American Mineralogist, Volume 59, pages 393–401, 1974

PRESENTATION OF AWARDS FOR 1973

The Roebling Medal and the MSA Award were presented at a luncheon meeting of members and guests of the Mineralogical Society of America in the Camellia Room, Baker Hotel, November 13, 1973.

Prior to presentation of the awards, the President made a few informal remarks about the dynamic state of mineralogy and the excellent future before the society. He publicized the joint meeting with other mineralogical societies at Tucson in February 1974 and the next annual meeting at Miami in November 1974. After introducing the members and guests seated at the head table (Dr. P. M. Bethke, Treasurer; Mary Holliman, Managing Editor; Mr. and Mrs. Moore, parents of P. B. Moore; Dr. P. B. Moore; Dr. S. W. Bailey, Vice-President; Dr. and Mrs. G. Tunell; Dr. G. Kullerud; Dr. H. S. Yoder, Past-President; Dr. F. D. Bloss, Editor), he thanked many members of the society for their services, especially Dr. Joan Clark, Secretary, who was sitting "below at salt" because of shortage of seats at the head table. Joan Clark, through the generosity of Richard Erd, had attached a micaceous crystal to the name plates of the guests: these were passed around, and identified by some, but not all present, as tunellite.

The President then presented Paul Moore to the Vice-President for the MSA Award, and Gunnar Kullerud presented George Tunell to the President for the Roebling Medal (see later).

In concluding remarks, the President thanked the outgoing Past-President, H. S. Yoder, for his services to the society, especially with respect to financial matters. The outgoing councilors, G. Switzer and D. Lindsley, were thanked for their services, and the incoming Councilors, A. L. Boettcher and P. M. Orville, were welcomed. The award from the Kraus Fund could not be announced then because of the President's absence by illness from the Council meeting on November 11, but the Council met on November 13 and selected Shu-cheng Yu, a graduate student studying at Pennsylvania State University; the proposed research involves defects in pyroxenes. (The committee members—W. A. Dollase, Jen-Ho Fang, C. Frondel, and G. V. Gibbs—were thanked for very prompt evaluations of the applications). The President thanked members of MSA for many helpful letters, especially those on the proposed short courses; the 1974 Council took the first step to implement a Short Course prior to the Miami meeting, and a final decision is scheduled for the April meeting. Finally the outgoing President welcomed the incoming President, S. W. Bailey, whose record of brilliant research on feldspars and layer silicates plus dynamic administration at the University of Wisconsin augured well for the success of his term of office. The incoming President then thanked the outgoing President for his vigorous leadership, stated some of his plans, and requested advice and help from all members of the society. The 1974 Council authorized preparation of Newsletters, which will acquaint members with the business of the society in 1973–1974.

American Mineralogist, Volume 59, pages 394–395, 1974

Presentation of the Roebling Medal of the Mineralogical Society of America for 1973 to George Tunell

GUNNAR KULLERUD

Department of Geosciences, Purdue University,
West Lafayette, Indiana 47907

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

This year's Roebling Medalist, George Tunell, is a remarkable individual indeed. In his academic career he has excelled in research not only in the field of crystallography but also in thermodynamics of phase equilibria in aqueous systems. He is well known for his abilities to perform intricate experiments and for his theoretical approach to many pertinent scientific problems. Few teachers ever gave more time to their students than did George Tunell nor were more concerned about their students' progress as scientists as well as human beings.

The numerous scientific societies of which he is a fellow or member have recognized not only his talents as a researcher and teacher but also his administrative abilities by electing him to high offices. He has, for instance, served two terms as vice president of the Geological Society of America; he has served as president of the Geochemical Society and as vice president as well as president of our own Mineralogical Society of America.

Somehow George always found time for all these activities and unfailingly produced first-class results as an administrator, as a teacher, and as a researcher. We are honoring him today for his achievements as a researcher, and specifically for his contributions to mineralogy and ore deposits.

George Tunell was born in Chicago, Illinois, on April 4, 1900. He earned his B.S. degree in mining from Harvard University in 1922 and was awarded his PhD degree in geology also from Harvard in 1930. He joined the Geophysical Laboratory of the Carnegie Institution of Washington as a petrologist in 1925 and there performed the experimental work for his PhD degree. He remained with the Carnegie Institution until 1945 when he became at first Visiting Research Associate and later Acting Associate Professor of Mineralogy and Metalliferous Geology at California Institute of Technology. In 1947 he joined the University of California, Los Angeles as an Associate Professor. He was promoted to full Professor in 1949 and remained in this position until 1962. At that time he transferred to the faculty at Riverside where he remained until his retirement in 1967. The following year he was invited to join the Department of Geological Sciences of the University of California at Santa Barbara as Professor Emeritus recalled to active duty. He presently serves in this position.

Dr. Tunell became interested in the processes taking place during oxidation of copper ores at an early stage in his career. He studied these ores in the field and they provided the theme for his doctoral dissertation. One suspects that it was his desire to perform experimental work that led him to join the Geophysical Laboratory of the Carnegie Institution of Washington. There he was indeed introduced to experimental petrology. His early publications, some of them jointly with E. Posnjak, dealt with laboratory treatments of oxidation phenomena that he had observed in the field. Titles such as "Studies of weathering and sedimentation"; "The system, cupric oxide-sulphur trioxide-water"; and "The stability relations of goethite and hematite" are indicative of his curiosity. During these studies Tunell realized more and more the need of introducing sound thermodynamic theory and definition into the mineralogy-petrology field. He published a series of very fundamental papers on this subject. He gradually became involved in X-ray studies of crystal structures and did pioneering work in developing methods in this field. He derived, as an example, the Lorentz correction factor for equi-inclination Weissenberg films. This factor was a necessary prerequisite for meaningful usage of the photographic intensities of diffracted X-rays on equi-inclination Weissenberg films. In 1933 Dr. Tunell published a paper entitled "The crystal structure of tenorite" in which he presented the first scientific proof of square coordination of cupric copper. This paper was the first of numerous contributions to crystallography.

As a student George was already much inspired by Professor Davis's and Professor Bridgman's lectures on thermodynamics. They led to a 37 year study of thermodynamic quantities. The results were published in a book which appeared in 1960 under the title "Relations between intensive thermodynamic quantities and their first derivatives in a binary system of one phase."

All the Tunell publications can serve as models of systematic, careful, and imaginative experimental and theoretical studies. They are, in addition, written with great care to assure correct expression and are characterized by a clarity only achieved by authors who truly understand their subject.

The move from Washington to Los Angeles in 1945 was largely necessitated by health considerations. In California George went through the transition to academic life with enthusiasm. The responsibility of teaching was a challenge which he met with vigor. In California he became greatly interested in mercury ores. Soon he and his students became involved in field and laboratory studies of mercury ore deposition. In this process Dr. Tunell carried with him to his California laboratory much of the approach and the methods and attitudes of the Geophysical Laboratory and integrated these into his university research program. These efforts were successful. The studies involved systems such as HgS–Na_2S–H_2O, S–H_2O, S–H_2O–Na_2S, *etc*, and included investigations of the solubilities of As_2S_3 and Sb_2S_3 in related systems.

In a few years of concentrated field and laboratory studies Dr. Tunell acquired more knowledge and understanding of the formation of mercury ore deposits than is possessed by any other person.

Mr. President, it is indeed a privilege and honor and a great personal pleasure to present to you the Roebling Medalist for 1973, Dr. George Tunell.

J. V. SMITH, President:

George Tunell, having missed by four years the privilege of working with you at the Geophysical Laboratory, it is now my great pleasure to present you with the Roebling Medal in recognition of your distinguished research in the fields of crystallography, mineralogy, and thermodynamics.

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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1973

GEORGE TUNELL

Department of Geological Sciences, University of California,
Santa Barbara, California 93106

Mr. President, Professor Kullerud, Fellows and Members of the Society, and Guests:

The telegram from Professor S. W. Bailey and the letter from Professor J. V. Smith informing me that the Council had selected me to be the recipient of the Roebling Medal in 1973 gave me the greatest pleasant surprise of my entire life. When I had the pleasure of introducing two previous medalists for this award, I did not dream that some day a kind friend would act in a similar capacity for me. I am very doubtful that I deserve a medal of any kind, but I feel most fortunate that the Council of the Mineralogical Society of America has taken a favorable view of my work.

My interest in minerals began at Castle Hot Springs, Arizona, during the winter of 1907–1908. At Christmas time a miner presented me with a cigar box filled with specimens of minerals and ores and after that I began to collect iron-stained chalcedony in Crater Canyon and calcite crystals from the dump of the Black Beauty Mine. In the following years I spent many afternoons at the Field Museum

of Natural History in Chicago, where Dr. O. C. Farrington, the Curator of Geology, helped me with the identification of the specimens that I had collected.

By the time I finished high school I had decided that I wanted to study mining geology. I applied for admission to Harvard University and was accepted as a student in 1918. After completing the mining course, I was enrolled for three years in the Graduate School of Arts and Sciences. At Harvard I benefited especially from the courses in mineralogy and petrology of Professors Palache, Wolff, and Larsen, those in economic and mining geology of Professors Graton and Smyth, those in mathematics of Professor Osgood, those in thermodynamics of Professors Davis, Kemble, and Bridgman, and those in analytical and physical chemistry of Professors Baxter and Richards. If my work has any lasting value, I owe this in very large part to the training and encouragement that I received from these professors.

In July of 1922, while a student at Harvard, I began work at Tyrone, New Mexico, on the oxidation of disseminated copper ores in altered porphyry as a part of Dr. Augustus Locke's investigation of outcrops. During the summer of 1923 I studied the changes across the contact between disseminated sulfide ore and leached capping at Morenci, Arizona. In many thin sections from the specimens that I collected, the complete change from entirely unoxidized sulfide-sprinkled ore to entirely oxidized capping was observed within a microscopical field one-eighth inch in diameter, one side of the field being dotted with grains of pyrite, chalcocite, and covellite with no oxidized iron minerals; the other side of the field with no sulfide grains, but dotted with cavities of the same sizes and shapes as the sulfide grains in the first side, these cavities being partly filled with hematite, goethite, or jarosite, or mixtures of these minerals. Thus it was possible to determine unequivocally what oxidation products had been derived from disseminated copper ore as distinguished from the products derived from disseminated pyritic waste at Morenci. I continued this work at Bingham Canyon, Utah, in 1924. Numerous questions concerning the oxidation process of disseminated copper ores arose from the field and laboratory studies of the specimens of ore and capping from Tyrone, Morenci, and Bingham Canyon. Fortunately for me, when I became a member of the staff of the Geophysical Laboratory in 1925 I had the opportunity to attempt to find answers to some of these physicochemical questions in collaboration with Dr. E. W. Posnjak and Dr. H. E. Merwin. These investigators had already determined the equilibrium relations of goethite, and the basic, normal, and acid sulfates of iron in the ternary system, Fe_2O_3–SO_3–H_2O; and Dr. Posnjak and I determined the equilibrium relations of tenorite and the basic and normal sulfates of copper in the ternary system, CuO–SO_3–H_2O. Dr. Posnjak and I also investigated the portion of the quaternary system, Fe_2O_3–CuO–SO_3–H_2O, of interest in connection with the outcrop investigation. One result of these equilibrium studies was to show quite clearly why the chief ore mineral in the oxidized zone of the great copper deposit at Chuquicamata, Chile, is antlerite and why brochantite occurs there only in a very small amount around the periphery of the deposit. Brochantite occurs in the system, Fe_2O_3–CuO–SO_3–H_2O, at outdoor temperatures only at concentrations less than approximately 1 percent of CuO and approximately 1.2 percent of H_2SO_4. At concentrations of CuO from approximately 1 percent to approximately 12 percent and concentrations of H_2SO_4 from approximately 1.2 percent to approximately 14.7 percent, antlerite is the stable basic sulfate at these temperatures. In the main ore body at Chuquicamata the supergene solutions contained in certain parts of the deposit as much as approximately 12 percent of CuO and approximately 14.7 percent of H_2SO_4, as indicated by intergrowths of antlerite and chalcanthite. Moreover the supergene solutions at Chuquicamata in general evidently contained more than approximately 1 percent of CuO and more than approximately 1.2 percent of H_2SO_4. Thus in this case the approximate concentrations of solutions involved in supergene oxidation and enrichment are known.

After becoming a member of the faculty of the University of California at Los Angeles, I began to investigate mercury ore deposits in 1952. At that time it appeared to me, as it had previously appeared to the late Dr. David Gallagher of the U. S. Geological Survey, that mercury ore deposits would be particularly amenable to the determination of the physico-chemical processes that brought about their formation. In almost all mercury ore deposits cinnabar is the principal ore mineral, although in some cases it is accompanied by considerable amounts of metacinnabar and/or native mercury. Thus the principal problem to be solved in the elucidation of the physico-chemical processes that produced the mercury ores is the determination of the kind of solution in which the large amounts of mercury sulfide present in the ore deposits were transported and from which they were precipitated. Christy, Becker, Dreyer, and Krauskopf had pointed out reasons for believing that these solutions were probably alkaline sulfide solutions. My associates, Professor F. W. Dickson and Dr. R. E. Learned, therefore determined the solubility of cinnabar in the ternary system, $HgS–Na_2S–H_2O$, and the quaternary systems, $HgS–SiO_2–Na_2S–H_2O$ and $HgS–Sb_2S_3–Na_2S–H_2O$, over the ranges of temperature, pressure, and concentration within which mercury ore deposits are believed to have been formed. Mercury sulfide is appreciably soluble in slightly alkaline solutions containing HS^- and S^{2-} ions, whereas it is one of the least soluble of all metallic sulfides in pure water. Moreover, Learned has demonstrated that solutions simultaneously saturated with quartz and cinnabar contain only about one-fourth as much HgS as similar solutions containing no silica. Thus the addition of silica to a solution saturated with HgS would cause the precipitation of most of the HgS. The greatest mercury ore deposits in the world, those at Almadén, Spain, which I had the opportunity to visit with Professor Kullerud in 1971, are, according to Ransome, chiefly replacements of quartzite. Also the greatest mercury ore deposits in the United States, those at New Almaden, California, were formed chiefly by replacement of silica-carbonate rock according to Bailey and Everhart. It is clear from the work of Learned that if alkaline sulfide solutions entered the sites of the ore bodies at Almadén and New Almaden carrying mercury sulfide, but unsaturated with silica, they would have been capable of dissolving quartz and the solution of the quartz in turn would have caused the precipitation of cinnabar. The existence of hot springs at Amedee, Lassen County, California, Cedarville, Modoc County, California, and Boiling Springs, Valley County, Idaho, in which we have observed that substantial quantities of cinnabar and metacinnabar are being deposited at the present time from boiling alkaline sulfide solutions is, I think, a further indication of the correctness of the principal conclusions reached by Christy, Becker, Dreyer, and Krauskopf regarding the nature of the solutions responsible for the formation of mercury ore deposits.

In conclusion I would say that my work has been concerned mainly with problems of ore minerals in one way or another. A previous medalist stated in his acceptance speech several years ago that his regret was that he had done so little in the vast field of minerals and always at so slow a pace. I feel that these words apply to me much more than they did to the medalist who wrote them.

Mr. President: I accept this medal with more thanks than I know how to express; I am indeed most grateful to the Roebling Medal Committee and to the Council of the Society for this award.

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Presentation of the Mineralogical Society of America Award for 1973 to Paul Brian Moore

J. V. SMITH

University of Chicago, Chicago, Illinois

[A toned-down version of ad-libbed remarks]

Mr. Vice-President, Members and Guests of the Society:

Paul B. Moore is a brilliant young mineralogist. Although only 32 years old his publications amass to a grand total of 74 as of January 1973. Some years ago, some scientists thought that Paul did not work on really important minerals such as feldspars and pyroxenes, and that his work is concerned with odd-ball species. Without denigrating the common rock-forming minerals in any way whatsoever, I want to show that Paul's studies are extremely important indeed.

But first a brief biography. Paul was born in Stamford, Connecticut, on November 24, 1940, and moved to New Jersey at the age of 9. In sixth grade, he became fascinated by minerals and during the next decade developed into one of the leading amateur mineralogists in the world. In 1958, he entered Michigan Technological University to study mining engineering, but soon shifted his interests into physics, chemistry, and mathematics. In 1962, he approached Jerry Fisher about working for a Ph.D. on phosphate minerals, but Jerry was approaching retirement. Paul, with an NSF Fellowship in hand, thereupon commenced studies with me—I deliberately write "with" and not "under," because his precocious talent was immediately obvious. The Chicago faculty essentially waived all degree requirements after the first few months, and Paul sailed on to a brilliant degree some two and a half years after entrance, including a quarter at the Geophysical Laboratory with Gunnar Kullerud.

A National Science Foundation Post-Graduate Fellowship allowed Paul to visit Stockholm where he cleared up some of the uncharacterized species from Långban, especially those pointed out many years earlier by Gustav Flink.

Recognizing the exponential growth in Paul's production, we appointed him at Chicago where he became a (full) professor in July 1972.

Let's look at a few of Paul's papers to demonstrate his interests and abilities.

In 1964, we asked how many framework structures could be built with tetrahedral nodes at the corners of Archimedean polyhedra. Two of these were already represented by the commercial zeolite molecular sieves, Linde Type A and the faujasite family. Another one is sodalite; a third zeolite, Mobile ZK5, was discovered simultaneously; and another one may have been characterized this year. Since then Paul has studied topology with great thoroughness and has applied it to many of his crystal structures. For example see his 1970 paper on structural hierarchies among minerals containing octahedrally-coordinating oxygen; and, again, his 1973 paper on bracelets and pin-wheels, a topological-geometrical approach to calcium silicate and alkali sulfate structures. Many of his minerals have strong pleochroism, unusual hardness, or both. It is not improbable that some of these will attain commercial importance either for solid-state electronic devices, or gems, or other applications.

In 1965, Paul published a structural classification of Fe–Mn orthophosphate hydrates. Not only did he purge the literature of many erroneous data, he coordinated the accurate data into a coherent scheme. Paul's success in this area derives first from a thorough knowledge of the field relations of phosphate minerals gained by his own field trips, by his extensive correspondence with amateur and professional mineralogists, and by critical reading of the old literature; and second, by his careful optical and X-ray studies. The 1973 paper on pegmatite phosphates in *Mineralogical Record* is a gem that is of value to all mineralogists whatever their degree of training.

Just a quick look at silicates. In 1968, the crystal structure determination of sapphirine appeared; in 1970, that of β–Mg_2SiO_4, a mineral almost certainly present in the upper mantle; in 1972, the determination of the structure of merwinite, a high-pressure mineral whose analog may exist in the Earth's mantle.

Paul's language is delightfully exotic: *e.g.*, his description of wightmanite as a natural drain-pipe. Unfortunately some of his mineral names have raised a few hackles, such as eveite, a relative of adamite, and his double-barrelled names such as julgoldite.

Paul takes on a new interest every year or two. In the middle sixties, he concentrated on playing the organ in Rockefeller Chapel. Recently, he has returned to a boyhood hobby, that of collecting butterflies, no longer in New Jersey, but now in the basin of the Peruvian Amazon and in south-east Asia.

Paul is a perfect example of how a first-rate amateur mineralogist can develop into a first-rate professional. All members of our society can be proud of him.

Mr. Vice-President, it is a deep privilege to present such a brilliant mineralogist to you for the MSA award.

S. W. BAILEY, Vice-President:

Dr. Paul Brian Moore, on behalf of the Society, I am very pleased to present to you, along with my own congratulations, the Mineralogical Society of America Award for 1973.

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Acceptance of the Mineralogical Society of America Award for 1973

PAUL BRIAN MOORE

University of Chicago, Chicago, Illinois

" 'Alle Wahrheit ist einfach'. Ist dass nicht zweifach eine Lüge?" Nietzsche *(Götzen-Dämmerung)*

Mr. President, Colleagues and Friends:

My fascination in minerals began when, aged 12 years, I was inspired by Bill and Mary Welsh, both dedicated school teachers and outstanding amateur mineralogists who brought the world of beautiful and mysterious natural things to the classroom. Soon after, I was known to pester nearly every adult in sight who had an automobile and free weekend so that I could get to the Parker Shaft mine dump in Franklin, New Jersey—some thirty miles from home—to collect among the most extraordinary minerals known to science. My understanding and intelligent parents early encouraged this combination of curiosity and quest for goals. I even managed to infect some of my boyhood chums with the interest, and we could be witnessed stumping up Grand Avenue in Paterson, New Jersey, from bus stop to destination—bristling with crowbars, sledge hammers, and collecting bags to Garrett Mountain in the First Watchung Range and the then-accessible New Street quarries where, after diligent effort and bruised knuckles, most beautiful zeolites were culled. My hobby became "fixed" at 15 when Brian Mason, then at the American Museum of Natural History in New York, pressed reprints on Långban and Franklin mineralogy into my hands, and I soon fell under the spell of Palache's *Professional Paper* **180**, the classic treatise on the mineralogy of the Franklin and Sterling Hill zinc mines. Charles Palache was my hero then, and I once planned a "Journey to the East" to seek out my Guru, but as a lad I was too shy and frightened to meet the Great One. I regret never having met Palache, but Cliff Frondel doubtless carries on the Palache spirit, and I am pleased to count this esteemed colleague among my personal friends.

Since then, my goals have changed but my style has not. My collection of 4,000 personally collected samples of some 1,000 species no longer exists intact. It was sold piecemeal to provide funds for my expeditions to rather unusual and inaccessible places in search for rare and magnificent *Rhopalocera*. The

wheel has turned one cycle; my boyhood amateurism and enthusiasm revisited me again, now directed toward butterflies! The joy from uncovering a rare roeblingite at Franklin was magically transformed into the joy from capturing *Morphotenaris schönbergi*, one of the glories of my collection, in a remote New Guinea jungle.

My early and rather peculiar interest in rare and improbable things manifests itself in my choice of problems to this day. The weary splitting of hundreds of rockbridgeite fibers to obtain that one single crystal was a pleasure even surpassing my discovery of that roeblingite many years ago. The quest, spanning one decade, for single crystals of mitridatite, strunzite, and cacoxenite eventually brought triumph. The personal removal of some thirty species of phosphates, ten of which are new to scientific intelligence, in all their paragenetic glory from one altered triphylite pod exposed only six months ago at the Palermo pegmatite, another childhood haunt, brought back flashes of old phantasies. The lonely and countless hours in the computation center wrestling with the "blue monster" were hardly fruitless in retrospect.

Yet I wish not to paint myself as a "Childe Harold." Much of my patience and zest for hard work goes back to early introduction into the brass-knuckles facts of life by my parents and grandparents and their instructions on the dangers and ultimate dissatisfactions of idleness, sloth, and sloppiness. Paraphrasing Lord Acton, "innocence corrupts and impotency corrupts absolutely." I smile when I think of a translated quote from the old Bruckner to the young Mahler: "Now I have to work very hard so that at least the Tenth Symphony will be finished. Otherwise, I will not pass before God, before Whom I shall soon stand. He will say 'why else have I given you talent, you son of a bitch, that then you should sing My praise and glory? But you have accomplished much too little!" Alfred Russel Wallace wrote his monumental "Narrative of Travels on the Amazon and Rio Negro" when he was but 30—based on three years of solitary and dangerous expedition—followed by his even more colossal narrative of his seven year expedition throughout the Malay Archipelago. Compared with men as these, my contribution to human knowledge and enlightenment has been miniscule. Yet, I continue to derive great personal satisfaction from my studies and trust this shall continue for some years to come.

Professionally, many colleagues and professors of the past have helped me along the way. Most of all, I single out Joe Smith as the wellspring for my eventual professional development. The improbable event of having the President of the Society, once my mentor, provide the praeludium to my presentation, is too much a temptation to let pass in silence.

I wish to offer a few reflections about our Society and what it stands for. It is not out of arrogance but out of deep concern that you must hear me further. The Mineralogical Society of America was created largely through the generosity and support of dedicated amateurs as well as professionals, the most signal figure having been Colonel Washington Roebling. In its early years, many virtuoso performances in pure mineralogy were offered—the papers of Palache, Berman, Peacock, and Schaller, for example. Where is this spirit today? What does the Society symbolize, if anything? Why aren't more of our dedicated amateurs present at this meeting? Is it true that our science has become so obscure and specialized that the amateur can no longer play a supportive role? Or have we become too arrogant, too ensconced in our specialities? Who among us can go out and teach the inquisitive man in the street of the mysteries and marvels of the natural inorganic world? Who among us will strive to encourage the devoted amateur and treat him as an equal? Who among us can claim equally deep interests in the humanities and in other realms of nature—the dedication to and love of music of a Palache or a Peacock, the love of flowers of a Schairer, the love of gems of a Foshag? The Society represents to me a human institution, not a hard, cold, amorphous thing. It represents a focal point where the creators of specific, hard, objective knowledge become persons, where authors and readers become human and real. If the sparks of the Society—enthusiasm, love of minerals, a sense of confraternity—are quenched, it will surely die an anonymous death without even the dignity of a funeral cortege.

I say these words because I am proud of my award and the Society of which I am a member and want future recipients to be assured that they can look to their Society with pride and not with a sense of achieving a goal in silver which after its receipt is transmuted into lead.

I conclude my reflections with two highly personal observations. First, the most brilliant crystal structure analyst of minerals in this country, if not the world, is my colleague and chief collaborator, Takaharu Araki, who analyzes Patterson maps as a Go champion proceeds with his "Bead Game" on to victory. Second, I have so far failed to provide any student to the scientific community who has received solely my guidance, and the future prospects of finding a willing soul at the moment appear dim. This is unfortunate, for the problems in pure mineralogy are seemingly endless and my list of programs and projects would take more than one life-time to complete. Naturally, any dedicated student with a talent in pure mineralogy is always welcome in my laboratory, which includes the great outdoors as well as the solitude of the X-ray and goniometric facilities.

Thank you!