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Acceptance of the Dana Medal fo the Mineralogical Society of America for 2003

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Thank you, President Carpenter and the members of the Mineralogical Society of America, and my thanks to Ian for his kind and generous introduction.

I am deeply honored to be designated a recipient of the Dana Medal of the Mineralogical Society of America. The award is named in respect of the accomplishments of two of the most distinguished American scientists of the nineteenth century: James Dwight Dana, a naturalist of extraordinary observational perception and arguably the first scientist to study volcanic eruptions, and his son, Edward Salsbury Dana, who with his father systematized the study of minerals and effectively invented the science of mineralogy. To be mentioned in conjunction with these two legendary scientists is profoundly humbling, as it is to associate my name with previous recipients of this award.

But fortunately for me the Mineralogical Society of America has designed this award with an escape clause. The Dana Medal is a mid-career award, given both in recognition and in expectation. Infamous deeds of the past notwithstanding, the award of the Dana Medal raises the bar, points squarely toward the future, and demands an answer to the question “So, what are you going to do next?” For the challenge of continued accomplishment, I am more grateful to the Society for conferring this honor upon me than for any (perhaps misguided) assessment of my previous work.

The kinds of scientific questions that interest me all have a central theme, that being the quantitative evaluation of chemical reactions that drive differentiation in the Earth. I make thermodynamic models. Yes, you heard correctly; I make models. I admit it here before a higher power, and I’m proud of it.

In the not too distant past, geochemists and petrologists who spent most of their time constructing models were ostracized by their peers. Sometimes, with very good reason. Not so today, and I think rightly so. And, for a moment let me take advantage of the bully pulpit and tell you why I hold that view.

Models synthesize diverse data and provide a means of extrapolating these data to physical conditions inaccessible by direct observation or experiment. Models allow us to evaluate the internal consistency of the totality of our observations. This means they keep us from cheating–from considering only certain data in the absence of others, and especially so if we demand that our models have as ground truth observations made upon the rock and experimental record.

As Earth scientists we live now in an age of models. Some good, some bad. Some comprehensive, some trivial. Some useful, some useless. But as a practitioner let me tell you that there is an enormous responsibility associated with unleashing models upon the unsuspecting and eager scientific community. Models in the Earth sciences must address a real problem, not an abstraction. We are not chemists, we are not physicists, and we are not materials scientists. We study the Earth and we should be accountable to it. Our models should be testable against the rock record. In addition, the models we construct must be extensible. They must augment and not replace observation and experimentation. And, above all, they must be useful to and *useable by* the community of scientists for which they were devised. It is not sufficient to construct a tool that allows the solution of a scientific problem if that tool is not made available to the community who needs it. And, as model construction becomes ever more time consuming in the attempt to address more and more comprehensive issues in the Earth sciences, the need to meet the responsibility of extensibility and usability should be the primary conviction of the modeler. It is mine.

I’ve come to realize recently that I have developed this conviction about modeling and model development primarily as a result of studying and working with four individuals over the past two plus decades. I’ve come to think of these four as

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mentors who, each in a different way, have shaped my approach to Earth science.

I met the first two as a student at U.C. Berkeley. They are Hal Helgeson and Ian Carmichael.

Now, Berkeley was an amazing place to be in the mid- to late seventies. John Verhoogen was still teaching, Frank Turner and Howel Williams walked the halls, and fellow students like Mark Rivers, Jon Stebbins, Frank Spera, Wes Hildreth, Jim Luhr, and Charlie Bacon, just to name a few, generated a sparkling intellectual environment. But the two that really made it for me were Helgeson and Carmichael.

Helgeson had a profound influence over me. First of all, he taught me that there is nothing scary about thermodynamics. Second, he taught me that it is okay to work on hard complex problems that take a long time to solve. And third, he taught me that models must be comprehensive and rooted strongly in theory if they are to be useful and long lasting. Helgeson established by example a *modus operandi* for research that is now simply part of the way I do things.

Ian Carmichael instilled into me his own great desire to understand the physio-chemical principles that govern the diversity of the igneous rock record. Ian's view is that this problem can be understood in a deterministic way if one can characterize the physical properties and conditions of magma generation and transport. From Ian I learned that imagination and intuition are more important than knowledge, that thermodynamics can be used cleverly and usefully in the absence of data, and that nature is a lot better at doing experiments than most people give her credit. From Ian I also learned a great truth of working in science, that when whatever it is you are working on becomes popular enough to be fundable by NSF, it is probably time to move on to something more novel. The corollary of this is, never jump on the bandwagon. Be the bandwagon.

After leaving Berkeley I had the good fortune to meet and begin to collaborate with what would become my third mentor, Richard Sack.

Richard Sack taught me about minerals. Together, we accomplished a series of studies on the thermodynamics of the rock-forming minerals that allowed all subsequent work on the modeling of mass transfer in magmatic systems to be undertaken. Without Richard's insight and abilities, I would certainly not be standing here today and none of you out there would have MELTS to use. Besides teaching me about that "East Coast School's" approach to the study of mineral thermochemistry, Richard taught me what making thermodynamic models was really about. Somewhere around the time of our work on multicomponent spinels and pyroxenes, Richard started to say that we were engaged in one of the Labors of Hercules. Now, the Labor he had in mind was not the capture of the Cretan Bull, nor the defeat of Cerberus, or not even the subjugation of the Amazons. No, Richard meant that we were engaged in an activity equivalent to the cleansing of the stables of King Augeas. For those that don't know the story, Hercules was commanded by King Eurystheus to remove thirty years' worth of the accumulated excrement of thousands of cows from the Augean stables in the course of a single day. Richard's point is that making thermodynamic models is like cleaning up the stables: nobody wants to do it, but everybody knows it is long overdue and has to be done, and, when it's done, everybody has a nice clean new place to play–for a while.

When I came to the University of Washington I met my fourth mentor, Bernard Evans. Bernard above all others taught me what it is to love science, and to do so with wild and jubilant abandon. For Bernard, understanding a mineralogical problem is equivalent to having models that describe every nuance of the data. From Bernard I learned that there should be an expectation of perfection and a disappointment at compromise. A hard objective that is not always realizable, but one that holds absolute the notion that if you are going to make models that describe the rock record, they must do so at the precision of the observables. Bernard also taught me that, like a thermodynamic model, a microprobe analysis is never good enough. Never. And, that is just the way it is.

Before I close, I would also like to thank a few more of the extraordinary colleagues that I have had the pleasure of working with over the past 20 years. In particular, two of my former students, Peter Kelemen and Marc Hirschmann, have helped shape in no small way the course of my research on modeling magmatic phase equilibria. Their insights and contributions will be clearly seen in the lecture that I will present later on this afternoon. A similar debt of gratitude goes to my colleagues Victor Kress and Paul Asimow. Together these two have tackled difficult problems with grace and ease and have provided me with a sense of camaraderie and support during our joint efforts to model magmatic phenomena in nature. Lastly, I would like to acknowledge the support of Ed Stolper, who more than anyone realized the potential and application of my work to modeling melt production at pressure, and persistently made this clear to an at first skeptical and later enthusiastic scientific community.

Mr. President, I am truly grateful to be this year's recipient of the Dana Medal of the Mineralogical Society of America. My hope is that my future contributions will justify the Society's selection and will serve to honor the memory of the distinguished namesakes of this award.