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Acceptance of the Roebling Medal of the Mineralogical Society of America for 2000

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Mr. President and members of the society:

I accept the Roebling Medal with gratitude and humility. Gratitude to the American Mineralogical Society for honoring me, and gratitude toward my colleagues who supported my nomination. I am humbled when I examine the list of names of past Roebling medalists, and when I conside others who are also deserving but have not yet been selected. I hope their time comes soon.

I was raised in the small town of Dalton, Pennsylvania, and attended Dalton High School. My first interest in science goes back to a Christmas when I was perhaps 10 years old. A friend of mine had received a Gilbert Chemistry set for Christmas and he invited me to his home to examine it. I was hooked, and over the years of my high shool education, I continued to build up an impressive chemistry lab at home. This was made possible by the encouragement of our school principal, Mr. Lippert, who allowed me to add my lists of glassware and chemicals to periodic orders that the school placed.

My father was away in the army from 1941 to 1945 and thus was not present in our household during my teen age years. As an only child, I must have been a handful for my mother who nevertheless managed very well in my father's absence. I graduated in 1945, and after a partial semester at Penn State University, was drafted into the U.S Army Air Corps in the fall of 1945. I spent about one year of a two-year period in occupation duty in Japan where I played trumpet in an Air Force Band. I was discharged in 1947 and attended Keystone Junior College for two years, and Lafayette College where I graduated with a major in geology in 1950.

At this point, I was moving with little enthusiasm through a chemistry major. During my junior spring term, I had wistfully watched a friend go on geology field trips during beautiful spring days, while I was stuck in a dreary Physical Chemistry Lab. In the fall of my senior year, I took a beginning geology course and rediscovered my enthusiasm for science. I realized that I had found my future, that I could handle the material and enjoy it. I completed a geology major in my senior year because the necessary collateral courses had already be taken as part of my chemistry major.

Two people stand out in their influence on my career at this point. The first is Benjamin Tripanni at Keystone. He taught mathematics, and taught it very well. I was beginning to see that my interest in science, notably chemistry, was coming into conflict with my inability to handle mathematics. Tripanni's course showed me that I really could do math and the subject never bothered me again. The other important figure in my life is Yngvar Isachsen. We go back as friends for 50 years. Yngvar taught mineralogy at Lafayette was (and is) an enthusiastic exemplar of scientific liberalism—he is a person with a broad knowledge of most things, and one who expresses himself with humor and consideration. I had never met anyone like him.

I married my wife RoseAnn in 1950, graduated in 1951 and we moved to St. Louis where I enrolled in the Ph.D. program at Washington University. In those days I was interested in igneous/metamorphic petrology, and I did a laboratory thesis under A.F. Frederickson on the metamorphism of olivine gabbros from the south coast of Norway. I completed my graduate work in 1955 and went to work at the Stanolind oil company's research center in Tulsa, Oklahoma. John Hower, who I met in graduate school and with whom I quickly became a close friend, also went to Stanolind as did Frederickson who was hired to start a new research group in the company that included Hower and me.

There was not much application for igneous/metamorphic petrology in an oil company research laboratory, so I transferred my interests to sedimentary petrology and the geochemistry of the clay minerals. I worked with Hower in the same lab, where I learned clay mineralogy—sample preparation, X-ray powder diffraction pattern interpretation, etc—on the job from Hower who had done a Ph.D. dissertation on clays at Washington University. My own research at Stanolind concerned the boron content of illite and the salinity of the waters in which the illite formed.

I had always wanted a teaching position, so after five years in Tulsa I moved to Dartmouth College in Hanover, New Hampshire, where I remained until my retirement in 1997. in 1962 I obtained an equipment grant from the General Electric Company and a National Science Grant to study the concentration of boron in Precambrian seas. These grants enabled me to build a sample preparation lab and obtain a General Electric XRD 5 diffractometer/spectrometer. At that time, I was interested in X-ray fluorescence and wrote two papers on the use of Compton scattering for matrix corrections in quantitative trace element analysis. This work was a minor effort compared to my research on the boron content of illites from Precambrian rocks which lead to several publications.

The boron work was completed in the mid-1960s and then I became associated with the U.S. Army Cold Regions Research and Engineering Laboratory (CRREL) in Hanover. They employed me as an expert and supplied off-term salary and equipment/travel for any work involving clay minerals that lead to publication in refereed journals. This support was provided through the good offices of James Bender and Duwayne M. Anderson. It was an ideal arrangement for me. No reports or proposals to write, just pick out problems in clay mineralogy, attack them, and publish the results. During this period, I wrote on the structure of ethylene glycol and ethylene glycol monoethyl ether on montmorillonite, two papers on the bentonites of northern Alaska, and my results on the calculation of one dimensional diffraction patterns from mixed-layered clays, but the latter was only the beginning of a career-long effort on that subject..

Dartmouth College was the place to be in the mid 1960s for anyone interested in computing. John G. Kemeny and Thomas E. Kurtz had developed the basic language and implemented it on a college-wide computer time-sharing system. In effect, then, we had, 35 years ago, the qualitative capability of a modern PC for the creation and execution of computer code that solved numerical problems. It was just at this time that I decided to write a computer program for mixed-layered clays based on the theory published by D.M.C. MacEwan. The program would correctly incorporate the structure factors for the two kinds of interlayer material present. It seemed like a reasonable enough proposition, but in fact, it required two years of hard work.

It quickly became apparent that my mathematics background was not up to the task. I turned for help to Christopher Goetz, who was a geophysics graduate student who worked at CRREL during the summers. He quickly understood MacEwan's approach, though he knew nothing about X-ray diffraction theory, and proceeded to tutor me so that I could program it correctly. But I was a neophyte programmer, so I had to concentrate on that aspect also. Finally, however, I had code that withstood all of the error checking procedures that I could think of. I wrote a paper on the subject and sent it to *American Mineralogist*. It came back rejected because the single reviewer did not agree with the basic principles on which the mathematical development was based. I sought advice from other people in the field. A copy was sent to George Brindley who approved of it as did his visitor V. Vand. I requested additional reviewers from the *American Mineralogist*, and on the basis of two favorable reviews, the paper was published in 1967.

The paper attracted almost no attention. It must have seemed to many to be a complicated attempt to get the kind of information that was available using the closed-form equation of Hendricks and Teller, despite the fact that that equation produced calculated diffraction patterns that bore little resemblance to real ones. In addition, the question or ordering never came up with the Hendricks Teller approach, because it a was assumed that all of the mixed layered minerals were randomly interstratified. But a break-through came from my association with John Hower.

Hower and his graduate student, Edward Perry, were working on burial diagenesis of shales in the Gulf Coast. They had numerous high quality diffraction patterns of mixed-layered illite/montmorillonites arrayed down bore holes but could not make much sense out of the data. The patterns did, however, correlate almost perfectly with my calculated ones. I wrote a paper with Hower in 1970 in which comparisons were made between observed and calculated patterns, for ordered and random interstratifications, and that paper established the validity of the calculations. At that point, my program for calculating diffraction patterns was informally distributed as a series of Mod versions.

On the basis of my work, Brindley asked me to write the chapter on mixed-layered clays for a forthcoming British Mineralogical Society Monograph This project occupied a good portion of my time in 1978 and 1979.

During the 1970s I became involved with the Hubbard Brook Ecosystem Project with N.M. Johnson and G.E. Likens. This association lead to studies with Johnson on the rate of chemical denudation in the alpine zone of the Cascade Mountains. We obtained the surprising results that clay minerals were actively forming in that glacial environment and that the rate of chemical denudation exceeded that measured in New Hampshire. I also became part of the Lake Powell Project, with C.L. Drake and N.M. Johnson, and studied the kinetics of calcite precipitation in Lake Powell with the finding that precipitation rates are markedly inhibited by dissolved polyphenolic material from plant decomposition. In the 1980s, I continued my work on clay mineralogy with a paper on the Lorentz Polarization factor and co-authored two papers with Paul Nadeau based on his studies on the burial metamorphism of the Mancos Shale. I published a second paper on the Lorentz factor, coauthored a paper with Sterne and Zantop on ammonium illites, and derived a mathematical basis for calculating quantitative diffraction intensities from mixtures of simple and mixed-layered clay minerals. I also published results from two studies of the K/T boundary layer, one with with M.J. Johnsson and the other with M.R. Rampino, and a kinetic study, with A.M. Pytte, of the transformation of smectite to illite.

In 1985 I decided to write a user-friendly version of my Mod programs for execution on Macintosh machines. I wrote it in True Basic because that language was exportable from the Macintosh to the DOS environment. I named the program NEWMOD, to indicate that it superceded all of the earlier Mod versions, and have had excellent success with its distribution worldwide. At present, there is also a Windows version.

In the late 1980s, we started work on the chlorite minerals. Graduate students M.H. Hluchy and J.R. Walker did Ph.D. dissertations on chlorites as did P.C. Ryan, more recently. I coauthored a paper on mixed-layered serpentine/chlorite with M. Distefano and R.W. Lahann and in 1999, another with H.E. Roberson and D.M. Jenkins on the hydrothermal synthesis of corrensite. But the late 1980s were times of writing for me. I wrote three chapters for two of the MSA short courses, Reviews in Mineralogy, and a textbook with D.M. Moore on the X-ray identification and analysis of clay minerals.

A chance discovery with C. Thomson disclosed that the illite from the Potsdam sandstone of New York had the *cis*-vacant structure and thus was in space group *C2* instead of the common *trans*-vacant mineral which is referred to *C*2*/m*. We had noticed this unusual type of diffraction pattern in K-bentonites and ascribed it to the polytype 3*T*. But now we had a series of K-bentonites which showed diffraction patterns that were intermediate between the two end member structures, and a tool was necessary to quantify them. So, in 1989 I started work on a computer program called Wildfire, which calculates the three-dimensional diffraction patterns from micas with variable rotational disorder and variable proportions of *cis* and *trans*-vacant layers. The algorithm was published in 1992, and was used by D.K. McCarty in his studies of Paleozoic K-bentonites from the eastern U.S.

The last few years have been spent on a second edition of my textbook with Moore, collaborative work with G. Guthrie and D.L. Bish on the diffraction characteristics of opal; with Guthrie on the differences between electron optics and X-ray optics in registering optical coherence between slightly disordered crystal segments; and acting as a clay mineral advisor to research that uses isotopes in soil clays to document climate change in mountainous regions. I continue to work on a version of Wildfire that will be incorporated into a Rietveld package for the determination of disorder parameters in micas and kaolinites.

My career has benefitted from a stable marriage of 50 years and still counting, and three children who are well adjusted and getting along well in life. My wife, RoseAnn, worked with me as a laboratory assistant for seven years in the field of Water Chemistry. Except for this period, she was always home for me and our children, and this left me free to pursue my career wherever it lead. I have had bright and interesting colleagues at Dartmouth and elsewhere for discussions and arguments both technical and nontechnical. I think of Herman Roberson, Dewey Moore, Dennis Eberl, Dave Pevear, Paul Nadeau, John Hower and David Bish from my non-Dartmouth connection. Chuck Drake, Noye Johnson, John Lyons, and Dick Stoiber have been my closest associates from the Dartmouth Geology Department, but all of my colleagues at Dartmouth have contributed positively to my 40-year association with that enlightened and first-class institution.