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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1996

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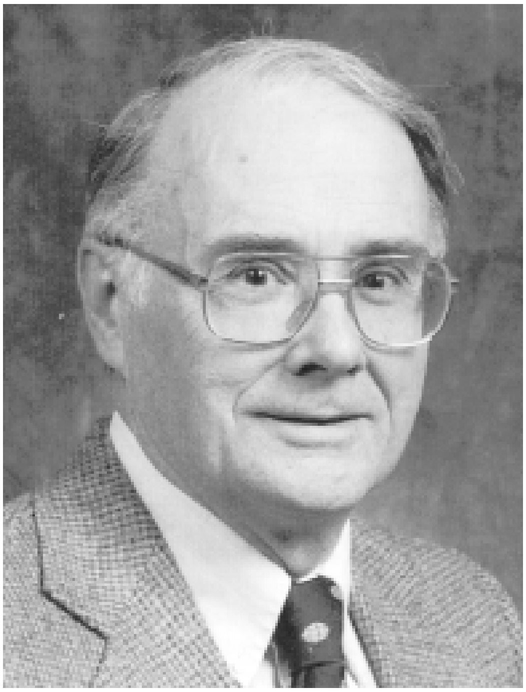

President Brown, ladies, and gentlemen:

As I looked over the list of previous Roebling Medalists, it was obvious that our Society saw special attributes in each of them. I concluded that today, MSA is recognizing my special ability to form productive alliances with people who in many cases are better scientists than I, and who in all cases have interests and abilities that are highly complementary to my own. Many examples will come out later in my talk—but as I cannot include of all who have contributed to my success, I apologize in advance for those I omit.

I am the second of five sons of a chemist and a musician turned (nearly) full-time homemaker. My interest in science was enhanced both by my father's encouragement and by the fact that my mother was so busy with my younger brothers that she had little time to spend checking up on me. As a result I was rather free to indulge in experiments of all sorts with little parental interruption. For example, I performed my first mineral synthesis when I was eight; the apparatus was a glass vial and my mother's stove. I tell only my closest friends that the phase was a sulfide, the starting materials being lead foil and sulfur! I was building a crystal radio, and it turns out that synthetic galena is far superior to the natural product as a semi-conductor. Had I been an only child, the origin of the resulting mess on the stove would have been obvious, but I managed to escape unpunished with a fine "crystal" for my radio.

From 1941 until 1949 we lived in and near Charlottesville, Virginia, and although I didn't realize it, much of my future was cast there. The University of Virginia lay between our home and my elementary school, so twice a day I walked through the campus. Coming home I rarely missed the opportunity to check the dump outside the Geology building. I could never understand why anyone would throw out fabulous millimeter-sized fragments of minerals, and I zealously collected them. (I was just as efficient at losing them; perhaps my mother considered them to be dirt and helped in that process.)

When I was 12, my father arranged for me to have a summer job in the Chemistry Department at the University of Virginia. One of my tasks was to help grow single crystals of copper, machine them into spheres, and then oxidize them under controlled conditions. The results were spectacular. The oxidation rate depends on crystal orientation, and each thickness of copper oxide produced a different color! The cubic symmetry was obvious, even to a twelve-year old: cube "faces" were blue, octahedra red, dodecahedra yet another color. With the help of a grad student, I learned to assign the Miller indices to the various orientations. One problem with the experiments was that the University of Virginia chemistry building lacked traps in the lab sinks. Someone on the top floor routinely poured waste H_2S down the drain and as a result I not only had to get used to the smell of rotten eggs, but also had to protect my precious oxidized copper spheres from becoming corrupted by sulfides. Perhaps my later love for oxides and distaste for sulfides was born that summer!

That same summer marked me in a different way as well. While I enjoyed the chemistry I was captivated by physics. I had a ride home with a physicist, and as he departed an hour after my chemistry job ended, I hung out in a physics lab from four to five each afternoon and was fascinated. It was a centrifuge lab, dedicated to building better and faster centrifuges. Since one aspect of the work involved spinning the rotors ever faster until they disintegrated, I learned a lot about protective shielding that summer, knowledge that came in handy when I later began to build high-pressure apparatus.

All through high school and into my second year at Princeton I intended to become a physicist. But on a hike in western North Carolina after my freshman college year, my father and I made a deal that changed my life. We agreed that he would learn about flowers and trees; I was to learn some geology, and then we would each educate the other as we hiked. Accordingly, I enrolled in Physical Geology that fall. I began to be hooked, and I can only describe my feelings as falling out of love with physics and into love with geology. Ironically, my father was dismayed when I changed my major, as he couldn't believe that geology was a "real" science worthy of being embraced by one of his sons! But then when he was in college, geology was the major of choice for football players, perhaps because sociology hadn't been invented yet.

I enjoyed all my geology courses, but I especially liked Dick Holland's Mineralogy and Arthur Buddington's Petrology. Bud finished the formal course a day early, so his last lecture was on his current research: his new magnetite-ilmenite geothermometer. His calibration was purely empirical, but as his assumption of solid solution between magnetite and ilmenite seemed not unreasonable, I was intrigued. I didn't know it yet, but the direction of my career had been set. During my junior and senior years, I had a high security clearance and made a map of the Pacific Ocean Basin for Harry Hess. We couldn't publish it, for in his guise as Admiral Hess, Harry had convinced the Navy that the information was essential for navigation of submarines! I like to think that with that map I played a minute part in Harry's development of the concept of sea-floor spreading.

When I entered Johns Hopkins for graduate work in the fall of 1956, my past helped determine my future. That previous summer Aaron Waters had spent some time with Keith Runcorn collecting paleomagnetic samples of Columbia River basalt. Aaron was intrigued by rock magnetism but also deeply skeptical, a skepticism that was unfairly enhanced by Runcorn's evident ineptitude with a Brunton compass. Aaron wanted a student to do a combined petrologic, mineralogic, and paleomagnetic study of his beloved basalts. I had had one more physics course than any other entering student at Hopkins, so I was drafted for the task. It was great fun. I got to roam the Spray Quadrangle in Oregon, mapping, measuring sections of the Picture Gorge Basalt, collecting oriented samples for later magnetic and petrographic analysis, and coincidentally collecting fossil leaves and vertebrates from the underlying John Day formation. My dissertation includes 18 vertebrate and three plant species along with the expected petrology, mineralogy, and rock magnetism. Petrographic study of the basalt samples reminded me of Buddington's lecture on the oxide minerals and geothermometry.

I became convinced that I needed to know more about the oxide carriers of rock magnetism, so I applied for and received a post-doctoral fellowship at the Geophysical Lab to do experiments on Fe-Ti oxides. Here I was fortunate that Hans Eugster had just developed solid-solid oxygen buffers, which turned out to be essential for my work. I decided that I needed to understand the equilibrium conditions of the oxides before I could attack their magnetic properties-and my career was set: I never went on to the magnetic aspects! One thing bothered me; try as I might, I could never make solid solutions between magnetite and ilmenite, as Buddington and most other mineralogists of the time were convinced must be the case. However, V. M. Goldschmidt had pointed the way. On crystal-chemical grounds, he doubted the likelihood of such a solid solution, and in 1928 had predicted both the existence of the component Fe_2TiO_4 and the possibility that its oxidation while in solid solution with magnetite would produce the classic "trellis" ([111]) pattern of ilmenite lamellae in magnetite that Bud had assumed resulted from simple exsolution. I tested this prediction experimentally and succeeded in making the trellis pattern by oxidation of ulvöspinel! With help and encouragement from Al Turnock and Dave Wones, I came to realize that while Bud's thermometer was fatally flawed, I had the tools and the data not only to place it on firmer ground with precise calibration, but also to extract information on the oxygen fugacity as well. All I needed was a set of well analyzed magnetite-ilmenite pairs on which to apply the new thermometer-oxybarometer. Once again fate intervened: I was asked to review a new manuscript by Buddington, one that was based on what I now knew to be an erroneous assumption, but that was also full of the analytical data that I needed. I could have torn the manuscript to shreds, but anyone who knew Bud would never treat that magnificent gentleman in such a shabby way. So I—a brash young upstart still in his twenties—wrote to Bud (then in his seventies) suggesting that we collaborate: all he had to do was admit the error of his ways and we could make petrologic history together. With bated breath I awaited his reply. It came: "Sure—if you can convince me of the oxidation origin." I sent him a photo of my experimental "trellis" intergrowth. He replied: "You win—let's write the paper!" My admiration for Bud, already high, went through the stratosphere. Here was a scientist, secure in his fame, who would far rather search for the truth than to defend his own turf. It is a lesson I try to remember always.

My later years at the Geophysical Lab were marked by fruitful collaborations with Steve Haggerty on oxides, and Joe Boyd, Jim Munoz, and Doug Smith on pyroxenes. I never worked directly with Hugh Greenwood, but I also learned a great deal from him, both at the Lab and later when I visited the University of British Columbia. My Lab years were also when I began a series of strong personal friendships and fruitful collaborations with crystallographers, Charlie Burnham and Larry Finger at the Lab, then Jim Papike, Charlie Prewitt, and now Rich Reeder and John Parise at Stony Brook. I am grateful for their tolerance of my antediluvian views of crystal chemistry and their readiness to help me whenever needed. In return, I have provided them with more than a few good crystals for their own work.

Since I came to Stony Brook in 1970, fruitful collaborations continued. I traveled through the solar system with John Delano, my first Ph.D. student, and continued phase equilibrium studies of pyroxenes with John Grover, Tim Grove, and Al Turnock. Paula Davidson, Ben Burton, and David Anderson educated me about the thermodynamics of pyroxenes and oxides. Late in the 1970s I gave a talk at the University of Wyoming, and having a good three hours to kill before my plane left, Ron Frost and I visited the Laramie Anorthosite Complex. I became a born-again field geologist. From this came a 15-year collaboration with Ron (and later Carol Frost) on the Laramie rocks. Numerous students also played an important role, among them Mimi Fuhrman, Al Kolker, and James Scoates. Not only were the field-related problems fascinating, but the questions raised by the field work provided the impetus for virtually all the new experiments in my lab as well. One key result of the collaboration with Ron has been the development of the QUIlF relation between Fe-Ti oxides and ferromagnesian silicates. Ron proposed the equilibrium Quartz + Ulvöspinel = Ilmenite + Fayalite to explain the absence of magnetite in some of the late-stage rocks of the Laramie Complex. We realized that the thermodynamic solution models being developed for pyroxenes and olivine by Paula Davidson and for oxides by David Andersen could be merged to provide a quantitative basis for the many equilibria possible among the oxides, pyroxenes, olivine, and quartz. David produced the QUILF program that many of you use. Dimitri Xirouchakis is extending this work by adding titanite to the assemblage.

As the years went on and I might have entertained thoughts of slowing down, a new colleague assured that I would have no chance to do so. Hanna Nekvasil and I are interested in many of the same aspects of petrology, and the combination of her quantitative, rigorously thermodynamic approach and my more intuitive, geometric approach has led to many heated but friendly arguments that are usually resolved when we realize that we are talking about and arguing in favor of the same thing! We can bare our intellectual souls without fear that the other will say “that’s a stupid idea.” Thus we are free to try out hypotheses and ideas we wouldn’t dare expound to someone else. It is my hope for you that each of you can develop an intellectual association at once so pleasant and so enlightening.

Finally I come to the longest and best collaboration of all. The most visible fruits of that collaboration are named Glenn, Janet, and Bruce. The collaborator, of course, is their mother and my wife of 37 years, Carol. As before, this collaborator deserves the lion’s share of credit for the project, in this case three offspring that any parent would be inordinately proud of. In addition, I would not be standing here today if it were not for her love and support for these many years. Carol, I thank you! And, ladies and gentlemen, on behalf of all my collaborators and myself, I thank you with all my heart. I am immensely proud and deeply touched by the honor you have bestowed upon us.