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 Kaishek's army. Dave entered the National University of Taiwan in 1959 and received his Bachelor of Science

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degree in Geology in June, 1963. Then for a year he served his country as a second lieutenant in the Air Force.

It was in the fall of 1964 that our paths crossed for the first time. Dave had applied and been accepted at the University of Rochester for graduate work in geology. Taro Takahashi and I had big plans for setting up a laboratory for studying the properties of minerals at high pressures and temperatures, using the diamond anvil cell just developed at the National Bureau of Standards, and Dave was to be our first graduate student. So, when we went together to pick him up at the Rochester airport, we were both anxious to see if Dave looked like the kind of student who would latch onto a new technique and make it work well.

At first, he seemed rather shy, but at the same time, he seemed to have an air of confidence about him. As it turned out, we needn't have worried. The first task we asked Dave to undertake was to extend the compression measurements that we had started on iron and iron–nickel alloys. Instead, he quietly proceeded to show that what we had done to date was wrong. He straightened it up and only then did he continue to extend the measurements to higher pressures and other compositions. That quickly led to a Master of Science degree in 1966 and a couple of very important publications on what we could expect the Earth's core to be like if it consists mainly of iron alloys. Next, he started on his Ph.D. project, which was to measure the compressibilities of the gamma phases of olivine. He finished it in record time and submitted a thesis in 1967, just three years after he arrived. This work also led to some important publications which provided valuable data on which many mantle models have been based.

We were very interested in NaCl at that time, not only because it served so well as an internal pressure standard but also because there had been reports that it underwent a phase transition at quite low pressures: 20 to 30 kbar. We looked hard for any evidence of a transition in that range but could find none. We were about to publish our negative results when Dave noticed some extra lines in some of his X-ray patterns of samples in which he was using NaCl as a pressure standard. When he measured them, they turned out to be the high-pressure phase of NaCl; but, instead of occurring at 20 to 30 kbar, he found the high-pressure phase at nearly 300 kbar, one of the highest pressures to be achieved in a static high-pressure device at that time.

By the time Dave received his Ph.D. degree, he had been co-author on four papers and had collected data that led to three more papers within a year. He has never let up since. He stayed on with us a few months as a post-doc. Then two important things happened in his life: he married his lovely wife Agnes and he accepted a position at the Geophysical Laboratory.

At the Geophysical Lab he greatly expanded the capability of the diamond anvil cell. Also, he quickly rose to a tenured position. More than once, Hat Yoder and Phil Abelson have expressed thanks for sending Dave their way.

Dave's accomplishments are numerous, and time permits mention of only the high points. Perhaps his best-known accomplishment has been his joint work with Peter Bell in which he achieved a pressure of 1.7 megabars, comparable to conditions well within the Earth's core. In our high-pressure work, we have witnessed the failure of many diamonds, but the nature of failure has always been the same, brittle fracture, the small, unmistakable, devastating sound of crunching diamonds. However, Dave and Peter witnessed for the first time a new mode of diamond failure, plastic flow. When they unloaded their sample, they found a dimple in one of the diamond anvil faces.

Dave's talents extend well beyond the experimental. He has made calculations on the stability of iron and its compounds. Then to the best of my knowledge he was the first to apply the powerful technique of electron microprobe to samples that had been subjected to high pressures and temperatures in the diamond cell. He and Peter reported that iron can coexist in the metallic and ferric states at high pressure, a discovery that has had a profound effect on models for the origin and evolution of the Earth.

Other important experiments include measurements of the effects of pressure on optical absorption and electrical conductivity of ferromagnesian silicates and oxides. From their results, they were able to conclude that the mantle is too dark-colored for radiative transfer to be an important mechanism of heat flow. These results not only led to a better understanding of processes within the Earth but also within the atom.

At present, Dave is studying solid hydrogen at high pressure, research whose importance extends far beyond geology. If hydrogen becomes metallic within the first few megabars, I am confident that Dave will be the first to see the glint of a metallic reflection off its surface.

Mr. President, it is with much pleasure that I present to you the most imaginative and competent experimental mineralogist I have known.