# THE PAST PRESIDENTS' MEDAL FOR 1987 TO PETER L. ROEDER

Mr. President, members of the Mineralogical Association of Canada, and guests:

It is a privilege to perform one of my last Past Presidential duties, the presentation of the Mineralogical Association of Canada's Past Presidents' Medal for 1987 to Peter L. Roeder. Indeed, the presentation is far more agreeable than was the selection process, owing to the large number of worthy candidates.

My own direct experience with Pete has largely been as editor and referee for numerous manuscripts, and it confirms what I am about to relate, which is largely based on documentation provided by Dugald Carmichael, Pete's colleague at Queen's.

Pete Roeder has devoted his research to basalt, which can be regarded as the most common and worthless of the igneous rocks. In so doing, he has ensured that his experimental studies would have the maximum possible geological relevance. Pete's "magnificent experimental data" (so lauded in 1976 by L.L. Perchuk, an eminent Soviet petrologist) place quantitative constraints on the conditions of genesis and crystallization of basaltic rocks throughout all of geological time and space. His data have been applied to rocks as young and fresh as historical flows in Hawaii and Iceland, as old and altered as Archean deep-crustal metagabbro in the Kapuskasing Structure, as exotic as basaltic maria on the moon, as controversial as ultramafic flows in Archean greenstone belts. They have been used on the macro-scale in modeling the genesis of basaltic magma in subduction zones and beneath mid-ocean ridges, and on the micro-scale in testing whether equilibrium has been attained in tiny experimental crucibles. His 1970 paper with Ron Emslie on olivine–liquid equilibrium, containing the best-known of the "saturation surfaces" that are currently so popular in igneous petrology, is still being cited about 50 times per year.

Recognizing that liquidus phase-diagrams for systems of more than three components were becoming intractably complex, while at the same time still providing only a poor approximation to real basalt, Pete pioneered a completely new approach. He and his students experimented with natural basalts in all their chemical complexity, controlling the oxidation potential with mixtures of gases so as to study the equilibria between basaltic magma and crystalline phases, particularly olivine and spinel. Pete is justly renowned for the theoretical rigor of these studies of basaltic systems, as well as for the utter reliability of the experiments carried out in his N.L. Bowen Laboratory. He was also among the first to summarize experimental results in the form of algebraic equations rather than complex geometrical phase-diagrams. He was among the first to use an electron microprobe to analyze the products of experiments, and probably the first to use a computer to calculate paths of magmatic differentiation in multicomponent systems.

In the 1980 Canadian Geoscience Council review of the Geosciences in Canadian Universities, Pete's research was singled out for special praise by Alex MacBirney, one of the well-known scientists who were consulted for their general opinions on the state of their discipline in Canada. McBirney wrote that Pete's ". . . work on the physical properties of silicate melts is simply unique . . . in my mind he is one of the finest scientists in Canada."

His current research interests include development and application of a proton microprobe to mineralogical and petrological problems and what will likely be another of his milestone experimental studies, this time on chromous and chromic ions in basaltic magmas.

President Greenwood, it is my honor and certainly a great pleasure to present Pete Roeder, one of Canada's most highly respected theoretical and experimental petrologists, with the 1987 Mineralogical Association of Canada's Past Presidents' Medal for excellence in research of an international calibre.

Citation prepared and read by Louis J. Cabri
Chairman, Past Presidents' Medal Committee

Ladies and gentlemen,

Thank you very much for those kind words. I would like to acknowledge all those individuals who had some part in this presentation and to thank a few of the people who are responsibe for my being here today.

When I was a teenager, my waking hours were consumed by a passion for the outdoors and, particularly, a passion for fishing. I read about fishing, I spent many hours with a fly rod in hand, and I even kept detailed records of my successes and failures. My parents, Sonja and Kenneth Roeder, watched in despair as their only son showed much more concern about what fly was hatching than a career of any kind, or even an interest in the opposite sex. Luckily for me they paid no attention to my stated intention to fish for the rest of my life, and they sent me to Tufts University. I started to major in political studies and was required to take a science course.

PETER L. ROEDER

I chose geology because of my interest in the outdoors. Professor Robert L. Nichols, a well-known glacial geologist and explorer, provided the inspiration and enthusiasm that started my career in the earth sciences. After completion of a B.Sc. in Geology with a minor in Political Studies, I was drafted into the Army for two years. This gave me the opportunity to read and think about what I wanted to do with my life. During this period I read The Evolution of the Igneous Rocks by N.L. Bowen, and was launched in experimental petrology. I was also inspired by an article in Scientific American on The Origin of Granite by O.F. Tuttle, and this led me to graduate work at The Pennsylvania State University. At Penn State, I was taught and inspired by people like Frank Tuttle, Arnulf Muan, Dick Jahns, Rustum Roy, and, particularly, my thesis advisor, E.F. Osborn. Ossie and Frank Tuttle laid a foundation in the application of phase equilibria to petrological problems that still serves me well and, I believe, has served my students very well. My fellow graduate students at Penn State, particularly Bob Fudali and Fred Glasser, had a great part in helping me appreciate how science can actually be done.

After completing my Ph.D., Fred Kuellmer very kindly provided an environment at the New Mexico Institute of Mining and Technology where I could complete the study with Osborn on the role of oxygen fugacity in the crystallization of melts in a simplified basalt system.

My first real job, which may be my last, was at Queen's University, where I was kindly welcomed by Willis Ambrose, Len Berry and Hugh Wynne-Edwards, who, together with the other staff, made me feel that I had arrived home. The early years at Queen's were particularly productive for me. In part, this was because of the very enlightened attitude of the University administration and the National Research Council of Canada, who provided my research funds. Unlike many present-day granting agencies and university administrations, they did not expect an immediate return; they seemed to know that good research is not the product of a ticking clock.

I was fortunate that Ron Emslie came to Queen's for a year as a sabbatical replacement, and we could start our fruitful collaboration on the equilibrium of olivine and basaltic liquid. Although my roots are planted in the laboratory, I have needed considerable nourishment from the field, which has been provided by people like Ron and my good friend Tony Mariano, who showed me quite different ways to look at minerals. I particularly wish to thank my students, that include Tony Naldrett, Fred Campbell, Robin Hill, Dave Haughton, and Heather Jamieson, who dared to ask why when I wasn't willing to acknowledge there was a question.

Of course, none of this would have been possible without my dear wife Claire and my three children, David, Kathy and Tina, who continually reminded me what was really important in life.

I look around this room filled with mineralogists and I feel very comfortable, but this hasn't always been the case. As an experimentalist, I sometimes felt that many mineralogists viewed my kind with suspicion. It was almost as though they were a little afraid that I might plunk one of their favourite single crystals into my furnace at 1400°C and, after a fantastic quench, out would come an amorphous glob of glass. Now to an experimental petrologist, nothing is prettier than a well-quenched glass, but to a mineralogist nothing is more abhorrent than an amorphous glob. I also found that when I first became an insecure professional, I would trot out the old trusty Gibbs Phase Rule at any sign of trouble. Most mineralogists seemed a little leary of this maneuver, as though they expected that I might pronounce that their favorite mineral did not exist because it should not. I have to confess that when Hugh Greenwood called to let me know this award, I wondered whether the Past Presidents of this august group had lost their mineralogical way or whether this was a sign that the mineralogical community may have finally made me into a reasonably competent mineralogist. Be that as it may, I am very happy to be here and thank you for your patience, your kindness, and your faith in me.

Peter L. Roeder