spars. Such was the fame of Taylor's laboratory that a student from Waupaca, Wisconsin, Bill Bailey, was attracted to Cambridge. He became known as the razor king for his skill at cutting tiny cubes with a razor blade. There were no computers then for calculating absorption corrections. Bailey and Taylor proved that the Al and Si atoms are ordered in microcline. Bob Ferguson, Bob Traill, and Paul Ribbe collaborated with Taylor in solving details of the albite structures, and Helen Megaw and co-workers built on Taylor's earlier work on the plagioclase feldspars. Read all about it in the elegant chapter on feldspars in *The Crystal Structure of Minerals.*

President Wones, the Roebling Medal is specifically awarded for creative research, and W. H. Taylor is an outstanding selection. But let me place on record the thanks of so many crystallographers for the additional inspiration of Taylor's creative administration and elegant style. His career is a testimony to the fundamental contribution of crystallography to the advances in mineralogy and petrology, and indeed also to geochemistry and geophysics.

Mr. President, I am deeply honored to present to you such a distinguished pioneer in crystallography for the award of the 1979 Roebling Medal.

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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1979

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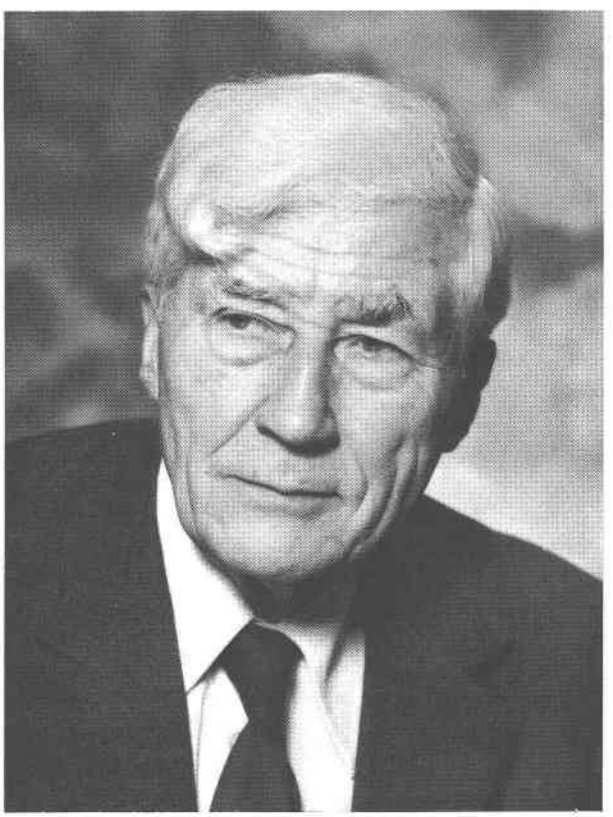
Mr. President, members of the Mineralogical Society of America, ladies and gentlemen:

May I first express my sincere thanks to the Society for the award of the Roebling Medal, which I accept with great pleasure: I am aware that no higher honour for work in this field could be offered. My many friends here would probably agree that I am not often reduced to a state of stunned silence, but when Professor Gibbs telephoned me in Cambridge about a year ago to tell me that I had been named as the recipient of the 1979 Medal, I fear that I became quite incoherent with mingled surprise and delight.

Professor Smith has obviously worked hard on my behalf, and I am grateful that he so willingly agreed to act as my citationist. I liked his light-hearted phrase about 'knocking off' several structures in my early years—at the time it did not feel like that at all. In similar vein, his account of my Christmas afternoon festivities in 1932 might be taken to imply that a couple of hours with a slide rule gave me the feldspar structure—but, of course, mountains of calculations on the feldspars had previously got me nowhere, until a sudden flash (which seemed bound to be correct—and was!) led straight to the solution.



The biographical note printed on your luncheon

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program[1] seems to include information on all my various research activities over about 40 years, and Professor Smith's citation has directed attention in particular to mineralogically important items. I would, however, now like to deal briefly with what I may call more personal aspects of these mineral studies, first in Manchester and afterwards in Cambridge.

Even before entering the University of Manchester as a first-year undergraduate in 1923, I knew from my school reading that I wanted to study X-ray crystallography and—if possible—to undertake research in that field: was I not exceptionally lucky in being able to do just this throughout my professional career? Moreover, on beginning research in the Physics Department in 1926 I stepped straight into the exciting early days of Professor Bragg's attack on mineral structures in general and silicates in particular, and found this project very much to my liking. The team included high-powered visiting workers from all over the world, of whom several appear in your Roebling awards list following Bragg's medal in 1948. Their stimulating influence on this young researcher was invaluable, in supplementing the direct help given by Bragg himself and by members of the Physics Departmental staff.

Among the visitors destined for the Roebling award I must mention two—your Professor A. N. Winchell and the Austrian Professor Felix Machatschki, at that time in Graz, I think.

Professor Winchell's modest bearing, gentle manner and quiet methods did not entirely succeed in hiding his encyclopaedic knowledge of the physical properties of a vast range of crystalline materials—extremely impressive even to a relatively raw beginner like myself.

Nor did Machatschki's gay and light-hearted presence (except when his work was going badly!) in any way diminish the intense interest (for me in particular) of his inspired speculations about the nature of the feldspar structures. For these formed the starting point for my own work on this mineral family, which continued (on and off, more off than on in recent years!) until 1978.

The financial rewards attached to an academic research career may not often be such as to excite envy: but how would you assess the pleasure which I derived, some 35 years after Machatschki's visit to the Manchester Laboratory, when—apparently at his express request—I was invited to Vienna to deliver a lecture on silicate structures, in his honour, at the time of his retirement from the Chair of Mineralogy there—a privilege indeed, which I relished.

No account of the Manchester Laboratory at this time would be complete without at least a reference to one other visiting member—W. H. Zachariasen, who was reputed to bring in each morning a new structure which he had determined the previous evening. This may or may not be literally true—but it gives a correct impression of a research team humming with activity and enjoying it!

An interlude of about 10 years, following my departure from Manchester in 1934, and devoted in part to widening my experience in various aspects of structural studies, served as preparation for what was to become my major life work, in Cambridge. For in 1945, the war drawing to its end, a decision had to be taken whether to remain in Manchester as Head of the Physics Department in the College of Technology, or to accept Bragg's invitation to rejoin him in the Cavendish, with responsibility for research in crystallography. In retrospect, of course, there should have been no hesitation in choosing the brilliant prospect offered by the Cambridge appointment, but at the time I was very loth to abandon wartime plans, about to be brought into effect, for a Physics Department (with status enhanced out of all recognition) in what was to be a new and greatly-enlarged College of Technology. [Later called UMIST, with a distinct suggestion of the MIT style—not accidental, I think!] In fact, I made the right decision and lived happily ever after.

As was to be expected at such a time of transition the small wartime team of Cavendish crystallographers melted away to take up appointments elsewhere—but not before entertaining Mrs. Taylor and me to an excellent 'hail and farewell' dinner. In the course of this pleasant event, one after another our hosts and hostesses took us aside and assured us that they were not leaving Cambridge on account of our arrival there; in fact good wishes were showered on us and some very lasting friendships were formed.

In Bragg's Manchester Laboratory from 1926 onwards my aim had been quite uncomplicated—to get on with the analysis of silicate structures, usually in collaboration with others, not infrequently as the senior worker. In Cambridge my task, as I saw it from the beginning, would be quite different. In the first place the Crystallographic Laboratory's resources must be adequate for the expected rapid

[1] To obtain a copy of this material and a bibliography of Dr. Taylor's publications, order Document AM-80-136 from the Business Office, Mineralogical Society of America, 2000 Florida Avenue, NW, Washington, D.C. 20009. Please remit $1.00 in advance for the microfiche.

postwar increase in the numbers of visiting researchers of various degrees of seniority, and of research students, many of whom would be preparing for the Ph.D. degree. Secondly, the Laboratory's effort should be directed not only to the production of original work of the highest quality, but also with an eye to the training of experienced workers who would become leaders of teams on leaving Cambridge to take up senior appointments elsewhere. Also, if the research in the Laboratory was to flourish it must take up a considerable variety of 'lines' to attract the interest of potential entrants, whether young research students or more senior visitors—it would no longer be possible to operate as a one-line research group as recently in Cambridge in wartime (concerned almost exclusively with metals) or as in my Manchester days (with silicates).

At this point I may formally acknowledge my indebtedness to Helen Megaw, William Cochran, and Peter Hirsch (the three pillars of the crystallographic state): I do this with the greatest pleasure and most enthusiastically, knowing better than anyone else how much their efforts contributed over the years to the studies for which I have just received the outstanding honour of the Roebling Medal.

Professor Smith stressed in his citation the importance for mineralogists of Helen Megaw's appointment. Having known her in Bernal's Laboratory in 1934–35, and being also acquainted with her researches during wartime, I was extremely anxious to secure her for my Cambridge Laboratory in 1945, but could offer her only a very junior appointment. It seemed unlikely that this would tempt her to leave Bernal's Birkbeck Laboratory, but discreet enquiry having revealed that a Fellowship in Girton College might be added to my Laboratory appointment, she accepted the opportunity to pursue her crystallographic studies and at the same time indulge her passionate interest in the higher education of women. This was a major triumph for my Laboratory, and I knew it! In due course—but with deplorable slowness—more senior status was secured on her appointment as University Lecturer.

My own feldspar studies, though proceeding slowly in the period 1934–45, had not been abandoned, and were now revived under favourable conditions. Well-qualified recruits joined the feldspar project from laboratories in U.S.A., Canada, Australia and elsewhere; some worked with Helen Megaw, some with me: their publications make a formidable list. At this time also the Department of Mineralogy and Petrology set up its own feldspar studies, in full cooperation with the Crystallographic Laboratory, and I was very gratified to find myself involved with research carried out in that Department by members of their staff who had very recently emerged from Ph.D. courses in my Laboratory.

Of the mineral structures which I was able to determine, the feldspars would be regarded as the most important, and I have already boasted that my studies of this family continued until 1978. This in fact came about in the course of a collaboration over about 12 years between the small remnant of my shrunken Crystallographic Laboratory and Professor Sergio Quareni's group in Padova. This was a most enjoyable joint effort which kept me in touch with feldspar problems for 6 or 7 years after my official retirement, but was terminated so very sadly by Quareni's untimely death.

I think, therefore, that I may with justice claim minerals as my first love and my last love in fifty happy years of crystal structure analysis, now so splendidly rewarded by my new distinction as Roebling Medallist. Mr. President, this is a very happy day for me.

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Presentation of the Mineralogical Society of America Award for 1979 to David Ho-kwang Mao

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David Ho-kwang Mao was born on June 18, 1941 in Shanghai, China. When he was only seven years old, his family fled from the mainland and resettled in Taiwan. His father was a general in Chiang Kai-shek's army. Dave entered the National University of Taiwan in 1959 and received his Bachelor of Science

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