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Acceptance of the Mineralogical Society of America Roebling Medal for 2005

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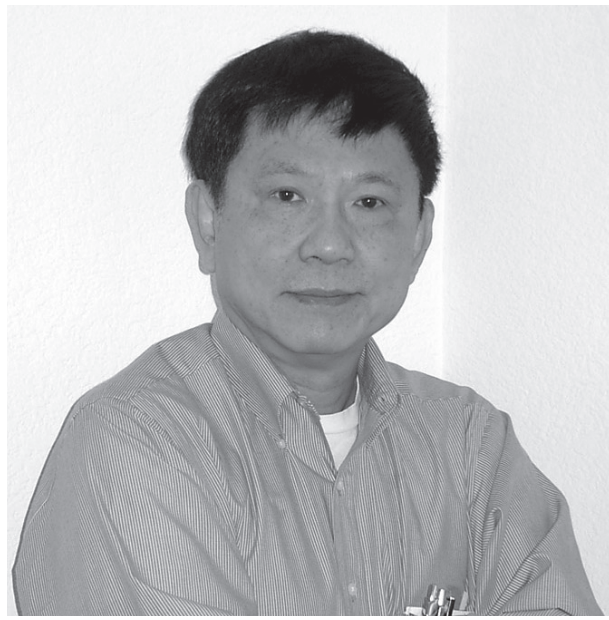

Mr. President, Ladies, and Gentlemen:

Thank you very much for this great honor of receiving the Roebling Medal. This is also a humbling moment because I know the Medal actually belongs to the large high-pressure mineralogy community, including my many friends and mentors in the audience. I happened to be with the right people at the right time in the right place. The last time I counted, the total number of coauthors on all my publications reached 394. Adding the people who have helped but have not coauthored with me, the total must be over 1000. The field is very active and covers a diversity of applications as demonstrated by the dynamic scientific program, yesterday and today, organized by Bill Bassett, Yingwei Fei, Rus Hemley, and Anne Hofmeister.

The field was not always this exciting, nor the future this clear when I started four decades ago. In fact I had serious doubts when I was an undergraduate student in the Geology Department of National Taiwan University. The on-going practice for senior thesis projects was to assign each student a 1:25,000 quadrangle for independent geological mapping. The mapping for the whole island was almost complete, and our Class of 1963 reached Hengchun Peninsula, the southern tip of Taiwan; a step further, and I would have to be an oceanographer. Although I understood that geology is more than mapping, I was still concerned about the future, and came to the U.S.A. where at least the area of land was considerably bigger.

In 1964, I went to the University of Rochester, New York, and very fortunately, became the first Ph.D. student of two young assistant professors, Bill Bassett and Taro Takahashi, who had started a diamond-anvil cell (DAC) program to explore the Earth's deep interior. The depth added a vast new dimension from the flat surface mapping and sounded attractive to me. Bill and Taro had just made the very important discovery of hexagonal-close-packed iron (ε-Fe) that is the main constituent of the Earth's inner core. Several years earlier, Ted Ringwood converted fayalite into the spinel phase (which was later named ringwoodite), and for the first time we had a non-crustal mineral in hand, which only formed in the deep mantle. I did my Master's thesis on determination of the transition pressure and equation of state of α–ε Fe, and my Ph.D. thesis on the pressure-volume equation of state of Fe-rich silicate spinels synthesized by Syun-iti Akimoto.

This was also the time that plate tectonics were being established and turning previous doubters into believers. The two-dimensional surface view, such as "continental drift," that was championed by my teacher Tingying Ma at Taiwan, must be extended to depth for understanding the root and mechanism of the three-dimensional plate motions. Later seismic tomography and mantle geodynamics were developed, while the high-pressure properties of minerals were the crucial link. The experimentalists fell far behind. The research frontier was trying to find the "post-spinel" phase beyond the 670-km discontinuity, and we hardly had any information on the mineral properties at depth. Suddenly there appeared to be a gigantic task and opportunity for research, if only we had the right tool to reach the pressure and probe the properties.

In 1968, I went to Geophysical Laboratory (GL) to work as a postdoc with Pete Bell under Phil Abelson's directorship, was later promoted to staff position by Hat Yoder as the GL Director, and stayed there my entire career. The GL is a unique place where the research staff has complete freedom to do whatever he/she chooses. There is no assigned mission, nor accountability of one's research time, nor requirement of getting grants. This unique system brings the total dedication out of individuals who can sustain focused, life-long pursuit in novel but not necessarily popular directions. At GL, I learned the art of synthesizing silicate glasses from Frank Schairrer and Hat Yoder, gas-mixing furnace from Neil Irvine, hydrothermal synthesis from John Frantz, Mössbauer spectroscopy from Dave Virgo, and X-ray crystallography from Larry Finger and Bob Hazen (no implication that I reached anywhere near their expertise). Pete's favorite subject

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was crystal-field spectroscopy and pressure-induced high-spin-low-spin transition campaigned by Roger Burns and Bill Fyfe then. With my familiarity in conventional petrographic microscopy, we built a double-beam optical spectrometer and used it to study microscopic crystal-field spectroscopy on lunar minerals that had just returned from the Apollo and Luna missions.

This experience was extremely useful to the development of a series of microscopic optical spectrometers for high-pressure studies, including absorption, Raman (with Shiv Sharma, Bjorn Mysen, and later Rus Hemley and Alex Goncharov), Brillouin (with Bill Bassett, Ed Brody, and Hiro Shimizu), and infrared spectroscopy (with Patrick Wong, Jian Xu, and Anne Hofmeister), thus opening the diamond-window to "see" and characterize the minerals in-situ at high-pressures conditions. I used the extensive high-pressure capabilities at GL, including the Tuttle bomb, Boyd-England piston-cylinder apparatus, Bridgman squeezer, and Birch gas apparatus for studies of deep crust and upper mantle minerals. It was clear that we must reach pressures higher than 30 GPa, which was the ceiling for all static high-pressure equipment. Multimegabar pressures had been claimed with diamond indenters and the split-sphere apparatus, but quantitative sample characterizations, including pressure measurements, were not conducted with these instruments. Pete and I modified the DAC to go above 100 GPa in 1975, and eventually reached the entire pressure range of the Earth (360 GPa) with Jian Xu. Meanwhile Li-chung Ming and Bill Bassett at Rochester invented the laser-heating technique to bring DAC samples to the geotherm pressure-temperature conditions, and John Liu, another student of Bill, began to synthesize numerous "post-spinel" phases with the laser-heated DAC.

I had witnessed the amazing advances of the electron microprobe from manual operation and WANG calculator analysis to total automation, and X-ray crystallography from the Ilford film method in the 1960s when the structure determination of a mineral would qualify for a Ph.D. thesis, to the completely automated four-circle diffractometer which solves the structure by itself. Larry Finger played a key role in both automations. At this high rate, however, all common minerals would have been studied very quickly. By 1980, the excitement in experimental petrology, crystallography, and high-pressure mineralogy seemed to quiet down. At an AGU meeting, a prominent geophysicist asked me, "Now that all major lower-mantle minerals have been discovered, and we all know the outer core is molten iron with some impurity and the inner core is almost pure solid iron, what is left for high-pressure mineralogists to do?"

This time I was not concerned. With so much to be explored in the newly opened high-pressure dimension, these fields certainly have not peaked out but reached a plateau for the next stage take-off. High pressures drastically alter all materials properties; we are facing a clean slate at every pressure level. In fact at high pressures, very little was known about even the most abundant mineral in the universe—solid hydrogen. The development of modern physics and quantum chemistry in the previous century started with hydrogen, and the history was to be repeated at high pressures. Using the enabling DAC techniques, Pete and I crystallized hydrogen at 5.7 GPa, observed Raman vibron turnover up to 63 GPa, and characterized the high-pressure hydrogen sample with infrared and Raman spectroscopy. With Rus Hemley and colleagues, we eventually reached 300 GPa, which is twelve times higher pressure than the metallic transition predicted in the 1935 classic paper by Eugene Wigner and Hilliard B. Huntington. We found high-pressure hydrogen was exceedingly rich in phenomena and far more complicated and interesting than W&H's original conjecture.

In the mid-1980s, the GL went through a period of significant transformation. The laboratory moved a couple of miles away from its century-old home base at 2801 Upton Street to collocate with its sister laboratory, the Department of Terrestrial Magnetism, at 5251 Broad Branch Road. Pete left academic research to become Vice-President of Norton Company and Saint Gobain, and applied his knowledge of ceramics to enrich the society. Rus Hemley joined GL, and became my principal collaborator. Charlie Prewitt succeeded Hat Yoder as GL Director. Charlie launched the celebrated Center for High Pressure Research (CHiPR) program, which led to a series of major breakthroughs and forever changed the high-pressure science.

The new development at GL was most clearly reflected in Charlie's appointment of new GL staff members, i.e., Ron Cohen, a world leader in mineralogical theories, Yingwei Fei who advanced both multianvil apparatus and DAC frontiers, George Cody who brought in NMR, soft X-ray spectroscopy, and organic biogeochemistry, and Rus Hemley who pioneered numerous novel directions in multidisciplinary high-pressure science. I benefited greatly from the opportunity to work with a huge list of talented young predocs, postdocs, and visiting scientist researchers at GL and COMPRES. Together we explored various heating and cooling methods to cover temperatures from 35 K to 6000 K at megabar pressure. We developed laser, electromagnetic, synchrotron, and most recently neutron techniques for probing electronic, magnetic, structural, elastic, and rheological properties of single-crystal, polycrystalline, and amorphous samples at high *P-T*. We investigated over 100 different metals, alloys, oxides, silicates, hydrates, and sulfide minerals and discovered myriad unexpected transitions. For each old problem that we solved, however, ten new problems emerged. This was like starting with a trace clue of a diamond mine. The more I dug the bigger it appeared: from handful to truckload and finally reaching a core that was a colossal single-crystal diamond. The metaphor may even be taken literally regarding the development of large single-crystal CVD diamonds that started as a CHiPR project supporting Chih-Shiue Yan to grow diamonds for anvil use.

So positive about the future, I even encouraged my third and youngest daughter Wendy Mao to switch fields from materials science to mineral physics. Three is the lucky number, and she actually did switch. (Her two elder sisters do not want to have anything to do with scientific research.) Last but certainly not the least, I am most grateful to my wife Agnes Mao who is far more than a supporting spouse but a major partner in research. She retired from her 30-year librarian career at George Mason University, and took over many of my tasks that amount to two-thirds of my daily work, including correspondences, routine reports and communications, scheduling, travel arrangements, accounting, and of course, literature searches. As a result, I have been able to focus on the fun part of the research and have become three times more efficient. I would like to thank each and every one of you for your support. Thank you all.