting head of our department at that time, what I needed for research. I immediately dreamed up $10,000 worth of X-ray equipment, had it approved, and I was shortly in the business of investigating the crystal structures of minerals.

In 1937 I was a member of Donald MacMillan's expedition to

Baffin Island in the Arctic. This trip up and back gave me the leisure to start my book *X-ray Crystallography*. The book had two objectives. The crystallographic preliminaries of crystal-structure analysis had never been treated in any detail, but had always been compressed into a small portion of a book covering all of crystal-structure analysis and the results the art had achieved to that date. *X-ray Crystallography* was intended to pay attention to the important preliminaries of unit-cell and space-group determination. At the same time it was written to the mineralogical crystallographers and as a protest against their current practices. At that time, the mineralogists made optical goniometric studies of the face development of crystals, and from these they attempted to deduce, on the basis of various theoretical ideas, mostly ill-founded, what the lattice of the crystal was. They were so confused that they tried to distinguish between a 'morphological lattice' and a 'structural lattice'. *X-ray Crystallography* was intended to present them with a fool-proof way of finding *the* lattice. I wrote a preface in which I stated my opinion of their groping methods. The preface was too frank to publish, so I had to write another moderate one, which now appears with the book, but I had the pleasure of writing what I thought of the mineralogical crystallography of the day. Writing two prefaces (the second publishable) was a practice I continued in later books.

I have found teaching a worthwhile career. In the first place, it has been my experience that students react to me as I reacted to my teachers. They need the teacher not only to guide them in technical matters, but to transmit a philosophy to them, partly by precept, partly by providing an appropriate atmosphere. Students are susceptible, and the teacher has a great responsibility. Many times I have had the experience of having a former student unconsciously quoting back to me my own philosophy. On the other hand, I have also been taught much by my students. The close rapport between student and teacher makes it possible for the teacher to absorb from his students knowledge which has developed since the teacher was involved in formal study, or which the student, with his youthful viewpoint, has seen fit to cultivate. More generally, I have found that a group of students and their teacher are members of a small select society, and that they teach and inspire one another, especially if the group has reached the critical size of, say, five. Within this group it becomes fashionable to advance in knowledge and to publish newly acquired knowledge, so that the members of the group emulate and stimulate one another.

Paul Niggli was a man I admired very much. I had the good fortune to spend a week with him when we both took part in the mineralogical excursion to Lapland following the 1951 meeting of the I.U.Cr. in Stockholm. I have had occasion to study his writings many times, and have been impressed by his characteristic of being ahead of his time. I encountered this rather recently when I was concerned about reduced cells. Niggli had worked out the whole matter years before it was used, and apparently just for fun. As editor of the *Zeitschrift für Kristallographie* he also demonstrated his high caliber. He either accepted a paper or rejected it, but did not require the author to make it fit his ideas or style. When I was asked to become a co-editor of the *Zeitschrift für Kristallographie,* I accepted with pleasure, remembering that Niggli had long occupied that post. Needless to say, Niggli's actions as editor have had a strong influence on my own. Because of my admiration for him, I also felt it a high honor when I was elected a Foreign Member of the Academy of Sciences of Torino to fill the place left vacant by Niggli's death.

I believe most scientists, including myself, do not spend enough time in an attempt to gain perspective. Usually we are too busy finishing the many projects we have started. I had this view impressed upon me as a result of a four-month stay in Brazil, while I was visiting professor in the Faculty of Philosophy in Rio. During that period my office routine could not intrude upon me, and I had a chance to think about the phase problem. Starting with Dorothy Wrinch's simplification of a Patterson map to a set of points, I was able to generalize her primitive result of solving the vector set based upon a triangle, and, before I left in January 1949, lectured to my class on how to solve a general vector set. The paper, typed on board ship while returning home, was sent immediately to *Acta Crystallographica*, but, unaccountably, did not appear until March 1950. By this time others were finding solutions of Patterson maps by other ingenious methods. My own viewpoint eventually lead to the broad class of image-seeking functions. The first of this class was the product function. The idea of a minimum function occurred to me as I was laboriously multiplying pairs of Patterson values to form the product function. The first minimum functions were therefore made by comparing pairs of values of the Patterson function as written down as if ready for contouring a Patterson projection of the crystal. I compared the two values at the ends of a line image as I allowed the image to range over the cell, selecting, at each location of the image, the minimum, and reading it aloud to my daughter Marla who wrote down this value in its proper place on another map, the M_2 map. This tedious procedure, after being practiced a few times, gave way to the graphical method. All this was stimulated by a visit to Rio, and I could mention other ideas developed by appropriate loafing in Florida, etc.

The precession camera was developed as I was writing the part of Chapter 10 of *X-ray Crystallography* concerned with the limited symmetry information obtainable from oscillation photographs. It was evident that the symmetry of the oscillation motion limited the symmetry of the record. Why not increase the symmetry of the motion? Precession ideally provided a radially symmetrical motion. The oscillation and precession motions are compared in a figure on p. 207 of *X-ray Crystallography*. It was easy to make the first camera, shown on p. 208, and the first photographs, shown on p. 209, justified the whole idea. All this was carried out in 1937. In 1938 de Jong and Bouman showed how to avoid radial distortion as well, and this improvement was quickly added to the primitive precession camera to give the first precession camera as we now know it. This just missed appearing in *X-ray Crystallography*, the manuscript of which was finished in 1940. It was described in *Monograph No. 1*, 1949, of the American Society for X-ray and Electron Diffraction. The availability of manuscripts of this sort brought forth the *Monographs of the ASXRED*, and these were later taken over and continued by the American Crystallographic Association.

Before World War II, most crystallographers published their serious papers in the *Zeitschrift für Kristallographie*. Editors of journals in the better recognized sciences did not always encourage crystallographic contributions. About 1939 I began to agitate among my colleagues for the establishment in the United States of a Journal of Crystallography. There was some support for the idea, but most were afraid to go ahead for fear of financial troubles, and certain societies, notably the Mineralogical Society of America, began to be alarmed at possible defection of some of its membership. Maurice L. Huggins and I corresponded about the possibility of a journal, and eventually I found myself a chairman of a subcommittee of the National Research Council Division of Chemistry charged with investigating the possibility of establishing a journal. In the end, the Americans did not want to proceed alone, but the episode drew together a group of X-ray crystallographers and this lead to their forming a society, to be called the American Society for X-ray and

Electron Diffraction (ASXRED), in July, 1941, at Gibson Island. I believe that Huggins and I pushed the rest into this.

Meanwhile there was already an active small group in Cambridge, Mass., who called themselves the Crystallographic Society, and later, the Crystallographic Society of America. My correspondence indicates that this group met as early as 1939–40. It had such speakers as Fankuchen and Bridgeman, and eventually held successful national meetings in Northampton, Mass. (21–23 March 1946), Annapolis (19–21 March 1947), New Haven (1–3 April 1948) and Ann Arbor (7–9 April 1949). This society paralleled the ASXRED, but did not tie itself down to the tool of X-ray diffraction. It held a joint meeting with the ASXRED at Yale University, 1–3 April 1948, and the two societies merged in 1950.

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