

also be used to determine the depth of origin of the lunar magma. Steve and his colleagues have shown that for both mineralogical and petrological applications, the "dirty" pyroxenes—those containing minor  $\text{TiO}_2$ ,  $\text{Al}_2\text{O}_3$ , and  $\text{Cr}_2\text{O}_3$ —are significantly different in melting temperatures and specific phase relations from the "pure" pyroxenes containing only Fe, Ca, Mg, Si, and O. An investigation by Steve, Bruce Lipin and Toby Wiggins of the partitioning of chromium between silicate crystals and melts under controlled oxidation conditions showed that this minor element sensitively reflects the conditions that exist where magmas are formed, and constitutes a petrogenetic indicator of great promise for lunar, terrestrial, and meteoritic rocks.

Clearly Steve is a catalyst and goad to his colleagues. He leads both by doing his individual research and by coordinating team studies. He is a perceptive critic and editor, and he is enormously gifted in things chemical, physical, mechanical, and electrical.

Steve and his colleagues at the Survey and elsewhere currently are tackling the especially difficult problems of diffusivity in pyroxenes, and along with Al Duba and others have devised procedures to insure that measurements of physical properties of minerals thought to form much of the lunar mantle are made on samples that have been very carefully described as to homogeneity and structure and do not

change significantly during the experimental measurements. Estimates of thermal distribution in planetary interiors derived from these measurements thus will be more reliable.

Huebner's contributions do not stop with his science. He has a strong sense of responsibility to all of the organizations with which he is associated. He has taken leadership roles as a project leader, as a principal investigator for lunar pyroxenes, as a member of NASA's Lunar and Planetary Review Panel, and as a member of at least three major Geological Survey committees. He has served his technical societies by assuming particularly time-consuming responsibilities: he was Treasurer of the Geochemical Society, member of the American Geological Institute Governing Board for 3 years, and Secretary/Treasurer of that organization for 2 especially demanding years. He has served the Geological Society of Washington in several capacities, including Secretary for 2 years.

Finally, let me add that Steve is a gentle person who has a fine family and a taste for fine wine and good sailing. He is an outstanding companion mapping in the field.

Mr. President, being personally cognizant of the many talents of J. Stephen Huebner and of his contributions to our science, I am especially honored to present him as the twenty-eighth recipient of the Mineralogical Society of America Award.

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## Acceptance of the Mineralogical Society of America Award for 1978

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*Dave Stewart, President Wyllie, Council, Society Members, and Guests:*

It is difficult to express my elation and gratitude at being chosen to join the distinguished rank of MSA awardees, which includes both Pete Wyllie and Dave Stewart. I say elation because it expresses my joy when I learned that I had been selected from among other deserving candidates. And I say gratitude be-

cause this award recognizes the scientific and professional communities with which I am associated.

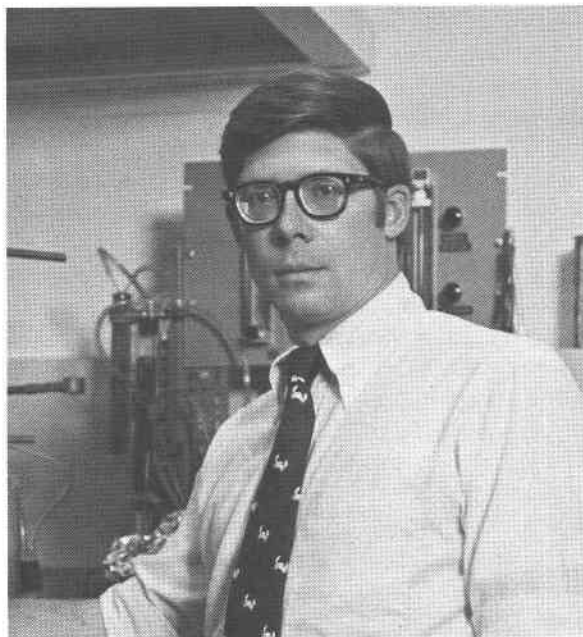
One might think, even demand, that the MSA awardee be creative or imaginative. I have had a year to think about today's remarks, yet upon reading the comments of previous awardees, I found I have little new advice or philosophy to offer. It follows that I agree with much of what has been said by my prede-

cessors. This being the case, I will spend only a few minutes telling you about things that are unique to me—how I came to stand here and how I hope this award is interpreted by my colleagues.

As a boy in Wisconsin, my father's father, Solomon, amassed a large collection of arrowheads from the furrows behind the family plow. Although later he became the foremost professor of insurance, and occupied that position for 40 or 50 years, he continued his fascination with the earth by collecting and acquiring minerals and agates, some from then schoolboy Edwin Roedder. As a child 5 years old I had already made up my mind in favor of the bright crystals of wulfenite, pyrite, calcite, and diopside, rather than the muted agates or the dark arrowheads. When I was six he presented me with a box of showy specimens and, totally encouraged by my parents, I launched myself on a career that brought me to this podium today. When I was 14 or 15, a fine naturalist and high school teacher, Robert C. Rugg, almost succeeded in permanently turning my attention: we designed an experimental program for studying the effects of colchicine on vegetables, and did worse things to upset the hormone balance of mice. This early contact with experiment, data, and the difficult report writing did as much as anything to help me survive the challenges to be offered a college undergraduate.

I enrolled at Princeton and expressed interest in a chemistry major. Then I met Dick Holland, who persuaded me that it was far better to study chemistry in the geology department than geology in the chemistry department. How true indeed. Dick offered contact with what I now recognize as the consortium approach to problem solving. This approach is excellent for students in the sense that they *must* interact, giving data to and receiving data from other members of the consortium. The Princeton group was studying the precipitation of calcium carbonate in caves and trying to unravel the geochemistry of calcite precipitated in the Providencia zinc and lead district, Mexico. Both investigations placed emphasis on the fluids. In the first study I investigated the water chemistry and the growth mechanism of stalactites; in the second study I was responsible primarily for fluid-inclusion thermometry and salinity estimates. I confess, however, that I have more vivid memories of peering down into the darkness of those Mexican mine stopes than of peering at vanishing vacuoles through the microscope.

I knew that I had been well prepared for Johns Hopkins when I survived next to a pair of ex-



ceptionally able and well trained Canadians, Mac Rutherford and George Skippen. Sometime during my first term, I spoke to Hans Eugster about a thesis in geochemistry. Hans suggested that I consider the experimental petrology of manganese, partly because it would build upon Bevan French's work on siderite and partly because, as I remember it, there was something about the availability of NSF funding. How lucky I was—to have a topic which captivated both the interest of my advisor and the support of NSF. Hans introduced the three of us (George, Mac, and myself) to bombs and buffers, then turned us loose to find out, largely by ourselves, how to make the lab really function. There were moments when we felt this was too much independence and responsibility, but in retrospect I appreciate Hans' wisdom. Soon after graduation, each of the three of us was to be responsible for a laboratory.

I am particularly grateful to Hans for emphasizing the importance of understanding how nature herself is an experimental petrologist. Bodies of manganeseiferous sediment were the experimental charges that were metamorphosed at different temperatures, pressures, and values of oxygen fugacity. It proved possible to guess which naturally-occurring mineral associations were stable assemblages and to erect a topology for the petrogenetic grid involving rhodochrosite, the manganese oxides, and the simple silicates. The later stages of the experimental work, dealing with the silicates, were strongly influenced by

this grid. After Hans' superb training, the transition to a career at the U. S. Geological Survey seemed natural.

I owe a tremendous debt to my colleagues at the U.S.G.S. They have given generously of their time, ideas, and philosophy, and tolerated my excursions into pseudoscience. Moto Sato taught me to appreciate high-temperature electrochemical methods and helped me calibrate oxygen buffers. Jim Papike, a previous awardee, and Mac Ross emphasized the importance of rock-forming minerals; we combined crystal chemistry with experimental petrology in our studies of amphiboles and pyroxenes. Several colleagues demanded quality—some would say perfection—but were willing to compromise on more obtainable goals. Dave Stewart, in addition, emphasized the importance of the scientific community and its societies. I could, and perhaps to be fair should, mention many more colleagues from this superb group, but time does not permit it. As a group, we have generally been given freedom to choose the subjects of our research, and when we have pursued "mission-oriented" research, we have been sufficiently creative to make it exciting. The support has been adequate, if not lavish. Most important, such support has been sufficiently stable to permit long-range investigations of such exceptionally complex subjects as the phase equilibria of pyroxenes, which are just now culminating. This year your award does not recognize an individual as much as a healthy scientific research team.

Pyroxenes have been at the center of my scientific life for several years now. From the first examination of the Apollo 11 samples, it was apparent that pyroxenes were moon-forming minerals. Not only did the lunar and planetary science community provide a stimulating and exciting environment in which to conduct basic research, but in addition NASA generously supported the effort. The managers of NASA's lunar sample programs realized that knowledge of pyroxene crystal chemistry and phase relations might help interpret lunar samples; my Survey colleagues realized that the same knowledge of pyroxenes might help the petrology of earth rocks. This research has largely been collaborative and has emphasized minute samples, phase characterization, and new experimental techniques. The investigations included the pyroxene subsolidus with Mac Ross; the melting relations of pyroxene with Al Turnock; and the electrical conductivity of pyroxene with Al Duba. My recent attempts to measure the diffusion rates of pyroxene components in orthopyroxene and augite have only verified that these rates must be very slow, but I am optimistic that joint efforts with new colleagues, using new methods, will make the problem more tractable. I hope I will be standing before some of you at a technical session next year, reporting successes in this area.

In closing, let me reiterate my thanks—with humility for myself, but pride for my associates. I hope that I will be able to live up to the great honor you have bestowed upon me.