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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1999

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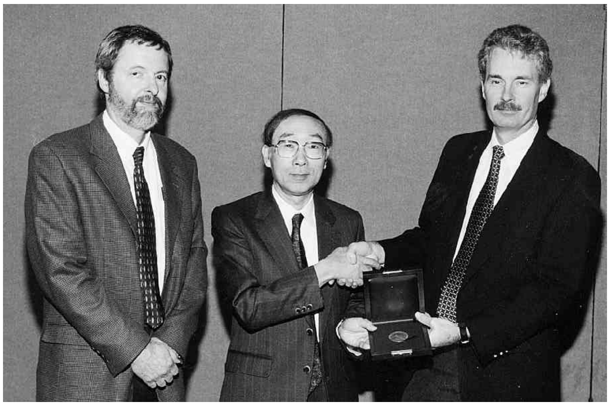
Mr. President, members of the society, and guests:

It is a great honor to be awarded the Roebling Medal by the Mineralogical Society of America. When I was informed of the news from the then-President Bruce Watson, I was surprised and felt that such an honor is more than I deserve, but at the same time I felt deep gratitude to the members of the nominating committee and people who supported me for the honor. I am most grateful to those people. I am also honored to be cited by Bruce Watson for the award and I thank him for a detailed citation of my work. Although his citation covers more than what I thought I did, I would like to introduce briefly my research career, and in particular, the motivation of my research.

I became interested in igneous petrology while I was an undergraduate student at the University of Tokyo in 1956, and I took petrology as major in the Geology Course. As my research subject, my supervisor H. Kuno gave me the problem of differentiation of basalt magma, which was then one of the most popular subjects in igneous petrology, and I studied differentiated dolerite sills in northeast Japan during both undergraduate and graduate courses. During the course of this study, I became more interested in the problem of the origin of basalt magma. At that time very few petrologists were studying this problem in Japan. A petrologist friend of mine at another university was told by his supervisor that graduate students should not study such a big problem, but should concentrate more on the descriptive work of rocks. Fortunately, Kuno did not tell me this, so I began to study the origin of basalt magma, which became my lifelong research subject.

My first objective was to examine Kuno's hypothesis that tholeiitic magma is formed at shallower levels in the upper mantle than alkali basalt magmas, and specifically, in the Japanese islands, quartz tholeiitic magma is formed at depths shallower than 200 km and alkali olivine basalt magma at depths greater than 200 km. My estimation based on a simple calculation using the experimental data of H.S. Yoder and C.E. Tilley indicated that quartz tholeiitic magma can only be formed at depths shallower than 35 km. I strongly wanted to check my estimation with high-pressure experiments; however, at that time, there were no high-pressure laboratories in Japan. Kuno, therefore, suggested that I go to the Geophysical Laboratory, and very luckily, I was appointed a post-doctoral fellow at the Geophysical Laboratory in 1962. That was the start of my career as an experimental petrologist.



Left to Right: Bruce Watson, Ikuo Kushiro, and John Ferry.

My first experimental work at the Geophysical Lab was to re-determine with J.F. Schairer the 1 atm liquidus phase relations of the system forsterite-diopside-silica determined by N.L. Bowen in 1914. Applying an X-ray diffraction technique to estimate the composition of pyroxenes, we found several new aspects on the liquidus phase relations, and revised the phase diagram of this system. Although this was not my planned research, I learned from J.F. Schairer and H.S. Yoder the fundamentals of experimental petrology through this work, which was valuable for my subsequent high-pressure experimental work.

From the second year, I initiated my planned high-pressure experiments related to the origin of basalt magma using the piston-cylinder apparatus developed by F.R. Boyd and J.L. England. My first work was to examine the effect of pressure on the liquidus phase relations in the systems forsterite-diopside-silica and forsterite-nepheline-silica, and the compositional change of melt coexisting with olivine and pyroxene. The results of the experiments indicated that the composition of melt becomes more silica-undersaturated and olivine-rich with increasing pressure in these systems, and this appeared to support Kuno's hypothesis, although the pressure range needed to produce tholeiitic and alkali basaltic melts were much smaller than estimated by Kuno. At that time, I considered partial melting of mantle peridotite as the process of generating various basalt magmas and did not take into account fractional crystallization, and such an approach was criticized by M.J. O'Hara.

In the high-pressure experiments on basalts, I worked with H.S. Yoder on the stability relation between basalt and eclogite, which was then another hot subject in petrology in relation to the nature of the Moho discontinuity. We studied the system forsterite-anorthite expecting to produce an eclogitic mineral assemblage at high pressures. However, we found that a spinel lherzolite mineral assemblage becomes stable at about 1 GPa. It was exciting, and we could locate the stability boundary between plagioclase-lherzolite and spinel-lherzolite.

In 1965 I returned to the University of Tokyo and began teaching a laboratory course of petrology. Although my research time was limited, I tried to find time for my own research. Fortunately, S. Akimoto just established his high-pressure laboratory at the University of Tokyo and I was able to collaborate with him and Y. Syono on high-pressure experiments. At that time, we were interested in the possible role of water in the formation of the low-velocity zone in the upper mantle. Therefore, we determined the solidus of mantle peridotite under hydrous conditions at pressures to 6 GPa. We showed that the hydrous solidus is 300–700° lower than the anhydrous solidus at pressures between 1 and 6 GPa, and that low-degree partial melting of the upper mantle in the presence of a small amount of water could explain the low-velocity zone. We also determined the stability of hydrous minerals in the mantle, and were surprised to find that phlogopite is stable at least to 7 GPa and at temperatures up to 1200 °C. During these studies I became interested in the effect of water on the composition of magma formed in the upper mantle. Fortunately, H.S. Yoder again invited me to the Geophysical Lab, and I returned to the Lab in late 1967 to study this problem. On the way to Washington, I stopped at the Hawaiian Volcano Observatory by the invitation of R.S. Fiske, and I saw for the first time a spectacular eruption of Kilauea, which happened to start the night I arrived.

At the Geophysical Lab I started the planned experiments with H.S. Yoder on the melting of mantle minerals under hydrous conditions, and soon I found that incongruent melting of enstatite persists at least to 3 GPa under water-saturated conditions, compared to less than 1 GPa under anhydrous conditions. The result was exciting because it indicated that silica-saturated magma can be generated in the upper mantle under hydrous conditions. I extended the hydrous experiments to more complex systems and showed that silica-enriched melts are produced by partial melting of mantle peridotite under hydrous conditions. The problem was to determine just how silica-rich the melts were in the case of melting of natural mantle peridotites, and this problem was intensely debated a few years later.

In 1969 I returned to the University of Tokyo, and I began working with my colleagues on the melting of natural mantle peridotites under hydrous conditions. To determine the composition of melts formed by partial melting of peridotite, we analyzed with an electron microprobe analyzer the quenched partial melts between crystals. We showed that hydrous quenched partial melts are really silica-rich, and some of them are andesitic or even dacitic. The compositions of quenched partial melts were, however, affected by quench overgrowth of nearby olivine, as pointed out by D.H. Green, and the results were misleading. Reliable data on the compositions of partial melts formed in natural peridotites were obtained later by improved techniques.

While I was at the University of Tokyo in 1969, Kuno passed away just before receiving the Apollo 11 lunar samples, and I succeeded him to continue the planned lunar sample studies at the University of Tokyo with Y. Nakamura, S. Akimoto, and H. Haramura. Studying lunar samples was exciting and I forgot experimental work for a while. This work opened my eyes to the planetary sciences and later I studied Antarctic chondrites and initiated evaporation and condensation experiments.

In 1971 I again returned to the Geophysical Lab and resumed experimental work on the origin of magmas. While examining the liquidus boundary between olivine and Ca-poor pyroxene in various simple systems, I found an interesting regularity between the extent of shift of this liquidus boundary and the oxide components added to the systems. With this regularity, the compositional changes of melts by addition of H_2O as well as CO_2 could be better understood. This work led me to study the properties of silicate melts in relation to their compositions in the following ten years. During this period, I also worked on partial melting of mantle peridotites at high pressures and genesis of basalt magmas, especially mid-oceanic ridge basalt magma in both synthetic and natural peridotite and basalt systems. In 1974 I returned to the University of Tokyo and started teaching petrology and established the experimental laboratory at the Geological Institute with T. Fujii and H. Fukuyama, and started high-pressure experiments there.

In 1975–1976 I again returned to the Geophysical Lab to continue studies on the properties of silicate melts, and tried to measure the viscosity of silicate melts at high pressures, which at that time were not well known at high pressures. Measuring viscosity at high pressures was, however, quite difficult. The major problem to overcome was to eliminate the temperature gradient within a relatively large charge. After many trials, I was finally able to develop a desirable graphite furnace that significantly reduces the temperature gradient in the capsule. Using this furnace assemblage and very small Pt spheres, I succeeded to measure the viscosity of jadeite melt at pressures to 2.4 GPa. The result was, however, surprising; it decreases with increasing pressure at constant temperature. I first interpreted that this may be due to the structural change of the melt from a framework silicate-like structure to a chain silicate-like structure. This interpretation was, however, disproved by B.O. Mysen and D. Virgo based on their Raman spectroscopic studies, and they proposed a new structural interpretation. Viscosity measurements were made on several other polymerized silicate melts and they also showed a similar behavior. C.M. Scarfe, B.O. Mysen, and D. Virgo, however, found that diopside melt and other depolymerized silicate melts showed the opposite behavior; the viscosity increases with increasing pressure. By these works we could better understand the relation between viscosity and composition of silicate melts at high pressures. The decrease of viscosity of jadeite melt was supported by the molecular dynamics computer simulations carried out by C.A. Angel and his colleagues on the diffusivities of network-forming ions in this melt; the diffusivities of Si, Al, and O are enhanced with increasing pressure to 20 GPa at constant temperature. Their work led N. Shimizu and me to experimentally measure diffusivities of ions in jadeite and diopside melts at high pressures. The results demonstrated a beautiful inverse relation between viscosity and diffusivity of network-forming ions in these melts.

After 1978 I stayed mainly at the University of Tokyo and was engaged in both teaching and research. I was fortunate to have a number of excellent graduate students in petrology, and I enjoyed discussing and doing research with those young people, such as E. Takahashi, M. Sakuyama, A. Tsuchiyama, K. Ozawa, H. Nagahara, Y. Tatsumi, and S. Umino. At that time, my major problem was to better understand the genesis of magmas in island arcs, and we made a number of experiments to determine the conditions of formation of island arc magmas including basalts, boninites and magnesian andesites. Partial melting of mantle peridotite was still my major interest, and we applied the "sandwich method" to eliminate quench problems in determining partial melt compositions, and the results were applied to the genesis of MORB, which was being intensely debated at that time. We also initiated experimental work to understand the condition of formation of chondrules. These studies were all very fruitful, mainly due to the ability and effort of the talented students mentioned above.

When I stayed at the Geophysical Lab in the early 1980s, I initiated with B.O. Mysen vacuum experiments to determine vaporous diagrams for understanding evaporation and condensation processes in the solar nebula; similar to determining the liquidus diagrams for understanding magmatic processes. The experiments were, however, difficult and were not fully successful. More vigorous experiments were later carried out by H. Nagahara and K. Ozawa.

During the period from 1967 to 1987, I moved back and forth between Tokyo and Washington, D.C. In most cases, I tried to find problems by studying natural rocks at the University of Tokyo, and whenever I found time, I returned to the Geophysical Lab to conduct the experiments to solve the problems. This nice arrangement was made possible by the generosity of the Geophysical Lab staff, especially H.S. Yoder, C.T. Prewitt, and B.O. Mysen, and by the positive cooperation of the faculty members of the University of Tokyo. I am very much grateful to those people.

Since 1987 my research subject has been mainly partial melting of mantle peridotites in relation to the genesis of basalt magmas and magmatic processes in subduction zones. I made both experimental and petrological work in these studies. In the early 1990s, K.T. Johnson, who was staying at the University of Tokyo, asked me if there is any way to experimentally simulate a fractional melting process of peridotite at high pressures. I recalled F.R. Boyd's attempt with diamond grains made in 1970s, and I thought that diamond aggregates might be used to simulate the fractional melting process. Then, we made experiments with a charge containing relatively fine-grained aggregates of diamond placed on top of or within peridotite powder. K. Hirose joined us, and we were able to successfully separate partial melts from the peridotite residue into the pore space of diamond aggregates, enabling us to determine the compositions of partial melts without quench problems. E.M. Stolper's group at Caltech independently developed the diamond aggregate method to determine the compositions of relatively low melt fractions. Both their data and our data were quite consistent, which was encouraging.

In 1994 I retired from the University of Tokyo and moved to the Institute for Study of the Earth's Interior, Okayama University at Misasa, where I started experimental studies on the behavior of trace elements in magmas at high pressures, mainly with E. Nakamura. We determined the diffusivities of trace elements in magmatic silicate melts and the partitioning of trace elements between melts an residues formed by partial melting of peridotites at high pressures. I also studied the partitioning of Mg and Fe between partial melts and olivine in the residues with M.J. Walter. While these studies were still under way, I retired from the Institute on March 31, 1999.

During my research career of almost four decades, I enjoyed conducting research at the University of Tokyo, the Geophysical Laboratory and the Institute for Study of the Earth's Interior at Misasa. I was fortunate to have outstanding teachers and colleagues in these three places. Without their collaboration, help, and encouragement, I could not pursue my research. I sincerely appreciate those people.

I thank all of you again for this high honor of the Mineralogical Society of America.