

PRESENTATION OF THE 1967 ROEBLING MEDAL OF  
THE MINERALOGICAL SOCIETY OF AMERICA TO  
LINUS PAULING

J. D. H. DONNAY, *Department of Chemistry, The Johns Hopkins  
University, Baltimore, Maryland.*

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

Linus Carl Pauling is an Oregonian. He was born in Portland on February 28, 1901. He earned his bachelors degree in 1922 at Oregon State College, married his June bride in 1923, and received his Ph.D. in 1925 at the California Institute of Technology. There he stayed on as a Fellow of the National Research Council. In Europe for postdoctoral study, he worked in Munich, Copenhagen, and Zurich under the sponsorship of the Guggenheim Foundation. Back home in 1927, he reached the rank of full professor four years later, in less than half the minimum time! 1931 was also the year in which he received his first token of professional recognition—the Langmuir prize of the American Chemical Society. Honors came to him, dribbling at first, then pouring. By now he has accumulated 2 unshared Nobel prizes, 17 medals (the last one, the Linus Pauling medal of the northwestern section of the American Chemical Society, named after him and of which he was the first recipient, was awarded to him less than a year ago), and 28 honorary degrees: the prestigious M.A. from Oxford and doctorates of all kinds, among which I am happy to note one from my Belgian alma mater (Liège, 1955). He has been elected to countless academies, both foreign and domestic. A zeolite has been named after him: paulingite (1960), which has the largest cubic cell on record ( $a = 35.10 \text{ \AA}$ ). In 1963, when he left the Gates and Crellin Laboratories, after an association of over 40 years, the American School of Crystal Chemistry, which originated there, had grown and multiplied in many lands and was actively raising a thriving fourth generation of students. After a few years spent at the Center for the Study of Democratic Institutions, in Santa Barbara, California, Dr. Pauling is now a Research Professor at the University of California at San Diego.

The short account of Dr. Pauling's career and honors to date will illuminate the remark I overheard a while ago, "This Mineralogical Society of America certainly is not taking any chances: it wants to be sure whom it gives a medal to!" Yes, indeed. Our Roebling medal is not intended to encourage promising young men (we have a special award for this purpose), it is meant to reward solid achievement! In this respect our 1967 candidate should give no cause for concern; he is the

safest bet ever, for the list of his achievements exceeds even that of his honors.

Scientists in many fields have reasons to be grateful to Professor Pauling. In fact his interests range so far afield that competently to analyze all his contributions would require another Pauling. Let me, at least, remind you of the fields of endeavor in which he himself admits taking an interest: crystal structures, molecular structures, line spectra, quantum chemistry, molecular rotation in crystals, ionic radii, theory of stability of complex crystals, proteins and helices, in short the vast subject of the chemical bond; turning toward biology and medicine: the relation between disease and molecular abnormality, immunochemistry, sickle-cell anemia; in other fields, structural problems of metals and alloys, ferromagnetism.

Professor Pauling has written eight books. Let me recall their titles: *The Structure of Line Spectra*, with S. Goudsmit (1930); *Introduction to Quantum Mechanics*, with E. B. Wilson (1935); *The Nature of the Chemical Bond* (1939, '40, '60); *General Chemistry* (1947, '53)—a book so revolutionary for its time that it could properly be dubbed the first novel chemistry textbook since the days of Lavoisier!—followed by a special presentation for younger students, *College Chemistry* (1950, '55, '64). The last book, also intended for the use of students, is a shortened version of the 1939 work: it is entitled *The Chemical Bond, A Brief Introduction to Modern Structural Chemistry* (1967). The next-to-last book signed jointly with the illustrator Roger Hayward, was *The Architecture of Molecules* (1965). The antepenultimate one was written in quite a different vein; it is entitled "*No More War!*" (1958, '62) and throws abundant light on another major interest in its author's life.

Whereas Dr. Pauling received his Nobel Prize (1954) in Chemistry for his investigation of the powers that bind substances together, he was awarded the Nobel Peace Prize (1962) for his fight against nuclear weapons. Having found out why atoms hold together, he showed us why men should stick together. His signature heads the list of some 13,000 signatures of scientists from all over the world, pleading for an end to nuclear testing. The conclusion of the Test-Ban treaty between USSR and USA is due, in no small measure, to his leadership. Professor Pauling is a great peace maker, worthy of the noble title that the World Fellowship of Religions bestowed upon him in 1966, "the Supreme Peace Sponsor."

Some people have asked me, "Why give Pauling one more medal? Will not our modest homage look like an anticlimax?" There are at least two reasons why we should be allowed to offer it to him. The first one: while it is true that he was elected "Correspondant pour la Minéralogie" by

the Academy of Sciences of Paris as early as 1948, nevertheless there is not a single mineralogical medal in his present-day medalary. He has been recognized by universities, colleges, societies, academies, institutes, foundations, and governments; he has been recognized by chemists, biochemists, physicists, physicians, artists, jurists, clerics, and educators. Our own profession, which owes him so much, cannot tarry, cannot be ungrateful any longer: it is high time we jumped on the band wagon!

In 30 years Professor Pauling has published many more than 30 mineral structures. Had he done nothing else in his life, he would go down in history as a great mineralogist. He published the structure of molybdenite (with Dickinson, 1923) when he was a first-year graduate student. You all know how, with his co-workers, he went through mineral sulfides, sulfosalts, halides, tellurides, oxides, and silicates, among which I would especially like to recall the micas and other platy species, along with such other beauties as zunyite, sodalite-helvite, the scapolites and the zeolites. In an era when determining a crystal structure consisted in two steps: (1) guess it, (2) prove it, Dr. Pauling not only gave us all these mineral structures, he also taught us how to guess a structure and provided us with the Pauling Rules to check it or disprove it. (At this juncture, for the sake of completeness, I should perhaps mention the Pauling Point, which some facetious critic has defined as the point beyond which a simple theory evaporates! Needless to say, Professor Pauling himself, who has cooked up so many simple theories for us, has always managed to keep them well below the Pauling point!) Throughout his versatile career he never lost interest, either in our science or in our society, in which he holds life membership. He even gave his only daughter in marriage to one of us. (No, I will not mention any name: I just want to take credit for the profession!)

The second reason why we can presume to add our medal to all the previous ones comes to me as a childhood memory. When a French general has received all the highest decorations, including the Legion of Honor, there is one more medal he can get: that is the *Médaille militaire*, normally awarded to privates and non-commissioned officers. I remember when Marshal Foch received this medal, at the end of the first world war, and how it was pinned on his tunic by one of his soldiers. *Mutatis mutandis* I would like to think that our medal will be Dr. Pauling's *Médaille militaire*. We want to present it to him, with respect and affection, mainly but not only for the tremendous contributions he has made to the science of mineralogy: coming as it does so late in his career, this medal should be considered a tribute to his whole life, to his human qualities, to his integrity as a scientist and as a citizen, to the simple courage with which he speaks his mind, to the higher patriotism (to use

Senator Fulbright's phrase) that guides his actions, and to the inspiration he has given us by showing us the way.

Mr. President, it is my honor to present to you the choice of our society as 1967 Roebling medalist, Professor Linus Pauling.

THE AMERICAN MINERALOGIST, VOL. 53, MARCH-APRIL, 1968

ACCEPTANCE OF THE ROEBLING MEDAL OF THE  
MINERALOGICAL SOCIETY OF AMERICA

LINUS PAULING, *Revelle College, University of California,  
San Diego.*

*Mr. President, Professor Donnay, Ladies and Gentlemen:*

I appreciate greatly the award to me of the Roebling Medal, and I thank the Mineralogical Society of America for it.

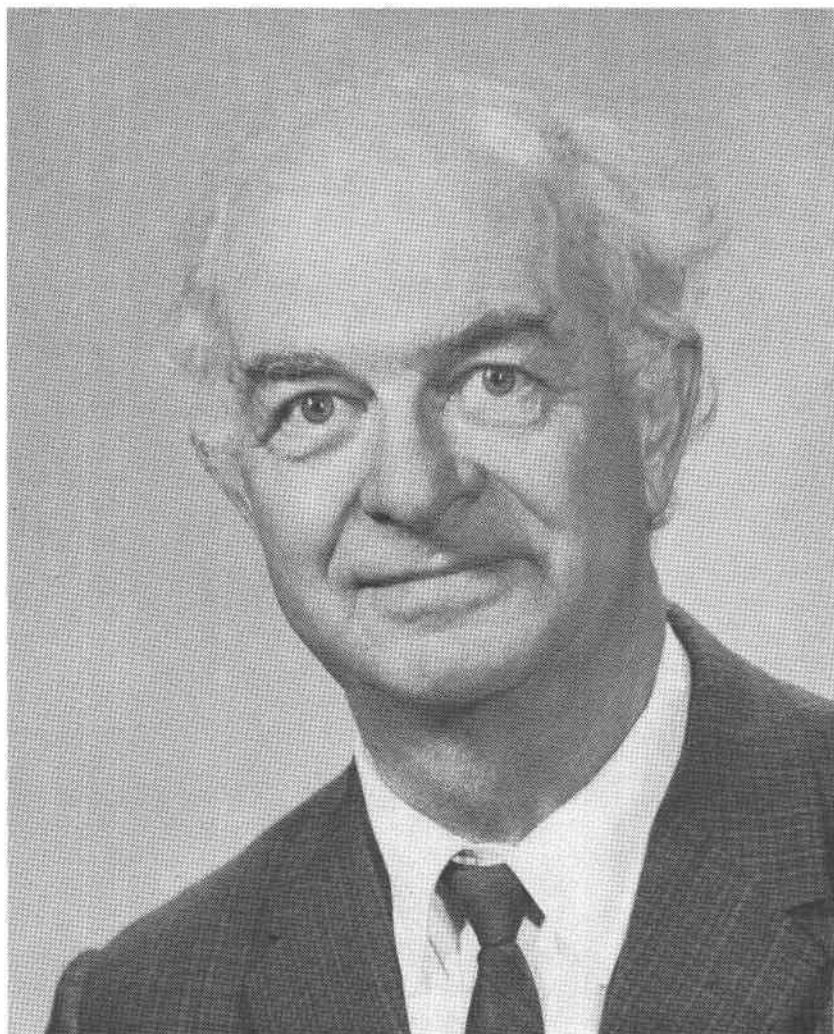
I am especially pleased to receive this award because of my love for minerals. I shall quote the first paragraph of a lecture on molecular disease that I gave at the annual meeting of the American Orthopsychiatric Society in 1949: "I like people. I like animals, too—whales and quails, dinosaurs and dodos; I am sorry that I shall never see a living dinosaur or dodo. I like trees; I hope that the giant redwoods will not all be cut down. I like microorganisms. I like crystals—minerals, such as calcite, rhodochrosite, pyrite, zunyite, lazulite. I do not like to think about the tons of beautiful crystals of galena, azurite, and malachite being mined and smelted to make the lead and copper used in unnecessarily great amounts in our expanding technological civilization."

I remember that when my wife and I visited Dr. Albert Schweitzer in Lambarene a number of years ago I suggested to him that his principle of reverence for life ought to be extended to include minerals, and perhaps could be named the principle of reverence for the world. Dr. Schweitzer said that he agreed with me.

My interest in minerals began fifty-four years ago, when for about a year I made an effort to collect minerals, after having devoted myself to insects the preceding year. I was not very successful, because I did not know how to find the interesting minerals in the Willamette Valley, near Portland, Oregon. I was not a keen enough observer to find such a beautiful and interesting new mineral as cavansite, which Dr. L. W. Staples told us about yesterday. I was limited in my means of transportation to a bicycle and the steam train that ran from Portland to Oswego, seven miles away, where my grandparents lived. At the end of the year

I was introduced to the wonders of chemistry by a friend of mine, my own age (thirteen). For two years, beginning when I was sixteen, I was a student in the School of Mines in the Oregon Agricultural College, Corvallis. I remember a course of lectures by Dr. E. K. Soper, Dean of the Schools of Mines, on mining geology, and a course of laboratory work on blowpipe analysis and fire assay.

In 1922 I became a graduate student in chemistry in the California Institute of Technology. My first published scientific paper was a report of the determination of the structure of molybdenite, which I carried out with Roscoe Gilkey Dickinson. The first determination of the structure of a mineral in the United States had been done in Pasadena, five years earlier. C. Lalor Burdick had been a student at the Massachusetts Institute of Technology and had gone to Europe to obtain his doctorate. The increasing probability that the United States would enter the war caused him to leave the continent for a stay in England, where he had some experience in operating a Bragg X-ray spectrometer. On his return to M.I.T. he built a Bragg spectrometer, and then, when, after a few months, he moved to Pasadena, at the invitation of A. A. Noyes, Chairman of the Division of Chemistry and Chemical Engineering in the California Institute of Technology, he built another X-ray spectrometer and, with the cooperation of Dr. James H. Ellis, carried out a structure determination of chalcopyrite. (Fifteen years later I suggested to one of my graduate students, L. O. Brockway, that he carry out a reinvestigation of chalcopyrite in order to determine the parameter with greater accuracy. He found that Burdick and Ellis had made an error, and had reported a wrong distribution of copper and iron atoms over the zinc positions of sphalerite. The correct structure was reported by Brockway and me in 1933.) The papers on chalcopyrite by Burdick and Ellis were published in 1917. Together with the X-ray powder diffraction studies of metals by Hull, they constitute the first X-ray diffraction work done in the United States. When I became a graduate student Dickinson and his associates had determined the structures of a number of crystals, including several containing coordination complexes and the first organic substance (hexamethylenetetramine) to have its precise molecular structure determined. I began searching for crystals that would be susceptible to X-ray examination by the methods then being used. Dickinson had been successful in determining the structures of cubic crystals of moderate complexity, such that some of the atoms, lying on symmetry axes, could be located in the unit cell by the determination of one or two parameters from the intensities of the X-ray reflections. It was considered a great feat when Dickinson succeeded in determining the five parameters locating the atoms in tin tetraiodide. At that time the effort was made to



*Linus Pauling*

use rather large crystals, between five millimeters and ten millimeters in diameter. Successive orders of X-ray diffraction were observed from the developed faces, and other faces were ground on the crystal for this purpose. Also, thin sections, from 0.1 to 0.5 millimeters thick, were prepared by grinding, in order that Laue photographs might be made. The principal technique in Pasadena at that time was the analysis of Laue photographs, with use of the methods that had been developed by Nishikawa and Wyckoff.

I made a search of the literature for suitable cubic crystals and synthesized about fifteen substances for X-ray examination. My first X-ray photograph, made on 28 October 1922, was a rotation photograph of a rather large crystal of potassium nickel sulfate,  $K_2Ni_2(SO_4)_3$ , which I had synthesized by melting potassium sulfate and anhydrous (dehydrated) nickel sulfate in an electric furnace that I had built myself. By slowly cooling the melt I had obtained crystals about 5 millimeters in diameter. The size of the unit cell and the space-group symmetry showed that the determination of the structure of the crystal would require the evaluation of nineteen parameters, and I went on to another substance. (Potassium nickel sulfate is now known to have the langbeinite structure, determined a few years ago.) I then grew large crystals of an intermetallic compound, sodium dicadmide, by dissolving some cadmium in molten sodium and allowing the melt to cool slowly, over a period of about a day. When the excess sodium was dissolved away with absolute ethanol, octahedral crystals of the cubic intermetallic compound about 5 millimeters on edge were obtained. Sodium dicadmide was an even bigger failure, its cubic unit of structure having an edge over 30 Å, and containing more than one thousand atoms. I worked on this intermetallic compound from time to time during a period of thirty-five years, until the structure was determined about four years ago by Dr. Sten Samson, in Pasadena.

After two months work and unsuccessful efforts with fifteen crystals I was rescued by Dickinson, who obtained a specimen of molybdenite from the chemistry stockroom. He cleaved a pebble, about 1 centimeter in diameter, glued the cleavage to a glass slide, and cleaved again to obtain a thin plate, essentially undistorted. With this plate he and I prepared rotation photographs and Laue photographs, and determined the structure of the mineral. The molybdenum atoms were found to lie in fixed positions and the sulfur atoms along the threefold axes, at positions determined by a single parameter. The structure of molybdenite was a pleasing one, in that molybdenite turned out to be the first substance found to have trigonal prismatic coordination of non-metal atoms around a metal atom.

During the next thirteen years I investigated sixty-three minerals by X-ray diffraction, and published structure determinations for about half this number. One of the early studies was a careful determination of the two-parameter structure of hematite and corundum, which I carried out in collaboration with another graduate student, Sterling B. Hendricks. From 1928 on many of my studies of minerals were carried out with use of specimens from the Oppenheimer collection. I had met Robert Oppenheimer in Germany in 1927, and discussed my work on the structure of minerals, as well as other scientific problems, with him after he came to California. He presented his fine collection of specimens of about one thousand minerals to me. Having these mineral specimens at hand probably contributed to stimulate me to investigate them, rather than synthetic chemical substances.

In 1922 I had become interested in the problem of the general principles determining the structures of inorganic crystals, and had made a survey of the available information about interatomic distances. The large body of additional information provided by many X-ray investigators, especially W. L. Bragg and his coworkers, as well as by our own studies could, I decided, be interpreted by the application to the problem of the theory of quantum mechanics that was developed in 1925. The success of this effort for the silicates and oxide minerals suggested that a similar attack should be made on the sulfide minerals. On 2 February 1934 I wrote a letter to Professors A. A. Noyes and J. P. Buwalda (Buwalda was the Chairman of the Division of Geology in the California Institute of Technology), asking them to communicate to the Geological Society of America an application for a grant from the Penrose Fund of \$4800 over a three-year period (\$1200 for apparatus plus \$1200 per year for salary of a post-doctoral fellow). Three of the paragraphs of my letter are the following:

"In the years 1927 to 1932 I was very much interested in the structure of the crystals of the silicates and related minerals, and devoted much effort to the study of this subject. This work was brought at that time to some degree to its conclusion by the formulation of a set of structural principles for complex ionic crystals. Since then I have been attacking the problem of the sulfide minerals, with the hope of ultimately reaching a similar understanding of the general principles determining their structure, composition, and properties. This program has progressed slowly; my students and I have determined the structure of chalcopyrite, sulvanite, binnite, enargite, tetradymite, and stannite.

"However, the investigation of more complex crystals and of minerals that do not crystallize well requires some apparatus that we do not possess, in addition to that at hand, and involves extensive and laborious graphical and numerical calculations. May I request that you communicate to the Geological Society of America an application for a grant of money to permit the intensive prosecution of this investigation during the next three years?

"With this aid I would plan to carry out during the year ending June 30, 1935, the



complete structure determination of pyragyrite, proustite, pentlandite, covellite, chalcocite, and the minerals of the niccolite group (niccolite, pyrrhotite, breithauptite, etc.). Preliminary X-ray studies have been made on all of these minerals, which indicates that their careful investigation should provide especially valuable information regarding the general nature of the sulfide structures; I believe that the completion of this program would do much to bring the same clarity into the field of sulfide minerals as has been reached for the silicates."

On 11 April 1934 I received from Waldemar Lindgren the following letter:

"Dear Mr. Pauling: Referring to your application, No. 193, for research on the crystal structures of sulfide minerals for an allotment of \$4800 over two years from the Penrose Fund of the G.S.A., the Projects Committee regrets that it cannot approve this application because preliminary to the beginning of the work there must be a large amount of money expended on apparatus. It would seem to be up to the Pasadena Institute of Technology to provide such apparatus.

Very sincerely yours, Waldemar Lindgren, Chairman, Projects Committee, G.S.A."

I wrote to Professor Lindgren on 17 April 1934, saying that the California Institute of Technology had spent \$8000 already to provide us with oscillating crystal apparatus, Laue apparatus, and other pieces of apparatus, and that the \$1200 requested for apparatus was to permit construction of a Weissenberg X-ray goniometer and a powder apparatus. I asked, however, that the application be changed by deleting the sum of \$1200 for apparatus, retaining only the sum of \$3600 for the salary of a postdoctoral research assistant for three years, at \$1200 per year. (I may mention that the salary \$1200 per year for a full-time postdoctoral research assistant shows the effect of the great depression of the 1930's; before 1930 a salary of \$1800 per year was considered proper. Also, Professor Lindgren's mention of two years was an error on his part.) I then wrote again on 12 July 1934, repeating my request for consideration of this grant of \$1200 a year for three years for the salary of a post-doctoral assistant. I did not receive an answer to either of these letters.

At about this time, however, I had become interested in hemoglobin, and had worked out a theory of the interaction of hemoglobin and oxygen. The Rockefeller Foundation came to our aid with a grant. Charles D. Coryell and I determined the magnetic susceptibility of hemoglobin, and obtained the interesting result that venous blood is paramagnetic, whereas arterial blood is diamagnetic. These magnetic studies led to the conclusion that the iron atoms in oxyhemoglobin are attached to six surrounding atoms in the same way as the iron atoms in pyrite are attached to the six neighboring sulfur atoms.

The foregoing episode suggests that granting agencies can influence the progress of science. I must say that I do not know to what extent the interest of the Rockefeller Foundation in organic chemistry, biochemistry,

and biology and the lack of interest of both the Rockefeller Foundation and the Geological Society of America in the structure of the sulfide minerals influenced my career. It is a fact that, although we continued to do some work on the sulfide minerals during the five following years (the structures of proustite, pyrrargyrite, and tetradymite were determined by David Harker, and some other sulfide structures were also determined), I have never got around to making a really thorough attack on the problem of their structure. My last paper in this field, published in 1965, was a theoretical discussion of the problem posed by sulvanite, whose structure we had determined over thirty years earlier.

In 1930 and the immediately following years I made an effort to have a staff member interested in X-ray mineralogy given a joint appointment in chemistry and geology. I suggested that this appointment be offered to an outstandingly able young man who had been a student of V. M. Goldschmidt. The Division of Chemistry and Chemical Engineering was willing that such an appointment be made, but the geologists were not interested.

In 1934 Professor Alexander N. Winchell came to Pasadena, on sabbatical leave from Minnesota. Together with Dr. H. P. Klug and me he carried out a determination of the crystal structure of swedenborgite. An account of this work was published in *The American Mineralogist* for 1935; for some reason this is the only one of my structure determinations of minerals that was published in this journal.

I have had a couple of graduate students in geology working with me. The first one was Maple Delos Shappell. He and I determined the structure of bixbyite in the early 1930s. I was astonished to discover, in the course of this investigation, that the positions of the metal atoms in bixbyite are such that there are two physically quite distinct structures possible with exactly the same set of interatomic distances. Just as I was wondering about this, I noticed an abstract of a paper that was scheduled to be given by Lindo Patterson, in which he said that he had proved that there could not be any pairs of physically distinct (and not enantiomorphous) structures with the same set of interatomic distances. After I had written to Patterson about the bixbyite case he changed his paper to a discussion of the conditions under which such pairs of structures might exist.

I continue to find pleasure in looking at minerals, and thinking about their structures. Again I thank the Mineralogical Society of America for conferring the Roebling Medal upon me.

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PRESENTATION OF THE MINERALOGICAL SOCIETY  
OF AMERICA AWARD FOR 1967 TO  
ALFRED E. RINGWOOD

BRIAN MASON, *U. S. National Museum, Washington, D. C.*

*Mr. President, Fellow Members, and Guests:*

A year ago in San Francisco, I, as your then President, was privileged to ask Dr. Pecora to introduce the recipient of the Mineralogical Society of America Award. Today I find myself elevated to his position. Appropriately enough, the request to perform the pleasant duty of introducing the first Australian to receive this award reached me in July in Central Australia, where I was engaged on a collecting expedition for the U. S. National Museum.

Alfred Edward Ringwood (better known to many of you as Ted) was born and educated in the state of Victoria. Even before he received his Ph.D. from the University of Melbourne in 1956, he had published two significant papers on the principles governing trace element distribution during magmatic crystallization. In these papers he discussed the influence of electronegativity and complex formation in modifying the classical Goldschmidt rules. Since then a steady flow of papers, incorporating experimental results, mineralogical and petrological observations, new ideas and hypotheses (some of them outrageous, according to reviewers) has appeared under his name, until at the present time his bibliography comprises almost one hundred publications. Curiously enough, only one of these has appeared in our own journal, and he is not even a member of our society. We are about to rectify the situation by means of this award. Now we are getting him into the society, I suggest that our Fellowship Committee observe his progress and consider elevating him from member to fellow.

After graduation from the University of Melbourne, he did post-graduate research at Harvard in 1957-58, returning to Australia in 1959 as Senior Research Fellow in the Department of Geophysics at the Australian National University in Canberra. He has been associated with that remarkable institution since then, having recently been advanced to the position of Professor of Geochemistry. I say remarkable institution advisedly—a post World War II creation, its meteoric rise to a high place among the world's great universities is evidently in large part a result of the remarkable quality of its faculty, and the facilities and freedom provided for research. I can testify as to the freedom—when I am

visiting Canberra he is always somewhere else! However, he claims the same is true for me when he visits this country, although we have managed to coordinate our movements for this event.

Since few of you are likely to go out and browse through his many publications to see why he is receiving this award, I should briefly outline his major fields of research. Most of you, I am sure, associate him with high-pressure phase transformations and their bearing on the constitution of the mantle, a theme on which he expounded brilliantly in this morning's geochemistry session. In 1930 V. M. Goldschmidt observed that magnesium orthogermanate,  $Mg_2GeO_4$ , had two polymorphs—one with the structure of olivine and one with that of spinel. In 1936 J. D. Bernal suggested that olivine itself might show a similar polymorphism, inverting to the denser spinel structure at some depth within the earth's mantle. This was merely an intriguing speculation until Dr. Ringwood took it up. In 1956 he calculated that  $Mg_2SiO_4$  should transform into a spinel structure at pressures of about 175 kilobars at about 1500°C. These conditions were unattainable in the laboratory at that time, so he turned his attention to similar compounds that might transform at lower pressures and temperatures. In 1958 he produced this transformation in fayalite,  $Fe_2SiO_4$ , at 600°C and 38 kilobars. Since that time, by careful selection of isomorphous compounds, he has pushed his experimental results closer to the  $Mg_2SiO_4$  composition, until in 1966 he was able to announce the synthesis of a spinel-like  $Mg_2SiO_4$  at 970°C and 170 kilobars, a brilliant confirmation of his 1956 prediction.

This, however, is only one facet of his many contributions to our science. He has elucidated many other high-pressure phase transformations, and by coordinating these results with the geological and geophysical data has provided a comprehensive account of the composition and evolution of the mantle, and of the genesis of basaltic magmas. He has written extensively on the chemical evolution of the earth, of meteorites, and of planets. In addition, he holds a patent on the production of rutile from ilmenite and related ores!

Equally at home in the deep earth and in deep space, he may well be characterized by a line from Gilbert and Sullivan "What a very singularly deep young man this deep young man must be."

Mr. President, it is my great privilege to present Dr. A. E. Ringwood, recipient of the Mineralogical Society of America Award for 1967.

THE AMERICAN MINERALOGIST, VOL. 53, MARCH-APRIL, 1968

ACCEPTANCE OF THE MINERALOGICAL SOCIETY OF  
AMERICA AWARD FOR 1967

ALFRED E. RINGWOOD, *Australian National University, Canberra.*

*Mr. President, Dr. Mason, Ladies and Gentlemen:*

It was a particular pleasure to have been introduced by Dr. Mason, a friend of long standing. I'm very glad, indeed, that at least on this occasion, our orbits have crossed.

I can only say that I am delighted to have been chosen as the recipient of the MSA Award for 1967. This is an award which is greatly valued by the younger generation of petrologists and mineralogists, and in the light of the achievements of many of my contemporaries in these fields, I count myself very lucky indeed. Although the Mineralogical Society of America is a national organization, it has a truly international outlook and has always been quite exceptionally generous in its recognition of non-Americans. I would like to pay tribute to this.

Since most of the work for which the present Award was made was carried out at the Australian National University, it might be of some interest if I describe our department there and also mention something of our approach to experimental petrology.

The Department of Geophysics and Geochemistry in Canberra was founded about 15 years ago by a remarkable man, J. C. Jaeger, under a general directive to conduct research in fields of earth science not at that time covered adequately in other Australian universities. Jaeger, incidentally, was an applied mathematician and engineer, without an earth science background. Nevertheless, he soon demonstrated a rare ability to recognize rewarding fields in the early stages of development and to attract people to work in these fields. Under his direction, several small groups working in geochronology and isotope geochemistry, trace element geochemistry and cosmochemistry, experimental petrology, petrology, seismology, paleomagnetism, heat flow, rock deformation, and rock mechanics were established, and currently produce about 90 papers a year describing their researches.

An interesting feature of research at the ANU has been the fruitful collaboration between groups—thus, the joint project between the paleomagnetic group and the geochronologists was a pioneering venture in this field, whilst the collaboration between the heat flow group and those interested in the geochemistry of uranium, potassium, and thorium was also very rewarding. My presence here today would not have occurred but for the favorable research atmosphere and liberal support which I



*A. Edward Ringwood*

have been fortunate enough to experience, and I would like to acknowledge my great indebtedness to J. C. Jaeger and to my colleagues at the ANU, particularly D. H. Green, on this occasion.

The high pressure experimental petrology group consists of two staff members (Green and myself), two technicians, and an average of two Ph.D. students. We usually have several projects proceeding simultaneously—some cooperative, others individual. Currently, our principal interests are in phase transformations at ultra-high pressures (100–250 kb) and in experimental petrogenesis. Removed as we are by some considerable distance from the mainstream of research in experimental petrology, we have developed a philosophy which I find differs slightly from that in some U.S. laboratories. One aspect of this concerns the investigation of relatively simple two-, three-, and four-component systems as contrasted with the investigation of natural rock systems. It seems to us that these investigations should be complementary and not exclusive. Yet there is a contrary view which maintains that because of the intrinsic complexity of systems with more than five components at atmospheric pressure, and the compounding of this complexity at higher pressures, the direct investigation of natural rock systems is unlikely to be rewarding.

I believe that this view neglects the order which is observed in descriptive petrology. Although, for example, the compositions of basalts vary over a fairly wide range, petrologists have long recognized certain well-defined sub-classes of basaltic magmas, and deduced that these magma types are related to one another by specific crystal fractionation processes or fractional melting behavior. These fractionation mechanisms must possess a high degree of generality since they operate throughout a wide spectrum of individual basaltic compositions. It follows that it should be possible to discover these fundamental fractionation controls by an inductive approach—*i.e.*, by choosing a number of typical and significant basaltic compositions and studying their crystallization behavior over a wide range of temperatures and pressures.

This was the initial approach of some of the early experimental petrologists of the late Nineteenth Century. But they were forced to abandon it for technical reasons and because of an inadequate realization of the enormous influence of pressure upon crystallization equilibria. Today the situation is different: we have apparatus available which permits the investigation of sub-solidus and crystal-liquid equilibria over a wide range of pressures and temperatures. Furthermore, with the aid of the electron probe microanalyzer we are enabled to measure the compositions of multicomponent phases and residual liquids in the experimental runs, and thus to follow quantitatively and in detail the fractionation of

complex natural rock systems as a function of pressure and temperature. The marriage of the electron probe to high P-T investigations of natural rock systems gives us the capacity to solve many of the most fundamental problems of petrology. I hope in the future that more and more petrologists will use this capacity.

I would like to emphasize at this point that I am not in any sense attempting to detract from the value of investigations in simple systems. Such investigations are absolutely indispensable for the detailed understanding and interpretation of petrological and mineralogical equilibria, and our present knowledge in these fields is mainly based upon results in simple systems. In my own case, all the ANU work on phase transformations at pressures above 100 kb has been carried out in simple systems. The point I am making is that these two types of investigations are complementary and that the one is rarely complete without the other.

Mr. President, these are exciting days for the experimental petrologist. We now have the capacity to reproduce under controlled conditions in the laboratory the entire range of P-T conditions existing in the mantle down to a depth of about 600 km. Soon, we will be probing still deeper. We have already learned a great deal about the different kinds of phase transformations—both solid-solid and solid-liquid, which largely control the constitution of the mantle down to 600 km, and the origin of magmas. But this is merely the top of the iceberg. Who knows that the next 10 years will bring?

Thank you for your reception and particularly to Dr. Mason for his generous introduction.

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## MEMORIAL OF HENRI BUTTGENBACH

February 5, 1874–April 29, 1964

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Henri Jean François Buttgenbach was born at Ensival, Province Liège, Belgium, on February 5, 1874, the son of a practicing physician. He died at his home in Woluwe-Saint-Pierre, near Brussels, on April 29, 1964, having passed his 90th birthday. He is survived by his widow, one daughter, and one son.

His career spans over half a century. He enters the University of Liège in 1892, where he registers as a pre-engineering student. His first aca-