

Acceptance of the Mineralogical Society of America Award for 1984

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President Prewitt, Ladies and Gentlemen:

I am delighted to accept the MSA Award for 1984 and I thank the Society for the honor it bestows on me. After reading many of the erudite speeches of previous recipients I must immediately declare my inability to compete at their level. I've decided that all I can do is resort to a brief personal history.

I was brought up in London and taught to prize the virtues of the English grammar school. As I recall, these were emphases on Latin, fair play, and a stiff upper lip. Science was gaining ground among those incapable of understanding the ablative absolute, but it was not regarded as essential to a good education. I developed a vague interest in chemistry tempered by a passion for the games we were busy "fair-playing." When the time came to think about a career I postponed the decision by going to a small local college (the Northern Polytechnic) and taking a degree in chemistry and, as an afterthought, geology. This was a lucky choice because it brought me in contact with a number of excellent teachers and provided me with the impetus to seek an academic career.

At the suggestion of Peter Harris (of Leeds) I went to graduate school in Newcastle to work with the Late Roger Strens. Roger had no teaching duties and I was his only student so we spent most of our time working together. Since, however, our laboratory was completely without equipment this work involved discussions roaming over wide areas of science, politics and the stock market. We also designed the kinds of experiments we would do if we should ever get any equipment. Roger emphasized the importance of physics and chemistry in mineralogy and forced me to learn and relearn whole areas of solid state physics, spectroscopy and thermodynamics. This education has stood me in great stead in the intervening thirteen years. We tried modeling the crystal field spectra of Fe^{2+} -minerals and made some crude attempts to use our results to predict the stability fields of Fe–Mg solid solutions. I decided that real experiments would be more fun than the computer variety and looked for a post-doc where I could integrate my interest in thermodynamics with some high pressure experimentation. Manchester seemed to be the right place.

My arrival in Manchester coincided with that of Shohei Banno, a visiting faculty member. We found that we were both interested in aluminous orthopyroxenes, I because I was trying to use experiments on them to test simple solution models and he because he was trying to apply the experimental data to garnet lherzolites. He helped me un-



learn my poor thermodynamics and we set about collaborating on a paper.

MacKenzie entered my office one afternoon and announced that I was going to Berkeley for two quarters to teach metamorphic petrology. I pointed out that I knew nothing about metamorphism but he told me that I had a couple of months to read up on it and anyway, "Americans will be much more impressed by your accent than by the content of your lectures." So I went.

My first lasting impression of Berkeley after an 11-hour plane ride is the sight of a very large man pouring me a very large scotch from the biggest bottle of whiskey I had ever seen. He refilled my glass and interrogated me about entropy. I think I did all right that night and for the rest of my 6 months around Ian Carmichael. I quickly learned about bumptious graduate students. I had an adjoining office to one of the more vocal ones, a fellow by the name of Bruce Marsh. We argued and scrawled obscene comments on one another's work and I had to lock him out of my office whenever I wanted to think. I entered Harold Helgeson's lab rather timidly but was treated to a series of stimulating inquisitions. The parties were great, too. I loved all of it. I went back to England believing that Berkeley was a typical American town and that I'd stumbled on some great ideas for experiments.

I spent the succeeding period (1973–1979) as a faculty member at Manchester trying to put the ideas on solid solution thermodynamics into practice. This was a very

happy and productive period for me and I should like to thank all of my former colleagues for the support and encouragement that they gave in abundance. I should also acknowledge Don Fraser of Oxford who convinced me to collaborate on a textbook and who sold the idea to a respectable publisher.

After reading Bob Newton's and Ole Kleppa's papers on high temperature calorimetry I was convinced that this technique provided a means further to constrain solid solution properties and I went off to Chicago for a year to work with them. Apart from surviving the worst ever Chicago winter, I learned all the reasons why every calorimetry lab should be built next to a bar.

In 1981 my brief, misguided attempt to sort out the scientific problems associated with nuclear waste disposal

ended at the instigation of a rather unlikely knight in shining armor, John Holloway. He talked me into easing myself back into academic life by spending a really enjoyable year at Arizona State. From there I moved to Northwestern where I find my new colleagues tolerant of my eccentricities and capable of enjoying good science and a good joke. I thank them in advance for their future forbearance.

The science of mineralogy has advanced dramatically in both theory and instrumentation since I began graduate school. It will continue to advance at comparable pace whether I contribute to it or not. I sincerely hope, however, that I can be a small part of its future and that I can bring more luster to the MSA Award. If wishing it and working at it doesn't do the trick, then it can't be done. Thank you.