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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1997

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On reading last year's Roebling medalist's (Don Lindsley) acceptance speech came the realization of how influenced I was by his classic paper on the iron-titanium oxide thermometer and oxybarometer. From my earliest days as a student I was fascinated with obsidians and pitchstones but it wasn't until 1964, when his paper was published, that the means to get their temperatures of phenocryst equilibration (the natural liquidus) became available, and I scoured the western United States for silicic lavas to separate their sparse Fe-Ti oxide phenocrysts. Here began my interest in oxygen fugacity, a topic that runs in and out of my research interests.

I had inherited from my father, a Scottish neurologist employed by the Medical Research Council at one of the London teaching hospitals, his view that medicine was a golden realm of human nature at its best, that in its practice, science required both inspiration and perspiration, and that in the workings of nature and the mind, the abnormal is as important as the common; in true puritan belief, following fashion was always to be eschewed, but setting it was another matter!

From this background, reinforced by years at boarding school, arose my fascination with places far off the beaten track and with unusual rocks. As a graduate student at the University of London in the 1950s, I worked in Iceland, far from the focus of contemporary topical interest, before the days of the electron microprobe or of XRF machines, in a period when dating rocks was in its infancy. Crystal fractionation was in full force as the reigning cause of igneous differentiation, and oxygen fugacity had become a significant consideration in affecting the hypothetical iron-rich liquid trend of the Skaergaard intrusion. Thus it became a plausible conclusion that the Tertiary lavas of Thingmuli in eastern Iceland, ranging from olivine-tholeiites, through iron-rich intermediates to spectacular obsidians, were the volcanic complement to the layered gabbros of the Skaergaard.

As an undergraduate at Cambridge University, I was brought up on the two- or three-component phase diagrams made famous by the Geophysical Lab and Penn State, but there always seemed to be an unconvincing extrapolation to the compositions and phase assemblages of natural liquids. Later in Iceland I became fascinated with glassy lavas and dykes, and I saw in these the equivalent of quenched equilibria of the experimental labs. However, even for glassy silicic lavas that were close to the simple ternary system Ab-Or-Qz, the only way to tackle the complexities of feldspar crystallization in nature was the tedious separation and wet chemical analysis of the glass and phenocryst assemblages of pitchstones and obsidians; without an electron microscope, mineral separation and microanalysis occupied me for years at the Imperial College of Science.

After going to the University of Chicago on sabbatical in 1964, I was offered a chance to visit Berkeley as a post-doc for a year, which became a permanent move on the retirement of Howel Williams. Here was a golden opportunity to explore volcanism in the western United States, although the Cascades were the ''territory'' of the U.S. Geological Survey and the University of Oregon, and my single excursion there with Alan Smith was not to be repeated for 30 years. The unusual minette lavas of the Navajo-Hopi area, the alkali-rich magmas of the Raton area, and the carbonatites of Iron Hill, Colorado, became the research areas of Jim Nicholls, Jay Stormer, and Bill Nash, and from their studies came the first explorations into writing reactions for igneous rocks (the beginnings of silica activity) and using simple models to predict halogen behavior in magmas. Other students (Gary Lowder, Bob Heming, Bruce Marsh, Frank Brown) went further afield to New Guinea, the Aleutians, and to east Africa, but the logistic problems became difficult, and I wanted field areas that we could drive to.

There was something very provocative about the vast ashflows that were becoming commonly recognized in the western United States, and it seemed as if these could be plutons displayed eruptively. So Wes Hildreth went to the Bishop tuff of Mono basin, but as NSF reviewers asserted that this rock-body would not reveal the pre-eruptive magmatic regime, he was funded on another grant. Parenthetically it would be of interest to know how many investigations on the Bishop tuff have subsequently been funded by NSF as a result of Wes's work. Another student, Frank Spera, went to the nodule-bearing lavas of the Grand Canyon and so started his interest in magma dynamics, an expanding and exciting field pioneered by Bruce Marsh, and now added to by the experiments of Don Snyder seeking to comprehend his observations on layered intrusions in Labrador.

In his turn, Steve Nelson took a much battered university vehicle to Ceboruco volcano in western Mexico, and so started my fascination of more than 25 years with the tectonic and igneous problems of the Mexican volcanic belt. Far from being the geologically unrecognized area of the 1960s and 1970s (witness the fact that Paricutin, a cinder cone erupting intermittently for nine years was better known then than Colima, a 13,000 ft volcano and probably North America's most active cone), Mexico has

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now attracted the international community of earth scientists; in a word, it has become fashionable! Here in a rather small area lies more petrological diversity among the volcanic racks than anywhere else on earth, with lavas of oceanic-island type being erupted contemporaneously side by side to types of a typical subduction environment. And all these with a close association to extensional and strike-slip faulting on a converging plate margin called the Jalisco block. Here the researches of Jim Luhr, Gail Mahood, Becky Lange, Jamie Allen, Toshi Hasenaka, Paul Wallace, Kevin Righter, Gordon Moore, and more than 40 undergraduate field assistants have unraveled the sequence, age, style, and substance of volcanism.

Fieldwork by itself has always posed problems that may take considerable lab investigation to resolve, and therein lies the great benefit of field geology; a continuous supply of the unknown and the unexpected. To illuminate these, experiments were traditionally designed to illustrate the equilibration conditions of the various mineral assemblages, but there had to be another way to constrain these other than by experimentally reproducing the assemblages in *P*-*T* space for almost every common igneous rock. Metamorphic petrologists had for years been able to calculate the position of phase boundaries using thermodynamic data on minerals and gases, and igneous petrologists had to bite the bullet and follow their lead. What was needed was a solution model for the nine-component silicate liquid, a simple enough concept in comparison to aqueous electrolytes, but there was a dearth of thermodynamic data and some confusion on the thermodynamic difference between glass and liquid. Just about the most simple model other than an ideal liquid is a regular solution, first advocated as being useful for magmas by Dan Weill, and developed to the full by Mark Ghiorso. But there were so few thermodynamic data on simple or natural silicate liquids for incorporation into any solution model, that we (Charlie Bacon, Jon Stebbins, Mark Rivers, Becky Lange, Victor Kress, Quentin Williams, Dan Stein, Don Snyder) became involved with measuring enthalpies, heat capacities, densities, thermal expansions, compressibilities, and thermal conductivities on anhydrous multi-component silicate liquids covering the natural range. Later we (Victor Kress, Gordon Moore) explored the connection between ferric and ferrous iron dissolved in a silicate liquid and the liquid's composition, temperature, and oxygen fugacity, including water. Throughout this experimental focus on silicate liquids I was fortunate to attract several post-docs (Richard Sack, Jim Murdoch, Dave Shirley, Glen Mattioli, and presently Jenni Barclay) who all contributed to quantifying some aspect of liquids relevant to magmas.

In hindsight, one of the most important features for modeling these properties turned out to be the precise composition of the liquids, often synthetic mixtures high in alkalis and not conveniently measured on the electron probe; wet chemistry thus came back into its own with its very high precision and provided me endless hours of manual satisfaction. If there is a sense of craft in being a scientist, performing wet chemical analysis surely satisfies that.

Earth science has had its prevailing fashions that always seemed to have passed me by; studying the origin of granite, the most contentious issue of my student days; rare-earth technology (I could never appreciate how 0.01% of the rock could reveal the origin of the remaining 99.99%!); instrumental analysis and forsaking wet chemistry; lunar petrology; and MORB evolution, although I did make one salutary excursion into mantle nodules with Kurt Kyser. Lately I have moved to the less well known but exciting area of central Mexico with Dawnika Blatter and to northeast California, where east of the Shasta-Medicine Lake region is an area that I hope will not attract the interest of legions from the U.S. Geological Survey or the universities. Here with the help of Paul Renne in argon dating, I hope to find the magmatic response to the intersection of two quite different tectonic regimes, a parallel to those displayed in western Mexico.

In retrospect, I believe that the advice given me by John Verhoogen, a sagacious and erudite scientist, to embrace change, is as important in life as it is in research. I count myself fortunate to have been at Berkeley for more than three decades when funded by thoughtful program managers at NSF (John Snyder) or DOE (George Kolstad) and participating in a sensible and perceptive campus administration on graduate matters and research (Sandy Elberg and Joe Cerny), we could do all manner of experiments on silicate liquids, we could go anywhere to collect and map volcanic rock, we had the analytical resources to support both types of research, and last, but really first, where some 35 Ph.D. students of incomparable quality chose to become enmeshed in my life.

I wish I could have been in Salt Lake City to thank you, Mr. President and the Mineralogical Society of America, for this great honor. The evening of a professional career is never an easy prospect to face, particularly after bypass surgery, but the award of the Roebling medal will surely help me retain the immense satisfaction that I have always had from looking at rocks in the field or in the lab.