

## AWARDS

### Presentation of the Roebling Medal of the Mineralogical Society of America for 1975 to Michael Fleischer

EARL INGERSON

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*Mr. President, Members of the Society, and Guests:*

The U. S. Geological Survey man whom you have chosen to honor by presenting him with the Roebling Medal, highest award of the Mineralogical Society of America, is not a mineralogist or even a geochemist, if you believe his civil service classification and title.

Mike Fleischer received his B.S. and Ph.D. degrees in chemistry from Yale, served three years as a physical chemist at the Geophysical Laboratory of the Carnegie Institution of Washington, and has risen from chemist P-3 to research chemist GS-16 during his years of service with the Survey.

In spite of this purely chemical background on paper, Mike is a past president of the M.S.A., belongs to most of the national mineralogical societies in the world, and has been elected an honorary fellow of the French and British societies and elected to the Deutsche Akademie Naturforscher Leopoldina. He is also a past president of the *Geological Society of Washington*.

What has this *chemist* done to deserve all of these offices and honors from *mineralogical* organizations? Plenty! While still an undergraduate at Yale he took W. E. Ford's course in mineralogy with some thirty geology majors. He emerged as top dog in the class with the coveted Penfield prize.

After receiving the Ph.D. he spent two years helping Professor Ford on a revision of Dana's *System of Mineralogy*, which gave him ample opportunity to see the mess that mineralogical nomenclature was in. He developed an intense interest in mineral chemistry and nomenclature and must have resolved at that time to do something about the latter, because less than five years later he was doing just that.

In February 1941 the "New Mineral Names" section of *The American Mineralogist* appeared with the initials "M.F." for the first time. Up until then the section was merely descriptive and largely uncritical abstracts. Fleischer began promptly to evaluate new descriptions and to suggest which ones were probably valid and which ones were not. Also, within two months of his take-over two new subsections ap-

peared, "New Data," and a much more significant one, "Discredited Minerals." A summary of the data appearing in the "New Mineral Names" section for 1941 through 1950 is instructive and impressive. Of 164 new mineral names proposed, Fleischer considered only 77 to be valid. Of the others, 22 were identical with known minerals, 33 were unnecessary varietal names, and 32 were without sufficient data to justify a new name. During this same period 85 older minerals were discredited, so for the minerals considered in the "New Mineral Names" section there was a net decrease of eight names—a very laudable accomplishment!

During 1951 to 1960, 419 new mineral names were proposed, of which Fleischer judged 234 to be valid, and 185 invalid. During this same period 139 more older names were discredited for a net gain of 95. For the twenty-year period 1941 to 1960, therefore, 583 new mineral names were considered, of which 311 were judged valid, 272 not, and 224 older names were discredited, for a net gain of 87. Unfortunately no summary for the years 1961 to 1974 is available, but undoubtedly not so high a proportion of invalid names got into print during this decade because of the increasing activity and influence of the Commission on New Minerals and Mineral Names of the International Mineralogical Association.

The abstracts and discussions in the "New Mineral Names" section and the summaries that have appeared from time to time have been most valuable to mineralogists and to the science of mineralogy. Fleischer's recommendations are not binding but few, indeed, are the mineralogists who would disregard them.

The work of the I.M.A. Commission is even more important in some respects than the "New Mineral Names" section of *The American Mineralogist*. The aim of the Commission is to act as a central group to coordinate the study of the problems of mineral nomenclature, to call attention to problems whose resolution would aid the science, and to initiate discussion leading to as much uniformity in nomenclature

as possible. This has included in practice the formulation of an outline of what should be given in the description of a new mineral and the review of proposed new names by the Commission in advance of publication where possible.

In 1959, the first president of the International Mineralogical Association, Professor R. L. Parker of Zurich, appointed Michael Fleischer to the chairmanship of this very important Commission on New Minerals and Mineral Names. Under Fleischer's very active guidance the Commission has attained its aim remarkably well. He has tried for the last four or five years to resign the chairmanship. He finally succeeded in 1974, when he became Chairman Emeritus, but I am sure that he will maintain his interest in and activity with the Commission.

Another little known but very important mineralogical project of Mike's is his revision of the very widely used but out-of-date *Microscopic Determination of the Nonopaque Minerals*, by Larsen and Berman. This has been accomplished during the last dozen years or so in his "spare" time and probably will appear within a year or two as a most useful compilation and another highly significant contribution to mineralogy.

Mike practices what he preaches. His work in mineralogical nomenclature has by no means been confined to theoretical considerations and recommendations as to what other people ought to do. By all odds his greatest practical accomplishment in this field has been resolution of the problem of the manganese oxide minerals, which was in chaos when the late Foster Hewett suggested in the late 1930's that Mike do something about it. He did.

He instituted a series of research papers with the general title *Studies of Manganese Oxide Minerals*. Nine of these have been published and Mike is the author or co-author on eight of them. Four summaries have also appeared and Mike is an author on all of them. The problem of the manganese oxide minerals may not be completely solved, but thanks primarily to Fleischer's efforts it is well in hand.

He and several co-authors at the Survey have published at least eight other definitive articles on problems of the identity and nomenclature of minerals and mineral groups other than the manganese oxides.

I was not privy to the deliberations of the Council leading to the selection of Dr. Fleischer as a recipient of the Roebling Medal. The chances are good, however, that the award is based largely on the contributions to the science of mineralogy just outlined.

However, Mike has performed and is performing a

service in a closely related field that, at least in terms of sheer bulk, dwarfs all of the items we have been discussing. In 1941 he became editor of Section 8 of *Chemical Abstracts*, entitled *Mineralogical Chemistry*.

At that time it was one of the weakest sections of *Chemical Abstracts*, consisting of hardly a page in each semi-monthly issue with perhaps ten to a dozen abstracts of papers on the chemistry of minerals. Under Fleischer's editorship the section expanded rapidly in scope as well as in size.

By the early 1950's a typical issue contained up to about ten pages and 100 abstracts, which covered such topics as hydrogeochemistry, isotope geology, ore deposits, petrography, volcanology, etc. It had surpassed most of the earth science abstracting services containing, for example, many more abstracts in the same fields than *Mineralogical Abstracts* and *Geological Abstracts* combined.

Most geochemists and mineralogists who knew about Section 8 and had access to *Chemical Abstracts* found it to be the most useful single abstract journal available to them. In 1953 the broad coverage was recognized and the section's name changed to *Mineralogical and Geological Chemistry*.

The section has had its number changed several times over the years—18, 25, and now 53—but it has maintained its excellence, broad coverage, prompt publication, and incomparable editor. With the tremendous growth in the number of papers appearing, it has been necessary to increase the number of abstracts in the section accordingly. From probably less than 1,000 abstracts per year in 1941, the number has increased to an estimated 9,500 per year for 1975. A rough summary suggests that something like 140,000 abstracts have been prepared under Fleischer's editorship. He didn't write them all, of course, but he reviewed, approved and arranged them for publication in *Chemical Abstracts*. This is an average of almost twenty abstracts per working day, year in and year out, for 34 years—a really astonishing accomplishment in view of all of the other things he was doing during those same working days. The section in *Chemical Abstracts* has maintained its edge in quality and in publication time, as well as quantitatively over the years. For example, the last issue of *Chemical Abstracts* contains 25 large pages in fine print with 343 abstracts in Section 53. *Mineralogical Abstracts* is a quarterly but if we calculate for the same period of time *Chemical Abstracts* has approximately five times the number of entries covering essentially the same subject matter. Moreover, at the end of the section

each time there are numerous cross-references to abstracts of pertinent articles in other sections of *Chemical Abstracts*, which are almost all missed by abstracting services in the earth sciences.

As in mineralogy, Fleischer's work in geochemistry is not just abstracting the literature. He has authored or co-authored some two dozen papers and reports on the geochemistry, occurrence, or distribution of at least twenty different individual trace elements and groups of trace elements, as well as having published numerous papers on other aspects of geochemistry.

Various organizations have recognized his interest and accomplishments in these fields of geochemistry. He served a term as chairman of the Commission on Geochemistry of the International Union of Pure and Applied Chemistry, has been vice-president of the Society for Environmental Geochemistry and Health,

and is a past president of the Geochemical Society.

The American Chemical Society should recognize geochemistry as an important part of its subject matter. It would be appropriate for them to establish an award for contributions to geochemistry analogous to the Roebling Medal. Mike Fleischer would be a prime candidate for its first recipient.

My wife has reminded me that Mike is not only a great scientist but also a very human individual. This I readily admit and join her in proclaiming that he is a great guy, a wonderful husband and father, and that Mike and Helen Fleischer taken together are as great a pair of friends as one could find anywhere!

Mr. President, it is extremely gratifying to me personally to have the privilege of presenting to you Dr. Michael Fleischer as a recipient of the Roebling Medal.

*American Mineralogist, Volume 61, pages 510-512, 1976*

## Acceptance of the Roebling Medal of the Mineralogical Society of America for 1975

MICHAEL FLEISCHER

*U.S. Geological Survey, Reston, Virginia 22092*

*President Muan, Professor Ingerson, ladies, and gentlemen:*

Nearly 40 years ago, in December 1935, I travelled from New Haven, Connecticut, where I was employed (at 50 cents an hour) to prepare summaries of the chemistry of minerals for the Seventh Edition of Dana's *System of Mineralogy*, all the way to New York City to attend my first Mineralogical Society of America meeting. I was thrilled to meet such legendary figures as Charles Palache and Waldemar Schaller and to discuss with them some discrepancies in published mineralogical data that had been puzzling me. Nothing could have been further from my mind than to imagine that I would ever stand where these first two recipients of the Roebling Medal did in 1937 and 1938. For this great honor, I am deeply grateful to the Mineralogical Society of America and to all those who have helped to make it possible.

When I look back, it is evident that I have had a most extraordinary run of good luck. Perhaps the most important event was that my paternal grandfather, living in a peaceful village in Hungary where the village and everything for miles around was owned by one nobleman, told my father, "Go to



America, where you won't have to bow down to anyone, and where your children can get more education than the 4 years you could get here." And so I was born in this country, with the chance to go to school as long as I wanted, and with parents willing to sacrifice to make it possible. Even so, the full meaning of this was not apparent until the early 1940's, when my many cousins in Hungary and Czechoslovakia were ruthlessly murdered, not for anything they had done, but solely because they were their parents' children. And to me, as to millions of others similarly affected by these tragic events, they have ever since been a reminder, "What have you done that warrants this good fortune?"

I was also very fortunate in having two older brothers who were brilliant scholars. I must admit that as I followed in their footsteps through high school and college and graduate school, there were many times when I was very sure that this was *not* a good thing, but hindsight makes it clear that trying to live up to their records was very important to someone like me, with a strong natural inclination to get by with minimum effort.

At Yale I had the good luck to be a student of H. W. Foote in chemistry and of W. E. Ford in mineralogy, and my debt to these two students of S. L. Penfield is very great, not only for instruction and inspiration but for many personal kindnesses during the grim years of the depression. For me, this broke in February 1936, when I received a fellowship sponsored by the Geological Society of America to work under G. W. Morey at the Geophysical Laboratory of the Carnegie Institution of Washington on systems with  $\text{CO}_2$  and  $\text{H}_2\text{O}$  at high pressures. No one could have been exposed to the atmosphere of that laboratory without being affected profoundly. The scientific staff was small, less than 25, but it included six future Roebling medalists (H. E. Merwin, F. E. Wright, N. L. Bowen, J. F. Schairer, George Tunell, and E. F. Osborn), three future Day medalists (Morey, Schairer, and Earl Ingerson), and two future Penrose medalists (A. L. Day and Bowen), as well as luminaries such as L. H. Adams, C. N. Fenner, R. E. Gibson, R. W. Goranson, J. W. Greig, F. C. Kracek, E. W. Posnjak, E. S. Shepherd, and that most remarkable analytical chemist, E. G. Zies.

I think that the experience of watching Earl Ingerson measure "just 50 more quartz grains" to make sure that a fabric diagram was correct, or seeing George Tunell spend months of tedious calculation, in those days before computers, to make sure that he had the right space group, began the slow process of

overcoming my tendency to be satisfied with "once over lightly," even though it was to take years of Waldemar Schaller's efforts to bring about something close to a cure.

In January 1939, I joined the U.S. Geological Survey, where I was to work closely with mineralogists W. T. Schaller, C. S. Ross (both Roebling medalists), Charles Milton, and later with many others including G. T. Faust, E. J. Dwornik, and Mary E. Mrose. In those first years, I learned from some of the world's best analytical chemists, including R. C. Wells, J. J. Fahey, and R. E. Stevens, as well as K. J. Murata, who was just then carefully and patiently converting optical spectrography from a qualitative tool to a method for the quantitative determination of trace elements in rocks.

Two years later, as the U.S. Geological Survey turned its efforts towards the search for strategic minerals, I became involved, along with W. E. Richmond of our X-ray laboratory, in a project on the mineralogy of the manganese oxides, under the guidance of D. F. Hewett, C. F. Park, Jr., and later T. A. Hendricks. Our primary goal was to be able to distinguish and identify these poorly known minerals, and to learn the limits of their compositional variation, but our further goal was the ambitious one of relating composition to paragenesis, in the hope that we might deduce conditions of formation from the chemical composition and the mineral associations, thus providing new aids to prospecting.

We did learn to distinguish most of the common manganese oxides, and we did learn much about their compositions, but we only provided a small beginning for future workers on our second main objective. As so often happens, one of the major contributions of this work was an unexpected byproduct—it revealed to the dry-cell industry the startling new idea that crystal structure and solid-state science could be as important to their work as the chemistry of solutions.

In 1944, this interesting work, and the delight of having a dozen geologists, especially S. K. Neuschel, shipping us beautiful minerals to study, had to be set aside regretfully because of our involvement with the Manhattan Project's search for new sources of uranium. Laboratory work was replaced for me by a combination of administrative duties plus library researches on geochemical abundances of elements, as the U.S. Geological Survey was called upon to find new sources of beryllium, boron, germanium, niobium, the lanthanides, selenium, tellurium, and other rare elements. During those years, I had the privilege

of working with four men who were not only capable scientists but were also outstanding in their leadership and their understanding of their colleagues: the late E. S. Larsen, 3rd, the late John C. Rabbitt, the late W. T. Pecora, and that unsung genius of analytical chemistry, F. S. Grimaldi.

Work on the rare elements eventually led to involvement in the complex problems of the effects of trace elements on human health and to several years of work with the National Research Council Committee on the Medical and Biological Effects of Environmental Pollution, probably the most exciting, educational, and aggravating work I have ever done. It brought home to me as never before the interdependency of the sciences and the value of a multidisciplinary attack on complex problems.

It also made me realize how much all of us depend on the work of a large group of librarians, editors, and abstractors, without whose efforts the fast-growing literature of our science would not be available to us. We in mineralogy have been fortunate in having available both *Mineralogical Abstracts* and *Chemical Abstracts* to cover the fields of interest to us, and fortunate that so many persons have contributed their efforts to this work. I wish I could list them here, but space permits mentioning only one, Marie Siegrist, who may be taken as "type specimen." It is my hope that all these persons will feel as I do, that in accepting this great honor from you today, I am acting as their representative to receive the thanks owed by the members of the Mineralogical Society of America to all of them for their services to the science.

*American Mineralogist*, Volume 61, pages 512-513, 1976

## Presentation of the Roebling Medal of the Mineralogical Society of America for 1975 to O. Frank Tuttle

ELBURT F. OSBORN

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Orville Frank Tuttle, better known to his older colleagues as "Tut," was the first recipient of the Mineralogical Society Award. Later, for continuing notable achievements in research, he received the Arthur L. Day Medal of the Geological Society of America. Our Society is now pleased to be able to recognize his outstanding contributions still further by bestowing on him its highest award.

Dr. Tuttle is being honored for his very great contributions to experimental petrology. Among several major attributes that have made his achievements possible are his ability to devise relatively simple, novel laboratory equipment and techniques that work, and an intuitive ability to select important and doable geological problems on which to work.

While a member of the staff at the Geophysical Laboratory after World War II, he developed, and of course used, hydrothermal research equipment that was in my opinion the single most important factor in the flowering of this type of research during the following two decades. Using this equipment in collaboration with N. L. Bowen, he first studied the system  $MgO-SiO_2-H_2O$ . The method of presentation of the

data and the application of results to important geological problems were strokes of genius. This investigation was a milestone in phase-equilibrium research—a classic study that opened up a whole new field, and that, incidentally, closed out the controversy of that time on the possibility of the existence of ultramafic magmas.

He and Bowen turned then from olivine and serpentinites to feldspars and granites, using continually improved hydrothermal equipment. Their paper in 1950 on the system  $KAlSi_3O_8-NaAlSi_3O_8-H_2O$ , providing us with a subsolidus phase diagram for the alkali feldspars, was followed in 1958 by their G.S.A. Memoir on the origin of granite. Their hydrothermal studies of phase relations of the alkali feldspars and their work on the origin of granite are masterpieces that will endure as significant, fundamental contributions to geological science. Few papers are as often referred to as the Tuttle and Bowen G.S.A. Memoir, "Origin of granite in the light of experimental studies in the system  $NaAlSi_3O_8-KAlSi_3O_8-SiO_2-H_2O$ ."

For 12 years beginning in 1953, Dr. Tuttle served as a most distinguished member of the faculty of the

Pennsylvania State University, inspiring students and faculty colleagues while demonstrating how to do highly creative research. In the experimental petrology laboratory that he developed, notable contributions were studies of carbonate systems, especially the systems  $\text{MgO-CO}_2\text{-H}_2\text{O}$  and  $\text{CaO-CO}_2\text{-H}_2\text{O}$  and their bearing on the origin of carbonatites.

In 1965 Dr. Tuttle joined the faculty at Stanford University, where he was instrumental in building

another high temperature–pressure research laboratory. The programs that he started at Penn State and at Stanford have continued as productive, high-caliber programs for graduate students and faculty in the geological sciences.

Mr. President, in view of his exceptional and highly important contributions to mineralogy and petrology, it is indeed an honor and great pleasure for me to present Frank Tuttle for award of the Roebling Medal.

*American Mineralogist, Volume 61, pages 513–514, 1976*

### Acceptance of the Roebling Medal of the Mineralogical Society of America for 1975

O. FRANK TUTTLE

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*Mr. President, Dr. Osborn, Fellows and Members of the Mineralogical Society, and Guests:*

I am honored to receive the Roebling Medal. It is particularly nice to receive such an honor when there is only a slight chance that I will recover enough to make significant contributions to experimental mineralogy again.

I became interested in mineralogy after two “false starts” in college. First I took two correspondence courses from Penn State, Engineering Drawing followed by English Composition. I do not recommend education by correspondence because of the lack of personal interaction between professor and student. Fortunately, a Penn State Center was set up in Bradford, about sixteen miles from my home, and I was able to take enough credits at night school there to complete my first year as a Mechanical Engineering major.

During my second year I became acquainted with several students who were enrolled in Petroleum Engineering. My home was in the Pennsylvania oil fields, and I had worked as a tool dresser to pay college expenses, so it seemed reasonable to change to Petroleum Engineering.

Then I took a course in Optical Mineralogy from Dr. A. P. Honess which inspired me to change majors again, this time to Geology. Every lecture that Dr. Honess gave was an inspiring lesson in mineralogy, petrology, or some related field. He particularly liked to select controversial subjects such as the Fenner–Bowen argument concerning the iron enrichment



in basalts, or the Bowen–Brögger disagreement about the Fen area in Norway. I took several courses from Dr. Honess, and each was challenging and exciting.

By now one might think I was confused, but in reality by now I was no longer confused. I have never



regretted the changes. Each area of study enriched my background, and exploring the mysteries of minerals was my consuming interest from 1939 until 1967.

Perhaps the committee was aware of the situation in experimental mineralogy in 1947 when N. L. Bowen and I started to work on the feldspars. We had no pressure vessels that were suitable for the pressure-temperature range we were interested in exploring. We had the Morey bomb, which had served G. W. Morey so well in his study of the system silica-potassium silicate-water. Most of the runs made by Morey were below 500° C and 1,000 bars. It was very likely that the lowest temperature and pressure we would be concerned with were well above the capability of the Morey bomb. We had access to a pressure vessel developed by Goranson to study the melting of granite. This apparatus was extremely complicated and would have required many hours, if not several days, to make a single run.

Fortunately, high hot-strength alloys were developed during World War II, and some of these alloys were also resistant to corrosion by water or steam. The "hydrothermal quenching apparatus" was developed using stellite-21, a high hot-strength alloy. It is characterized by a 60° cone seal in the hot area made by two rods with external thrust on the cone seal. The principal advantage of this apparatus is that there are

no threads in the hot area. The high temperature and pressure limit of this apparatus is 900–1,000° C at pressures of 750–1,000 bars. The lower stellite rod has a small axial hole that serves to connect the pressure chamber to a pump and gauge for pressure generation and measurement. This apparatus was used to investigate the feldspars + water and feldspars + quartz + water (granites) and was capable of making runs in twenty to thirty minutes.

One day, shortly after the "hydrothermal quenching apparatus" was put into service, the instrument maker who was working for me came to my laboratory and left a lower unfinished stellite rod with a drill broken off inside for me to heat and oxidize the drill. The unfinished rod did not have a cone seal on the upper end. In fact, at first sight the rod looked like a piece of stellite without the small axial hole which served to carry water under pressure up to the pressure vessel. It occurred to me that a pressure vessel much simpler than the hydrothermal quenching apparatus could be designed. It would consist of a rod of stellite with an axial hole drilled to within one-half of the diameter of the stellite rod. This, then, is the cold-seal pressure vessel which has been so widely used by universities and industries throughout the world.

Mr. President, I accept this Medal with many thanks to the Mineralogical Society of America.

*American Mineralogist, Volume 61, pages 514–515, 1976*

## Presentation of the Mineralogical Society of America Award for 1975 to Roger George Burns

W. S. FYFE

*University of Western Ontario  
London, Ontario*

*Mr. President, members of the Mineralogical Society of America, guests:*

It is with great pleasure that I introduce to you Roger Burns, Professor of Geology at Massachusetts Institute of Technology, as the 1975 recipient of the Mineralogical Society Award. As I do so, I just begin to wonder if New Zealanders are receiving some form

of favored treatment by this Society or perhaps there is something about that country that induces interest in rocks and minerals.

It was a very long time ago at Berkeley that we asked the National Science Foundation to support a project we had in mind concerning crystal field influences in geochemistry. At that time, a number of

chemists in the United Kingdom, particularly Leslie Orgel, had resurrected much earlier work of Bethe concerning crystal field theory, and Orgel had used these ideas to rationalize problems like the color, magnetism, and thermodynamics of solutions and solids with transition metal ions. It seemed obvious that the same concepts should be important in transition metal geochemistry. But at that time we had very little relevant data, and good measurements of absorption spectra were needed. So after some false starts "what's that got to do with geology" we were able to acquire the necessary equipment and get started.

At about that time, an old friend, Professor R. H. Clark of Victoria University, New Zealand, arrived at Berkeley on sabbatical leave. Bob was one of those people who had good hands, some knowledge of gadgets, and a good rugged sense of science. So some techniques were developed and the results looked interesting. Bob returned to New Zealand and suggested to a certain young man Burns, who was a chemistry major, that he might work with us at Berkeley.

So Roger arrived and, naturally, was first put into the senior field course, and survived. After this test of endurance, he worked very hard indeed and developed the necessary techniques to obtain the data that were needed, in particular polarized absorption spectra of minerals and glasses. His first major paper was published in the 1964 Vernadsky volume of the Academy of Sciences of the U.S.S.R., with Clark and myself.

As with any good student, by that time he had left his supervisor far behind. Roger presented a monumental thesis (Electronic spectra of silicate minerals: applications of crystal field theory to aspects of geochemistry) for his doctorate in 1965.

Roger left Berkeley for further studies in the De-

partment of Mineralogy and Petrology at Cambridge. In that environment of mineral expertise he was to blossom further, particularly because another interesting young man, G. M. Bancroft of Canada, was working on Mössbauer spectroscopy in the chemistry department. The two of them often put their heads together and each wrote books bringing together the state of the art (Bancroft, *Mössbauer Spectroscopy. An introduction for inorganic chemists and geochemists*. McGraw-Hill, 1973; Burns, *Mineralogical applications of crystal field theory*. Cambridge University Press, 1970). In particular, Burns made large contributions to mixing and site preference influences in solid-solution series. I think that these books must go down as landmarks in the development of a realistic approach to the geochemistry and mineralogy of the transition metals. Certainly in the future we will see developments associated with better and new techniques but Burns and Bancroft did much to introduce all of us to these important approaches.

Roger has continued his work since he joined the faculty of M.I.T. He has shown widely ranging interests from manganese nodules in the deep sea to behaviour of transition metals in the deep mantle of the earth. But his basic interest in the detailed environments of atoms in crystals has continued to be a major theme.

Since they first met in Berkeley, Roger has always had the support of a dedicated co-worker, his wife Virginia, who also has wide-ranging interests in mineralogy. For me, my association with Roger has been a total pleasure; he has contributed to my education in many ways and has always been ready to help and cooperate in a critical way. I know that he will continue to produce good data and stimulating new ideas.

Mr. President, I present Roger George Burns.



## Acceptance of the Mineralogical Society of America Award for 1975

ROGER G. BURNS

*Department of Earth and Planetary Sciences  
Massachusetts Institute of Technology  
Cambridge, Massachusetts 02139*

*Bill, President Muan, Honored Guests, Friends, Fellows, and Members of the Society:*

I am deeply honored to be the recipient of the MSA Award for 1975. However, I wish to acknowledge at the outset that this is a joint award, because a large measure of our achievements is attributable to my co-worker in mineralogy and in life, Virginia Mee Burns, who is at my side again today.

Almost a year ago, Gerry Gibbs and Peter Buseck came up to us while we were eating dinner in a Chinese restaurant at Miami Beach and told us that, pending Council approval the next day, I was to be nominated for the MSA Award for the coming year. Although I had secretly yearned for this distinction, I must admit that as we talked that evening to Gerry and Peter, both of whom are dynamic and productive leaders in mineralogical research, I realized just how lucky I was to be singled out for the Award. I am sure that each one of us in this room today can name several outstanding young mineralogists who have not been so recognized.

Consequently, in the interim year, I have reflected upon factors that might have led to this year's choice for the Award, and suggest to you that the Society is recognizing a special branch of mineralogy as much as a specific individual. If one surveys the list of distinguished mineralogists who have been past recipients of the MSA Award, and this in itself is a humbling experience, one notes that the majority have made valuable contributions in the more petrologic, crystallographic, thermodynamic, and phase equilibria branches of mineralogy. This year, I believe, the Society is recognizing the more quantum theoretical aspects of mineralogy, which have come into prominence through the emergence in recent years of spectroscopic and bonding studies of minerals. In this connection, the significant contributions made by Bill White, Gordon Faye, Mike Bancroft, Stefan Hafner, Dave Virgo, and Subrata Ghose should also be acknowledged, as well as the work of Soviet scientists S. V. Grum-Grzhimailo and A. S. Marfunin.



Other realms of reflection during the past year have been about the people and twists of fate that have brought me to the platform today. Fundamental to them all, of course, are the love, encouragement, and sacrifice of my parents, and in this regard I want to pay tribute to my mother who came all the way from New Zealand to be present at today's function. In my graduate student days in chemistry at Victoria University of Wellington, New Zealand, geology became a welcome release from the cold room set at  $-20^{\circ}\text{C}$ ,

in which I was studying the kinetics of a very fast organic reaction for a master's thesis in chemistry. When I graduated in 1960 with dual degrees in chemistry and geology, Professor R. H. Clark suggested combining the two fields and carrying out graduate studies in the then relatively new area of geochemistry with an up-coming young New Zealander that he knew at the University of California in Berkeley, named Bill Fyfe. It was there that I was exposed to the rigorous education and training in hard-rock geology that Berkeley is renowned for in the Turner-Verhoogen-Fyfe-Williams-Pabst era in the 1960's. First, there was the vitality and enthusiasm of Bill Fyfe, who as well as being a damned good scientist, was a human being and took a personal interest in the well-being of his students. He taught us to be critical of our own research as well as the work of established giants in the field. I owe you a debt of thanks, Bill. Then, there was the careful and fastidious training by Adolf Pabst in X-ray techniques, crystal chemistry, and structure determinations. Also at Berkeley, there were the authoritative and diversified educative skills of John Verhoogen and Frank Turner. It was during those long hours in the laboratory classes associated with Frank Turner's igneous and metamorphic petrology and U-stage courses that the idea came to me of where to stick the petrographic microscope plus universal stage—in the Cary spectrophotometer. This microscope technique opened up new horizons for measuring spectra of oriented small mineral crystals in petrographic thin sections, and has shed much light on the causes of color and pleochroism in minerals, as well as providing fundamental data for understanding the crystal chemistry and bonding of transition metals in minerals.

Following graduation from Berkeley in 1965, we spent a fabulous year or so in Cambridge, England, at the invitation of Stuart Agrell. It was there that Mike Bancroft and I discovered one another—he a Ph.D. student with a new technique, Mössbauer spectroscopy, in search of a novel problem, and me with suites of analyzed minerals in search of techniques for extending our knowledge of the crystal chemistry and bonding of minerals. Those were exciting days in Cambridge as we reconnoitered the relatively untouched field of the Mössbauer effect in iron-bearing minerals.

It was in the late 60's that I became associated with my first crop of outstanding graduate students, first in New Zealand and later at Oxford University. Looking back, I realize how lucky I was to have such people as Jean Prentice, Frank Huggins, Colin

Greaves, Miles Tew, Tony Law, Bruce Brown, and David Vaughan working with me. This trend has continued during the past six years at MIT, and I acknowledge the innovative work of Frank Huggins and Rateb Abu-Eid (whose findings were reported yesterday in the Symposium on Minerals and Rocks under High Pressure), as well as the thesis research of Hank Simon, Earl Whipple, Jim Besancon, Mike Thonis, and currently Bruce Loeffler, Chien-Min Sung, Sandy Yulke, Kay Parkin, Malcolm Siegel, and Ken Schwartz. During our gallivanting through academic institutions around the world, we have been associated with several stimulating colleagues. At Berkeley, there were Bernard Evans, Eric Essene, Dan Weill, Bill Crawford, John Starkey, and Roger Strens, with whom we also interacted at Cambridge. Also at Cambridge, there were Mike Bancroft, Mike Clark, Stuart Agrell, and Jim Long. And most recently at MIT, I have been fortunate to collaborate with two outstanding research associates, David Vaughan and Jack Tossell. Through combined efforts of all of these people as well as by interacting with other colleagues in the field, the principles and applications of mineral spectroscopic techniques, such as electronic absorption, vibrational and Raman, ESCA, electron spin resonance, and Mössbauer spectroscopy, together with modern models of the chemical bond, such as crystal field theory and molecular orbital theory, have become integral parts of modern mineralogy.

Exciting times lie ahead in mineralogical research, now that the theory and applications of mineral spectroscopy to transition metal geochemistry are established. We saw some examples in yesterday's Symposium. I, personally, am attracted by the opportunity to apply these principles to the benefits of mankind. Our current work on the manganese(IV) oxide phases, for example, appears to have important implications in battery fuel cells and hence energy, in deep-sea manganese nodules as viable resources for strategic metals, in soil research and agriculture, and in pollution control. Metallogensis patterns at convergent plate margins will, I believe, be elucidated by spectral measurements of silicate liquids and aqueous fluids above the critical point, employing the concepts of crystal field and molecular orbital theories. Our science is in need of the elegant measurements and calculations that Peter Bell, Dave Mao, Tom Shankland, George Rossman, Frank Huggins, Rateb Abu-Eid, Gerry Gibbs, Jack Tossell, David Vaughan, Chien-Min Sung, and Bruce Loeffler are currently performing.

Thank you very much for recognizing our region of the spectrum of mineralogical research, and for singling out my quantum of effort as being worthy of the MSA Award. In common with fellow kiwis before me (and I refer to Bill Fyfe, Doug Coombs, Brian Mason, Colin Hutton, and Frank Turner), I appreciate the international outlook and unusual generosity that the Mineralogical Society of America shows in recognizing the contributions of people

born overseas by election to high offices or by presentation of prestigious awards. Finally, I would like to pay tribute to a respected scientist and good friend of many of us, David Waldbaum, who passed away so tragically last year. I believe that had he been alive today, we would be witnessing a two-fold, double-header in which the platform was being shared by two MSA Awardees as well as two Roebling Medallists.