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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1990

STURGES W. BAILEY
University of Wisconsin–Madison, Madison, Wisconsin 53706, U.S.A.

President Robinson, honored guests, and fellow mineralogists:

I am highly honored to receive this award—especially so because I will join a number of former mentors and colleagues who already are on the list of Roebling Medalists and who have been important in shaping my career.

One of the questions I am most frequently asked is how I first became interested in mineralogy, X-ray crystallography, and phyllosilicate minerals. The first came easily, but it was a crooked path for the other two. As a sophomore at the University of Wisconsin I took courses in physical and historical geology from W. H. Twenhofel. Twen was a great teacher and I was soon hooked on geology. The following year as a declared major in geology I was assigned as an advisee to Alexander N. Winchell (Roebling Medalist in 1955), who placed me in his mineralogy course with R. C. (Con) Emmons (MSA President in 1944) as the laboratory instructor. It was an intensive two-semester course of ten total credits devoted entirely to minerals. I was fascinated by minerals at once. Professor Winchell was aware of this, and he assigned me to his X-ray laboratory to learn how to identify minerals by X-ray diffraction. As a senior in the 1940–1941 school year I suddenly found myself in charge of the day-to-day operation of that lab when the graduate student who usually supervised the lab left town hurriedly for personal reasons. As I delved deeper into the subject matter I became intrigued by finding out all the things that could be done with X-rays in the study of minerals. For the first time I had an inkling that it might be really neat to become an X-ray crystallographer. But it was to be another nine years before I got a chance to study toward becoming a crystallographer.

I started graduate study at Wisconsin in the fall of 1941, but Pearl Harbor and America's entry into World War II brought about a change in plans. I spent four years, 1942–1946, in the U.S. Navy as a photographic interpreter. When I returned to the University of Wisconsin in the fall of 1946, I found Professor Winchell retired, the X-ray equipment junked as obsolete, and the former X-ray lab occupied by the janitorial crew. X-ray crystallography did not appear to be a viable field for me, so I spent two years studying fluid inclusions for the M.A. degree under Con Emmons. I switched to the field of economic geology in 1948 under a new faculty member, E. N. (Gene) Cameron, and started to study for the Ph.D. degree. I am very pleased to have Gene here today as my citationist.

I still wanted to do X-ray work, however, and one day I asked Gene Cameron if the department had any plans

to buy new X-ray equipment and to hire a faculty person in that field. He asked me why I was interested, and I told him about my previous experience and interest. He didn't say much at the time. He certainly did not answer my question. But, to my surprise, a week or so later I found myself summoned to the office of the departmental chairman (R. C. Emmons). Con Emmons explained to me that they had been trying to hire a faculty member in X-ray crystallography since the end of World War II but had not been able to find anyone suitable. He then made me a proposal. If I would go to England to study with Sir Lawrence Bragg and W. H. Taylor at the University of Cambridge, the department would guarantee me a faculty position upon my return.

I was dumbfounded because I had never heard of such an arrangement and I had never seriously considered teaching as a career. But it seemed too good an opportunity to turn down. I was able to get a Fulbright Fellowship to study at Cambridge for two years. So in September of 1949 I found myself on a converted troop ship bound for England along with my new bride, Marilyn,

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who also graces your head table today. The period 1949–1951 proved to be an exciting and rewarding time that I shall never forget.

Crystallography was housed in the old Cavendish Laboratory at Cambridge, and when I arrived there I was taken to meet Sir Lawrence Bragg (Roebling Medalist in 1948). The year 1949 was the start of the Fulbright program in England, and Sir Lawrence thought it would be a nice gesture to name himself as my official adviser because I was the first Fulbright Scholar to study at the Cavendish Laboratory. Professor Bragg was head of the entire Cavendish, however, and had little time to spend on individual advice. So in practice my work was supervised by W. H. Taylor, who was in charge of the crystallography section of the Cavendish. At the start of my second year, Dr. Taylor became my official adviser. I was amazed to find entire groups of researchers within the crystallography section who were devoted to different areas of application of X-ray study. There were groups specializing in organic structures, metal structures, proteins, crystal physics, etc. W. H. Taylor, Roebling Medalist in 1979, and Helen Megaw, Roebling Medalist in 1989, supervised research on inorganic compounds and minerals. I worked with W. H. Taylor and started study toward a Ph.D. in physics with the crystal structure of an intermediate microcline as my thesis topic. Although I met once a week with W. H. Taylor to discuss my research progress, the practical aspects of crystallography were left to interaction with my fellow graduate students. My office mate J. V. Smith, Roebling Medalist in 1982, was the person who showed me how to take X-ray single crystal photographs and interpret them. Anything I don't know I attribute to his negligence.

By the fall of 1951 it was evident that a three-dimensional structural refinement using film techniques and prototype computers would take longer than the two years available. I had bitten off more than I could chew, but I received permission to finish my thesis at Madison. Back in Madison, I used my spare time for the next three years trying to cope with the primitive computing facilities available and to complete the thesis. I must admit that my enthusiasm for three-dimensional structural refinement was blunted by the excessive labor required at that time. This helped divert my attention away from crystal structural refinements for several years. But it is also true that my interests were changing at the same time.

The chairman of the Geology Department at Wisconsin in 1951 was Stanley A. Tyler, who invited me to join him in a study of the clay minerals associated with the Lake Superior iron ores. This meant that he would supply the samples, and I would do the work. But it proved to be very interesting work. The clay mineral assemblage was very diverse, and it included several chlorites and aluminous serpentines whose X-ray patterns were not in the ASTM powder diffraction files. This interested me, and I immediately boned up on clay mineralogy by reading the 1951 monograph of the Mineralogical Society of London devoted to that subject. The classic study of the polymorphism of the micas by Smith and Yoder appeared in 1956. It was evident to me by that time that the unidentified serpentines and chlorites in the Lake Superior clays were polymorphs, or polytypes as they then were being called, of the more common species. So in 1957 I derived the 12 standard polytypes for 1:1 layer serpentine-type structures by a similar procedure. These results were not published until 1969 because our computer was not big enough to calculate the X-ray powder patterns for six-layer structures. The important point, however, is that the calculated patterns for some of these theoretical serpentine structures fit perfectly those obtained from some of the clays in the Lake Superior ores. This hooked me for good on clay minerals, and I followed up with a similar derivation of the six chlorite structural types in the period 1960–1962. Here also some of the "misfit" chlorite patterns from clays in the Lake Superior ores turned out to be those of previously unrecognized structural types. This was very exciting and satisfying for me. By this time I was devoting most of my research efforts to phyllosilicates. I had become intrigued by the geometry of tetrahedral and octahedral networks. I thought it was great fun to work out the resultant symmetry of several superimposed layers and to find the subgroup symmetry imposed by different patterns of cation ordering. I was also curious about the relationship of these polytypic layer sequences to composition, temperature of formation, cation ordering, and overall crystal chemistry. Sometimes I feel that I don't know much more about these relationships now than when I started. There are many unsolved problems waiting for the next generation.

With improvement in computing facilities by the early 1960s, I turned again to careful structural refinement of those phyllosilicate species for which suitable crystals could be obtained. Although I continued to direct student theses on structures of potassic feldspars for several years, my primary interest was in phyllosilicates. I was encouraged to concentrate on phyllosilicates primarily by three people—W. F. (Bill) Bradley (MSA President in 1970), George Brindley (Roebling Medalist for 1970), and Marion L. Jackson (Professor of Soil Science at Wisconsin). All three pointed out to me what a worthwhile contribution could be made by a trained crystallographer in this area, where little previous structural work had been done, and all of these men were very influential in the development of my career. I took their advice and have never been sorry.

In conclusion, I want to emphasize how important my graduate and postdoctoral students have been in my research. They have done the hard work of finding good crystals, not an easy undertaking for phyllosilicates, and of refining many of the structures. I have had the fun of working out the crystal chemistry implications of each structure and of writing up the results for joint publication. I have been fortunate in having had a succession of bright and dedicated student colleagues with whom to work over the years. I am pleased to accept this Roebling Medal on their behalf as well as my own.