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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1986

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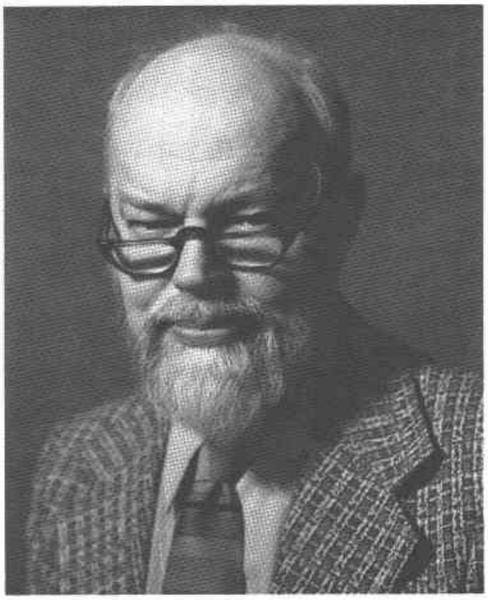
President Barton, Members of the Mineralogical Society of America, and Guests:

First let me thank Brian for his kind, but to various degrees, apocryphal remarks.

Last year my wife, Kathleen, and I spent about two months going around the world, highlighted by four weeks in the People's Republic of China as the guests of the various institutes we visited. Fully expecting to encounter a psychological letdown on returning to normalcy, the first piece of mail awaiting me at home was the completely unexpected announcement of this award. The committee's security arrangements concerning its deliberations were certainly leakproof. As a life-long mineral collector, I am particularly happy to be the beneficiary of the generosity of an even more avid collector, Washington A. Roebling, to whom this Society owes a great debt of gratitude.

It is instructive to review the work of the previous recipients of this medal, especially in terms of how they fit into the picture one may have of a "typical" mineralogist. The MSA holds the reins on a "troika" of subjects, mineralogy, petrology, and crystallography, but even so, most of these medalists worked in fields that were, to a large degree, peripheral to mineralogy; their work either made use of mineralogy or was useful to mineralogy, but only in a relatively few cases would it constitute a major part of a standard mineralogy textbook. This reinforces the increasingly obvious conclusion that mineralogy is a truly interdisciplinary science. My own work has been mainly in two fields, also peripheral to standard mineralogy: silicate-melt equilibria and fluid inclusions.

I first got interested in silicate-melt equilibria and phase diagrams as a result of my five years of applied petrography in the Research Department of the Bethlehem Steel Corporation, where I worked, during the war, mainly on what might be termed synthetic rocks—refractories, slags, and nonmetallic inclusions in steel. After the war I started graduate work at Columbia University, and my professor, C. H. Behre, Jr., arranged for me to do my doctoral research at the Geophysical Laboratory in Washington on a Fellowship. This was a wonderful opportunity, indeed, and Professor Behre was anxious for me to work out the phase diagram for a system of great and immediate interest to his field of economic geology. This was the system Fe-O-H-S. My Geophysical Lab advisor, J. Frank Schairer, wisely decided that I should do the silicate system K_2O-MgO-SiO_2 instead. This was indeed wise,



because I might still be working on Fe-O-H-S! After all, this was 1947, long before the important discoveries of the many ingenious experimental techniques so necessary for such corrosive, volatile-bearing and pressurized, polyvalent systems.

There is an amusing anecdote concerning the system K_2O-MgO-SiO_2. I had only one year to do my thesis, and so I had to work fast. The year was well along before it became apparent that a large number of my data points for various compositions in the most important and complex central part of the system seemed to be mutually contradictory. Schairer had advised me at the start to use integral molar ratios of the three components in making most of the compositions in the system, "in case any of these turn out to be actual compounds." One of these batches was synthesized to have the composition $K_2O \cdot 5MgO \cdot 12SiO_2$. After much more work, it finally became evident that something had to be wrong with that particular batch, as everything else had started to fit together. With only a few very hectic weeks left in the fellowship, I made up a new batch of composition 1 to 5 to 12. I found this new batch to be very different in phase behavior from the original batch. Its phase changes, although complicated, fitted perfectly with all the other data on nearby compositions, and it was, in fact, a new compound. (Parenthetically, I must add that I never did find out what was wrong with the original batch, or why the famous experimenter's friend "Murphy" picked *that* composition to be the one erroneous synthesis, but he could not have picked a better one.) In any case, the new compound was very interesting. It forms beautiful, highly modified hexagonal crystals that melt multiply incongruently, first to clinoenstatite and liquid, then to forsterite plus liquid. I predicted that it should occur in nature, but unfortunately its optical properties are exactly those of quartz, and what petrographer will bother to dig out and X-ray a grain that is uniaxial positive, with indices of 1.545 and 1.555? The resolution of the 1 to 5 to 12 puzzle was made even more pleasant when this phase, which caused me so much loss of sleep in those last weeks, was actually found in meteorites, and later in volcanics, and was named roedderite.

Most people in my position today are able to point to a single individual who started them in their careers in mineralogy. Unfortunately, I cannot. My mother tells me that when I was in 4th grade, someone gave a talk about minerals at my Philadelphia school. I don't recall a thing about it, but she remembers that I was enthusiastic about collecting pretty stones from then on. I do remember, however, that when I was in elementary school she gave me a twin-lens folding pocket magnifier that I carried for years, and she took me on numerous car trips to visit mineral-collecting localities from Maryland to New England. I was also lucky to find a copy of Dana's delightful 1897 beginner's book, *Minerals and How to Study Them* in our small local library. The kindly librarian got tired of renewing it month after month, and finally gave it to me. I was particularly fortunate that many famous southeastern Pennsylvania localities were within bicycling range: I could ride out 50 to 60 miles, collect for several hours, and limp home that night with a load of rocks. Unfortunately, this commonly meant a climb at the very end, as we lived up on a hill overlooking the Chester Valley. From memory, that was a long, hard climb, but the topographic map says it is only 100 meters vertical rise.

I also remember very clearly one incident from my undergraduate days at Lehigh University in 1940 that had a lasting impact. My petrography professor, D. M. Fraser, showed me a strange phenomenon in a slide of Precambrian quartzite—the so-called "Brownian" movement of the tiny bubbles in the fluid inclusions. The fascinating idea that these little bubbles had been bouncing around within their prison cells for hundreds of millions of years got me started on fluid inclusions. It was not until 25 years later that I realized that the bubble movement was actually *non*-Brownian in nature and was even more interesting as a result.

In switching back and forth among immiscibility and silicate-melt equilibria, fluid inclusions in ore deposits, lunar and meteoritic samples, volcanos and salt beds and the problems of atomic waste repositories, I have worked with or had help from so many people that it is not only difficult but perhaps unjust to single out names of those to whom I am most indebted. Professor B. L. Miller of Lehigh made it possible for me to leave the teacher's college I had attended for three years and come to Lehigh University in geology. Professor Behre got me into the Geophysical Laboratory as their first predoctoral fellow, where I could actually talk with legends such as N. L. Bowen; that year was truly the opportunity of a lifetime. J. Frank Schairer's patient and humorous guidance of my thesis work at the Geophysical Lab was unforgettable. It was a serious shock to learn recently that the Geophysical Laboratory's very existence has been threatened by a prospective consolidation with another department.

In more recent years, I must give special thanks to my many colleagues, both at the USGS and outside, with whom I have coauthored papers (or who have graciously added my name as coauthor on their papers). These include, to name just a few, in the usual alphabetical order, Paul Barton, Harvey Belkin, Phil Bethke, Bob Bodnar, I-Ming Chou, Masahiko Honda, Dave London, Greg Rosasco, Masakatsu Sasada, and Paul Weiblen (and 18 years ago, even Brian Skinner himself!). I have been fortunate indeed in having such friends. I have been even more fortunate in having a very supportive colleague of even longer standing, 41 years to be exact—my wife Kathleen.

In acknowledging and detailing the generous help I have gotten from so many individuals over the years, I have almost forgotten another major source of help. This is *money,* for pure and not-so-pure science, from the federal government. Before coming to the Survey (in addition to the Carnegie Fellowship), I was the fortunate recipient of a series of research contracts and grants from the Office of Naval Research, the U.S. Army Signal Corps, and the National Science Foundation. These supported my postdoctoral research on silicate immiscibility and other phase studies, and on fluid inclusions, both at Columbia and during my five years at the University of Utah. Following that, I have received even larger amounts of federal money in the form of 31 years of Survey salary and laboratory support. Obviously, this work could not have been done without such support, and it makes me wonder how the younger generation will fare in the future, with the increasingly ominous clouds of Gramm-Rudman-Hollings overhead, and the increasing percentage of a decreasing total federal research expenditure going to applied (and particularly military) research.

In closing, I would like to say that some 35 years ago I started giving what I call "missionary talks" for the cause of fluid inclusions. Now a growing number of "converts" is spreading the word, and their good works have attracted many others into the fold. It is this rapidly expanding body of inclusion work that has been so useful to the science of mineralogy, and hence it gives me great pleasure to accept this medal on behalf of all fluid-inclusion workers. Thank you all.