

## Acceptance of the Roebling Medal of the Mineralogical Society of America for 2001

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Mr. President, members of the Society, and guests:

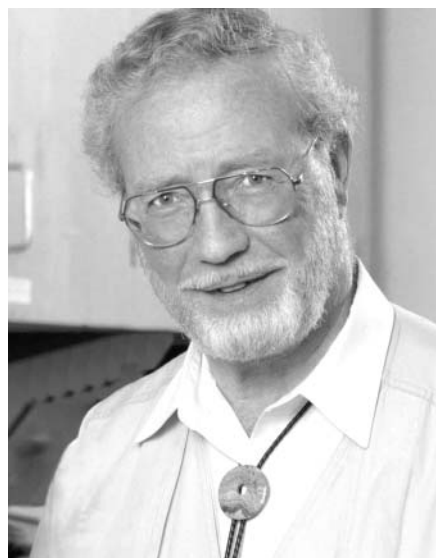
I am deeply conscious of the honor involved in receiving this Medal, and I thank the Committee and the Council for their decision, which is always a difficult one. And I thank you, Art Montana, for your generous introduction

It is very satisfying to have the research generated in my laboratory by more than 20 students and 20 research associates recognized in this way. I wish that I had space here to acknowledge them individually for their contributions—without their enthusiasm, dedication, and polished finger-tips, ideas would not have been translated into experiments, nor experiments converted into phase diagrams, and phase diagrams would not have been interpreted into petrological processes. They all deserve a share of the Medal, and none more that “Art the dart,” who had my lab buzzing loudest during his period with me, about 35 years ago. I must also thank NSF for starting me off with a laboratory as soon as I knew how to write a proposal, and for the years of support since then. The NSF policy of giving young scientists early independence is a great strength of our community.

This is the fifth time I’ve been on this platform: in 1965 to receive the MSA Award, in 1972 to present Art Boettcher (now Montana) for the MSA Award, in 1978 as President of MSA, and in 1982 to present Joseph V. Smith for the Roebling Medal. This visit has to rank as the best, and I am especially pleased to share it with my wife, Romy, and my children Lisa and John, dining over there while their spouses mind their children at home. Today is a very special day—it is Romy’s birthday. Now there’s a happy coincidence.

When I went to the University St. Andrews in Scotland, home of my ancestors, I took freshman geology as an extra subject because my father had suggested that I check it out. Before marrying, he had spent a couple of years in the Venezuelan jungle as a “Camp Boss” with Shell. He told me that geologists became his best friends, and perhaps there might be something special about the subject. In fact, I think his best times were probably in bars between the field excursions. In graduate school at Penn State, some years later, we all agreed that geologists do have more fun.

I think of myself as a geologist who entered the field because I wanted to travel and see every mountain range in the world. But my early research involved real mineralogy. My first study of minerals, after freshman geology, was making pencil rubbings while dangling down the walls of crevasses in



the Rink Glacier, West Greenland, to record the size distribution of ice crystals. The results occupied a single paragraph in my first publication 50 years ago, in 1951, co-authored with my professor and expedition leader, Harald Drever. He had taken me on the summer expedition to mountains and glaciers, and enjoyment of the assistance of Greenlander hunters with kayaks and fresh raw seal meat, in order to entice me from Physics into Geology. He succeeded.

My 1958 Ph.D. thesis was a petrographic study of a picrite sill that fused Torridonian sandstone into buchite. For my 6th publication, in *American Mineralogist*, 1959, I reported 2V measurements of more than 100 olivines, using the universal stage which was then the standard instrument to determine olivine compositions. The results showed that a single measurement of 2V for a magnesian olivine could be off by 3°, or 6 mol% Fa, because of the high angle of tilt of the sphere. I haven’t seen many universal stages since the electron microprobe vaulted into the analytical scene. In 1963, my 26th paper included the lengths and widths of 2011 olivine crystals measured in thin sections from a picrite sill. I interpreted the results in terms of continuous crystallization from a melt at a time when picrite magmas were taboo. Bowen’s primary basalt rep-

resented the maximum magnesium in magmas, until the Viljoen brothers in 1969 compared earlier scattered reports of ultramafic volcanic lava flows with their Barberton rocks and ushered in the komatiite frenzy of the 1970s. Not until Bruce Marsh tackled crystal-size distributions many years later did I realize how amateurish was my analysis. These three studies brought me very close to minerals.

My mentor at St. Andrews, Harald Drever, who arranged for my first expeditionary field work (which qualified me for 2 years behind a husky dog sledge with the British North Greenland Expedition, 1952–1954), then arranged for me to join Frank Tuttle as Research Assistant in his new high-pressure laboratory at Penn State. This appointment was facilitated by William S. MacKenzie, the feldspar man, a former St. Andrews student of Drever who had worked with Tuttle at the Geophysical Laboratory, after being TA for my first mineralogy class at St. Andrews and then showing me rocks in West Greenland. Drever hoped that I could prove experimentally the existence of picritic magmas. In 1956 I landed instead on the granite problem, because the apparatus could not reach sufficiently high temperatures for picrites. We were to explore the effect of volatile components in addition to water on the melting of granitic rocks. I shared an office with Bob Fudali who was working on the Residua System, and a third seat was occupied one year by visiting Professor C.E. Tilley who enjoyed grunting at our run samples under the microscope. Tuttle and I also managed to melt calcite at moderate temperatures, which opened up another line of investigations involving carbonatites, alkalic igneous rocks, and kimberlites. I learned about real carbonatites and kimberlites at Leeds University (1959–1961) from Barry Dawson and Pete Nixon as they were completing their pioneering Ph.D.s. I found in siliceous and carbonatitic magma systems enough experimental challenges to keep my laboratory busy through more than 40 years. In this way I was seduced from the freedom of field geology into the tyranny of the laboratory to conduct phase-equilibrium studies, the physical chemistry of mineral assemblages.

Mineralogists and geologists are full of ideas. They observe, describe, and interpret, but the history of earth sciences is replete with situations where a group of geologists could stand in front of the same outcrop, make the same observations, and come up with different interpretations. This was certainly true during the Granite Debate. I was a child of this Debate, which students don't hear much about these days, but in the 1950s it was very real. One was branded as "Magmatist" or "Transformist" according to one's beliefs. It was a perfect example of how strongly the interpretation of observations is influenced by the operating system and the mental software that process the signals entering the human brain. The debate fizzled out after the experimental demonstration by Tuttle and Bowen that under conditions where Transformists wanted to make granites in the solid state, many metamorphic rocks in the presence of aqueous pore fluid must be partially melted. The experiments were unambiguous.

Phase equilibria in mineral systems provide an experimental framework for discovering or testing the feasibility of processes. What appeals to me is that even as ideas and paradigms change, good experimental data last forever. I must say, however, that as I read about the new generations of experiments,

with marvelous instruments governed by computers, it is clear that old experimental data may be superseded by better-quality new data. Recently published SEM and TEM pictures are incredibly revealing compared with the crushed sample fragments that Art and I used to study with X-ray powder diffraction, immersion oils and petrographic microscope.

For the first half of the last century, the Geophysical Laboratory approach to understanding magmatic processes was to study progressively more complex synthetic mineral systems with spectacular success, as detailed in Young's fascinating MSA Monograph on N. L. Bowen. The Lab then introduced the study of whole rocks, with the granites of Tuttle and Bowen in 1958, and the basalts of Yoder and Tilley in 1962. These experiments inaugurated the systematic study of what became known as "dirty systems," which at first were considered to be somewhat suspect because of the many components and consequent lack of rigorous control in terms of the phase rule. By the end of the century the pendulum appears to have swung way over. Many petrologists now regard even complex model systems as too "clean" and inadequate, because they lack important components. Dean Presnall and I have emphasized the fact that both approaches are still needed—they complement each other. These approaches now have another companion. With improved thermodynamic databases (built from experiments) and computers, many phase diagrams can now be calculated, although many details of liquids with volatile components still need more experiments.

Whenever possible, we have tackled petrological problems by using both model synthetic mineral and whole-rock systems. The rocks have been provided by generous petrologists who may have spent years collecting and analyzing them. Tuttle taught me the effectiveness of deciphering what was happening in whole-rock experiments by analogy with the phase relationships in a model system, a system that could be analyzed in terms of the phase rule. Model synthetic systems illustrate possible paths of crystallization for a wide range of different starting compositions, each one representing a magma or rock. These paths elucidate likely, possible, or impossible processes. The model system framework facilitates interpretation of whole-rock experiments (single-bulk compositions), especially for parts of the rock system where phase percentages are low, or where liquids crystallize extensively during the quench. Charting a new path through a multicomponent system in search of a process, or testing the viability of a process, includes a sense of exploration that is just as exciting as trekking into an uncharted field area. The final test of an experimental process must be "Can it satisfy the field observations?"

The Wyllie-Tuttle experiments started off with crustal granites and carbonatites, leading to two petrogenetic papers at my first International Geological Congress concurrent with the General Meeting of the International Mineralogical Association in Copenhagen, 1960, from which I went to Helsinki for my first General Assembly of the International Union of Geodesy and Geophysics. Exploration of the effects of several volatile components additional to H<sub>2</sub>O on albite and granite melting was followed by albite-salt-H<sub>2</sub>O systems that illustrated particular crystallization paths and demonstrated the contrasting behaviors of fluorine and chlorine in silicate melts. A se-

ries of carbonate-silicate systems led us into processes of carbonatite origin, alkalic igneous rocks, liquid immiscibility, and refutation of the tenacious limestone assimilation hypothesis, more than half a century after Daly's 1910 proposal.

With the 1965 transfer of my laboratory to Chicago, individual granite experiments were extended into igneous rock series showing the phase fields intersected by a complex compositional line through the multicomponent calcalkaline rock system. We explored and classified H<sub>2</sub>O-deficient rock-H<sub>2</sub>O systems and vapor-absent melting (dehydration melting). We demonstrated how the H<sub>2</sub>O content of a magma changed the sequence of crystallization. By the way, does anyone know a better oxymoron than "fluid-absent melting"?

A piston-cylinder apparatus provided by Bob Newton and Julian Goldsmith and set up by Art Boettcher took us down subduction zones just as plate tectonics was breaking. Art and I co-authored 11 substantive experimental papers. Art demonstrated that the anticipated change in slope of the solidus in silicate-H<sub>2</sub>O systems at high pressures was caused by feldspar breakdown rather than by changes in molar volume of H<sub>2</sub>O. The only second critical end-point known in silicate systems was that for SiO<sub>2</sub>-H<sub>2</sub>O; Art located one on the solidus of CaO-SiO<sub>2</sub>-CO<sub>2</sub>-H<sub>2</sub>O at 32 kb, corresponding to upper mantle depths. Successive students and post-docs completed a series of silicate-H<sub>2</sub>O model systems to about 30 kb elucidating the role of hydrous minerals in parallel studies with granites, tonalites, basalts, and more mafic rocks. The phase relationships of calcalkaline rock series-H<sub>2</sub>O and pelagic red clay provided the basis for interpretations of and constraints on subduction zone processes and the origin of andesites, and on the prospect that incipient melting in the upper mantle might cause the seismic low velocity zone. The high-pressure melting relationships in CaO-SiO<sub>2</sub>-CO<sub>2</sub> provided a model for the behavior of calcite in subduction zones, and those in CaO-MgO-CO<sub>2</sub> provided the basis for understanding the effect of CO<sub>2</sub> on mantle melting. From the mid-1970s experimental and thermodynamic studies of parts of the model system CaO-MgO-SiO<sub>2</sub>-CO<sub>2</sub>-H<sub>2</sub>O convinced us that we understood the phase relationships in peridotite-CO<sub>2</sub>-H<sub>2</sub>O. We extended Bowen's petrogenetic grid for siliceous dolomites to melting reactions at mantle pressures, leading to processes associated with the origin of kimberlites and primary mantle-derived carbonatite magmas. In the 1980s we started experiments on interactions and hybridization between rocks and magmas, with applications to hydrous siliceous melts rising into peridotite from subducted oceanic crust.

Hybridization experiments were continued after the move to Caltech in 1983 with assimilation near the crust-mantle boundary, H<sub>2</sub>O from serpentinite diffusing into molten granite and tonalite, magma mixing of hydrous basalt and rhyolite, MORB with limestone, and amphibolite with hydrous granitic melt. Experiments on trondhjemites and tonalites were directed toward the origin of Archean crust, and complemented with dehydration-melting experiments on amphibolite, both powdered and solid, to explore the effects of time and mineral texture. A final round of experiments defined silicate-carbonate liquid immiscibility in progressively more complex model systems up to 2.5 GPa, and established the detailed phase rela-

tionships in the model system CaO-MgO-SiO<sub>2</sub>-CO<sub>2</sub> from 1 GPa through the critical pressure level at 2.8 GPa where CO<sub>2</sub> carbonates peridotite to form dolomite. These experiments provided many applications to the role of CO<sub>2</sub> in mantle melting and metasomatism, the probable occurrence of a critical end-point on the mantle-volatile solidus curve, the origin of primary dolomitic carbonatite magmas and their evolution to crustal dolomitic and calcitic cumulates, and some constraints on the origin of kimberlites.

I have always lived close to mineralogists. At Caltech, my immediate neighbor is George Rossman. I'm particularly pleased to note that George was awarded the inaugural (mid-career) Dana Medal this year, and would have been sharing the platform with me today except for the fact that MSA chose to bestow that Award at the Goldschmidt Conference. We are delighted with today's announcement that our young Caltech colleague, renaissance mineralogist John Eiler, will receive the MSA Award next year. In Chicago, Joe Smith was just down the corridor, and Paul Moore was just around the corner.

Romy shares with me not only our children, but also a mineral assigned to us by Paul Moore. Paul gave the name *Wyllieite* to one of his many discoveries, a rather grubby mineral (with a great structure) from pegmatites. He then generously named a related mineral *Rosemaryite*. As you know, new mineral names have to be approved by a Committee of IMA, which can be pretty bureaucratic at times (for good reasons). Paul persuaded the Committee that Romy's service to our community as Managing Editor of the *Journal of Geology* through several years was sufficient justification. She has since consolidated this claim by publishing her book: *Caltech's Architectural Heritage: from Spanish Tile to Modern Stone*, with a subtitle that demonstrates her qualifications in ceramics and minerals.

While on the subject of books, I'd like to comment on the satisfaction of turning class lectures and research into books that reach many others. My 1971 book *The Dynamic Earth* started off as a graduate course called "The Solid Earth," where I tried to merge what I learned in the laboratory with an interdisciplinary account of what I found in the literature to provide a global geophysical framework for location of the specialized research topics of our first-year graduate students. This just captured the beginning of the plate tectonics revolution. It seems to be best remembered for its dedication: "To wives the world over who put up with husbands who write textbooks when they should be in bed and dedicated, in particular, to Romy." For the large class of non-science majors in Chicago, I adopted a similar global approach instead of covering traditional "Physical Geology." By 1976, *The Way the Earth Works* was focused on a minimum-jargon presentation of plate tectonics and its historical development. I believe that these terminal courses that may be students' only formal exposure to Earth sciences should be structured quite differently from the foundation courses for majors in the field. My pleasure now that I am off payroll is to revise and expand this book with a subtitle: "a Century of Revolutions from Continental Drift to Earth System Science."

We are all bombarded with huge volumes of excellent new publications that we do not have time to read, and as I considered what work to do during my retirement I was impressed by

the enormous amount of excellent material in the older literature that new generations of mineralogists never see. Graduate students grow to be right up-to-date with the current state of science, without being encumbered by too much of the baggage of history that clogs some of our older minds. Concluding that most of our old papers were just gathering dust on library shelves, I decided not to add more dust by trying to polish off all those languishing first-draft research manuscripts that got away, but to concentrate on writing textbooks. It turned out that some of the old dust was actually being disturbed by a graduate student at McGill University. David Dolejs wrote to me that he had some difficulty trying to follow recently published melting relationships in natural rock starting materials, and then he discovered that in our older publications using model systems, he found both answers and principles that he was seeking. This was very gratifying, because my coauthors and I have usually had education in mind as we wrote research papers. It was even more gratifying to learn that Dolejs was sufficiently impressed with the old stuff to nominate me for the Roebling Medal. I have already thanked the Committee and the Council for their decision, but even more thanks are due to Dave Dolejs because the Committee evaluates only those who have been nominated.

We are mineralogists, but we study minerals for different reasons: because they are beautiful, because they make rocks, because they are economically useful or, as George Rossman explains: "because they are natural crucibles in which element organization and interactions can be studied." Geology grew out of mining and mineralogy. Rocks are incomprehensible without an understanding of minerals. Without minerals to capture elements for long-term residence, there would be no petrology, and only a fluid, ephemeral geochemistry. We appreciate the global nature of mineralogy, and its central role in Earth sciences, and we worry about the tendency in some quarters to treat classical mineralogy as somewhat old-fashioned. It is up to us to reinforce the theme that mineralogy, the materials science of the planets, is a vital subject embracing crystallography, petrology, and geochemistry, with strong overlap into geophysics. Applied mineralogy is also essential for the economies of nations. Similar processes are involved in mineral/fluid interactions during the formation of ore deposits, and in the disposal of waste fluids into rocks. I'm sure that we all worry about a new breed of graduates in environmental science or engineering tackling the latter societal problem with only a superficial understanding of minerals.

Professors must be the luckiest people in the world. We are paid to do what we love. We read, we learn, we teach, and we do research. We explore minerals and rocks, which are all around us wherever we go. We don't have to worry about layoffs (after the first seven years). We don't have to worry about stock market gyrations, confident that TIAA-CREF will take care of our shares and our retirement. Most of us do not get rich but we lead rich lives.

But we do have a responsibility to society. I have sometimes thought that instead of messing around in subduction zones and cratons, I should have been better employed doing something useful. We must find ways to bring the scientific knowledge of mineralogists and geologists to bear directly on our many hazardous and environmental problems. I became very conscious of this as chairman of the National Academy Committee that produced the 1993 disciplinary survey volume: *Solid-Earth Sciences and Society*, and again during my term as President of IUGG. It isn't easy, but Art Montana and Joe Smith have found ways to tackle these problems. When Art left academia and moved to New Mexico he was appointed by the Governor as Mining Commissioner, and he has since consulted on mine reclamation, dam removal, and nuclear-waste disposal in Japan. In his acceptance of the 1982 Roebling Medal, Joe wrote: "Just think how research and teaching in mineralogy and other sciences could be improved around the world with just one per cent of the money spent on war machines." Following his organization of an Academy Colloquium, he is now devoting a large fraction of his considerable energy to a project on "International Geoscience & Human Welfare" (Smith 1999, 2000).

Finally, I apologize again to my many co-investigators for having insufficient space to cite them here individually, I thank MSA again for this honor, and I hope that the young or aspiring professors among you find the same satisfaction in academic life that I have found. I wish you all good fortune as you continue to teach students about the beauty and utility of minerals, to educate your scientific colleagues with the fact that minerals are the foundation of the Earth, and to spread the word that mineralogy is the foundation of Earth sciences and much of our economy.

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