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# MINERALOGICAL MUSING[1]

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My text for today is taken from the book of Kelvin, an address by Bowen, and a few hopefully well-concealed bits plagiarized from my colleagues. I have been tempted to talk about a variety of things including the beauty of minerals and rocks, thus the title. Several of my friends have expressed displeasure with the thought of having to listen to me give a "scientific" address, and I understand their feeling and envy their frankness. My subject could have been, "Man was not meant to make microcline", but I have talked around that one before. A more modern theme would be "The new global mineralogy", and why not try that? A few words might also be said about some educational matters in the areas of our speciality, but discussions on education rarely seems to get anywhere. When I was a young man, papers that sounded like what I propose to talk about really put me off!

The reference to Bowen is his Presidential Address before this society in 1937, entitled "Mente et Malleo Atque Catino." It was a plea for cooperation between field and laboratory investigation in mineralogy and petrology, and more particularly a plea for more active experimentation; mind and hammer—and crucible. At this time Bowen had just set up a laboratory for experimental petrology at The University of Chicago, modeled after the Geophysical Laboratory. This high-temperature laboratory, and one but sporadically used at the University of Michigan, were the only quantitative petrological laboratories in any university in the country! It was a sign of the times, only 34 years ago, that the effect of pressure on reactions

[1] Presidential address to the Mineralogical Society of America, 2 November 1970.

is only briefly mentioned by Bowen in passing, although recognized it surely is. Rather little work at elevated pressure was being done, aside from the pioneering work of Percy Bridgman, although the first experiment done in what would now be called a piston-cylinder device was performed by C. A. Parsons on carbon and published in 1888. There are two good reasons why just a few years ago there was little emphasis placed on high-pressure research in mineralogy and petrology; first, many problems relating to the surface or the crust were at hand and unsolved (many still are), and secondly, high pressure experimentation has not been either easy or cheap. Any science must deal with the reasonably tractable or it becomes metaphysics. Classical geology and mineralogy have of course dealt with the accessible portions of the earth, and although speculation about its internal constitution is as old as geology, theory and analysis of the deep earth has for the most part been the bailiwick of the geophysicist.

Mineralogy, and especially crystallography is usually thought of as a three-dimensional subject, yet almost all of our experience with minerals is restricted to a nearly two-dimensional situation—the earth's surface. My favorite approach to mineralogy is through aesthetics; I think the best thing about many minerals is the way they look. The recent general appreciation of the intrinsic beauty of natural objects, including minerals and rocks, is expressed in the price of good specimens. But even here beauty is skin deep for we are dealing with crustal and commonly surficial material. One can be sure that in deep-seated rocks at least part of the beauty—that expressed by natural external forms—is missing. Another thing one can be fairly sure of is that the enormous variability and wide range of compositions (we have already excluded forms) and thus the number of mineral species will be severely restricted at depth. The varied and extraordinarily complex outer layers of the earth, as well as the extraordinary number and variability of compounds result from great variability of the environment in which they are formed. The surface and near-surface of the earth has extreme gradients of all types; composition, temperature, and pressure. Extremes are further accentuated at interfaces, such as those between solid, liquid, and gas, and weathering is one of the great differentiators of nature. In this case "extreme" pressure gradients are measured in millibars, but these gradients of millibars over distances of hundreds of kilometers drive the atmosphere. The inherently low temperatures at or near the surface add an additional measure of variability; the result of slow reaction and perhaps not uncommon failure of equilibrium leading to a range of states. Further beautification due to both shaping and patination is also the result of surface action.

Aside from these considerations the substance of the deep earth cannot be beautiful. Even if beauty lies only in the eye of the beholder, can the unseen be rated aesthetically? Perhaps in the mind's eye. It is also tempting to argue for a rather simple mineralogy at great depths. The ultimate would be the molten core, presumably a single phase. Thus a large volume of the earth is essentially monomineralic. We know little about compositional gradients at depth, but tend to assume a fairly homogeneous chemistry over significant distances in the mantle, at least transversly. Now consider the pressure gradient. If we take the pressure at the core to be $3.5 \times 10^6$ bars, and the effective radius over which this pressure operates to be 6,000 kilometers, we get an effective deep-earth gradient of approximately 5 millibars per centimeter. This would produce one hell of a wind if the deep earth were a gas. It is so far from a gas, however, that this gradient would be completely ineffective in inducing variability in any reasonably small distance. One must think in terms of the size of crystalline units in order to consider mineralogical variability, and thus must ask how large are the crystals at depth? Orowan (1967), on the basis of damping, creep, and the relations of internal friction and boundary viscosity considers the grain size in the deep mantle to be the order of meters or kilometers, rather than millimeters or centimeters. If this be the scale of things, the earth's temperature gradient would also be ineffective in promoting variability. This is not to say that these gradients play no role in deep, slow convective or other motions, for in fact the large scale differentiation of the earth must have been driven by just these gradients.

I am now musing on what could be called "global mineralogy." Global in the sense of the entire spheroid and not just its skin. The model of the new global tectonics has not only unified many concepts, but as aptly stated in the first report of the Geodynamics Committee of the National Academy of Sciences, it has done much more:

> "It would be difficult to overstate the success of these ideas in bringing together the different disciplines which constitute the earth sciences."

Mineralogy and petrology are at the very core of geodynamics. How in fact can we really understand how the earth moves and works internally unless we understand what it is really made of at depth, and how and by what mechanisms all that stuff reacts to the forces imposed upon it. To be sure, some very fine scientists have done a significant amount of top-flight research, both theoretical and experimental, aimed at understanding deep-earth materials and processes, but much of that work has been of a pioneering nature, and to turn a phrase, has hardly scratched the surface of the earth's depths. In the past rather few mineralogists have been concerned with the mantle, and not very many have worked with such things as pressure-induced phase changes, or in the field of high-pressure research in general. I will not be so presumptive as to make a plea for more work in the field of very high pressures; there is little doubt that it will come of its own volition. It must, if only because mineralogy and petrology cannot remain distinct and separate from physics and chemistry. Permit me to work over that point for a bit, and here is where Lord Kelvin comes in.

Just one hundred years ago, Kelvin (Sir William Thomson, who would be known in Chicago as Big Bill Thomson) gave his Presidential Address to the British Association (at Edinburgh, 1871). I think it is safe to say that Kelvin was the first champion of the cause of quantitative geology. In addition, he was truly a deep-earth thinker, unlike many of those who came later. If you didn't know it, Kelvin was president of the Geological Society of Glasgow for 21 years. Much of that 1871 address, and in fact a great deal of Kelvin's writings in general are not only charming reading, but of interest to many of this company. At that time a number of giants of science were on the scene in Great Britain—Kelvin, Clerk Maxwell, Lyell, Tyndall, and Charles Darwin to drop a few names, yet there were essentially no real research laboratories. The first physical laboratories were in Glasgow (Kelvin), Edinburgh, and Owens College in Manchester, but they had essentially no budget or staff. Oxford had just established a physical laboratory, and Cambridge was about to construct one "under the eye of Professor Clerk Maxwell" (The Cavendish). Kelvin, in *his* plea for experimental research quotes an "informant" who told him that in Germany,

> "Professors, preceptors, and teachers of secondary schools are engaged on account of their skillfulness in *teaching,* but professors of universities are never engaged unless they have already proved, *by their own investigations,* that they are to be relied upon for the *advancement of science.* Therefore every shilling spent for instruction in universities is at the same time profitable to the advancement of science."

A few years later Kelvin presented a paper before the Geological Society of Glasgow entitled, "The internal condition of the earth; as to temperature, fluidity, and rigidity." He shows, from several considerations including tidal (rigidity), and an elegant argument in which he deduces a non-linear *P-T* gradient, that, . . .

> "We are forced to the conclusion that the earth is not a mere thin shell filled with fluid, but is on the whole or in great part solid."

He also analyzed the behavior of a cooling molten body, and pointed out the likely instability of solid over liquid. The following quotation from this paper is of interest—at least to me:

> "Let us now consider a red-hot liquid globe, and suppose it to cool as does lava. The result of the cooling is that as soon as any portion of the matter reaches a temperature below the melting point, it freezes and becomes solid. Instead of molten lava it becomes solid rock. And now comes the great question—will the solid rock thus formed sink or float? I wish this question could be answered. We have at present very little information on the subject—none of a definite kind, as far as I know, except that given us by the experiments of Bischoff. Bischoff, experimenting on trachite, granite, and basalt, found the solids in these three cases about 10 percent denser than the liquids.
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> "This is a subject on which we ought to have better information, and I do think that physicists are at fault for not having given information regarding it to geologists. Speaking, on the other hand, in the capacity in which you do me the honour to place me, we, the geologists are at fault for not having demanded of the physicists experiments on this subject. This time last year I urged that someone should take up the subject. I now renew the suggestion. We have a great government fund for scientific inquiry. Why do not physicists and geologists unite in an inquiry like this, and apply for assistance out of the Government Grant fund? They should then repeat Bischoff's experiments and test his results, but on a far larger scale, and extend the investigation to many materials besides those which he used."

In Kelvin's 1871 address he quotes from Sabine's (1838) "Report on terrestrial magnetism." The report concludes with the following sentence:

> "Viewed in itself and its various relations, the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit; and we may feel quite assured that the completion of our knowledge of its distribution on the surface of the earth would be regarded by our contemporaries and by posterity as a fitting enterprise of a maritime people, and a worthy achievement of a nation which has ever sought to rank foremost in every arduous and honourable undertaking."

Does this sound strangely modern?

What's good enough for Lord Kelvin is certainly good enough for me, and even if 100 years late, I applaud his science, endorse his views on commonality in the sciences and desire for cooperation between scientists, and recommend the slant he takes toward research and education. It's a solid comfort when pouring out one's wisdom in a Presidential Address to have someone like Lord Kelvin to lean back on. A few minutes ago I stated that mineralogy and petrology cannot remain distinct from physics and chemistry. In the deep global sense they cannot, for the commonality of the sciences (or if you prefer, a real cooperation among scientists) becomes ever more necessary as we peer into the great depths. The idea that the sciences of the earth and earth substances are distinct from those sciences that deal with all other matter or processes has always seemed an anomaly to me, and the concept of a special application of physics and chemistry to geology is a redundancy. Certainly, as our understanding of natural phenomena increases, it must become more general in the sense that a deeper understanding comes from broader areas of science, and not from increased isolation or specialization. This becomes even more apparent if we consider the obvious fact that a deep look takes one away from the classical in mineralogy and petrology, and away from the familiar field approach to these "disciplines". The field observations can no longer be reconciled with the experimental work in the sense that Bowen would have it, for the simple reason that the "field" no longer exists—or more properly, it is inaccessible. I have been accused in the past of being not very friendly toward field geology. This is simply not true, some of my best friends are field men, and I am not about to denigrate that necessary and all important look at our planet. Indeed, as time goes by, and we continue to misuse and make refuse out of our precious resources, the know-how of the crustal scientist becomes all the more important. Nevertheless a new era has been dawning for the extreme pressure experimentalist, and he cannot be the cohort of the global dermatologists, but the theorist and geophysicist and experimentalist are all becoming each others handmaidens. The new global tectonics *is* bringing people together, and *has* cut across specializations, and I suspect that even if the concept is wrong, and the whole picture of sea-floor spreading, subduction, and its associated serindipity disappears down its own trench, much good will have been accomplished, and there will still be a residue of great ideas and good will drifting among disparate groups of earth scientists. I hope I do not sound, at least too obviously, like one jumping on a running board of the bandwagon called new blobal tectonics. The state of matter at great depth is surely at the heart of mineralogy and petrology, and if it appears to be an overstatement to say that mineralogy drives the world, it is true that the world, like an army, runs on its innards—those unseen minerals.

I have always shuddered at the term "inner space", referring to ocean depths, and usually discussed in the press in conjunction with the need for exploration of the seas. This exploration has been touted as particularly important by virtue of being significantly cheaper than sending men out of the earth's gravitational field, and specifically, to the moon. It is also associated by many, including Congressmen, with some valuable, perhaps mysterious, sea-bottom ore, or other recoverable substance. It is too bad that all of the science supported from governmental sources (and that is almost all of it) is done so with a pay-off in mind. Even that fraction ticketed for "basic research" has a practical, financial, or life-saving ultimate goal. Don't ever believe that research on any aspect of the earth would ever be financed unless there was some hope, in the legislative mind, that a practical use would come out of it. I will never forget the way this fact was clarified for me. I was testifying before the Senate Appropriations Committee, as a member of the National Science Board. The annual appropriation of the National Science Foundation was at issue, and the ill-fated and unfortunately named Project Mohole was the sore point. One of the Senators jabbed his finger at us, and in what I interpreted to be an angry and certainly a most forceful tone said, "Don't you *ever* attempt to justify the spending of even one cent of Federal money to satisfy your or anyone else's curiosity!" Funding for high-pressure research will increase, I believe, and for reasons not too difficult to rationalize, but not because anyone in Congress is curious about the inner workings of the earth.

We will learn much more about our planet, and one of the advantages of growing older is the perspective one gains as to how rapidly knowledge accrues and our views change. For many years the earth was thought of as essentially a static body. Of course obvious things like volcanoes and faults were well known (if little understood), but were considered to be more or less local features of activity sprinkled over a stolid earth. This was the result of the dogma, at least in this country, of the permanency of continents and ocean basins; these features were considered to have been "set" early in earth history and essentially frozen ever since. Kelvin, as an outgrowth of his calculation of the age of the earth had a running fight with the uniformitarian view of geological changes as expressed by Hutton and Playfair; no sign of a beginning, no prospect of an end. Geologists held this view even with the knowledge that there were long period changes long studied by geologists—episodes of mountain building (but what made the mountains?)—changes in sea level or of continental extent—global changes in climate or temperature patterns as seen in variations in the sediments, fossils, and ice sheets, for example. Yet the ocean basins and continents remained fixed. Why? Was it because, especially with respect to the oceans, we *need* to preserve an area of the unknown? A deep, dark, unexplored region that hides a variety of mysteries, acts as a reservoir for uncertainties, and could be used as a bin for unresolved and "unresolvable" problems? Is there a psychological need for a dark hidden region, perhaps a replacement for the bedcovers to hide under? If so, I hope we can overcome this need, at least with respect to the deep earth.

I threatened earlier to say a few words about education, although in a sense I have already done so. In making this threat good I will say very little, and try to restrict the few words to education relevant to whatever you may think I have already said. For many years I have been a champion of the concept of the unity of education and research. One can substitute science for research, and thus I believe that the separation of education (teaching?) and scientific accomplishment attempted by some is an artifice soundly backed by ignorance. Once again I refer you back to the quotation from Kelvin on German universities. Most of my scientific career has been with what I consider a great university with a tradition of experimentation in the field of education. In 1961 we did what an historian told me had never been done before—eliminated two (autonomous) departments, geology and meteorology, and spliced them, with some additions from other areas into a single and still harmonious group. The idea was that the study of the earth and its environment is best handled by all concerned with it, and that rigid, old-fashioned subdivisions served no purpose other than to stifle knowledge and obstruct free interchange of ideas and information. The department is called The Department of the Geophysical Sciences. Although I do not believe that universities with national stature should have their faculty or areas of emphasis influenced by location or geography, nevertheless for historical reasons Chicago, in the seismologically stable midwest has never been strong in solid-earth geophysics. We have, however assembled an outstanding faculty in another area of geophysics—hydrodynamics, or geophysical fluid dynamics. The paper advantage of bringing together the people in varied earth-related disciplines is beginning to pay off. Not only have our fluid dynamicists begun interacting rather strongly with our solid-earth people, especially the mineralogists, petrologists, geochemists—call them what you will, but interchange of graduate students has begun. I feel that fluid dynamics can and will make major contributions to our understanding of the deep earth. The earlier mention of the work of Orowan touches upon the necessary interaction of solid state studies with fluid behavior. Mantle convection, or at least motion, cannot be divorced from the substances undergoing the slips, slides, dislocations, or flow. If intermixing of ideas and disciplines does not take place in at least a few educational institutions, where will crossfertilization come from? I am not assuming that it is important for *all* students of mineralogy to rub shoulders with a hydrodynamicist, but I think that it is important that *some* do—somewhere.

I would like to close by once more returning to Lord Kelvin. If the audience thinks I am over-quoting Sir William Thomson let me point out that by recycling Kelvin's words we are helping conserve the energy and diminish the rate of increase of entropy of that universe composed of presidential addresses. In a biography of Kelvin by Andrew Gray (1908), Gray says:

> Chemistry, physiology, and physics cannot be walled off from one another without loss to all; and geology has suffered immensely through its having been regarded as essentially a branch of natural history, the devotees of which have no concern with considerations of natural philosophy. Lord Kelvin's dignified questions[1] were unanswerable:
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> "who are the occupants of 'our house', and who is the 'passer-by'? Is geology not a branch of physical science? Are investigations, experimental and mathematical, of underground temperature not to be regarded as an integral part of geology? . . . For myself, I am anxious to be regarded by geologists not as a mere passerby, but as one constantly interested in their grand subject, and anxious in any way, however slight, to assist them in their search for truth."

AMEN

[1] Kelvin responded to criticism by Huxley, who had accused Kelvin of not being a geologist, but a "passer-by", having no right to offer an opinion on a geological question, and "one who fancies our house is not so well built as it might be."

## References

Bowen, N. L. (1938). Mente et malleo atque catino. *Amer. Mineral.* 23, 123–130.

Gray, Andrew (1908). Lord Kelvin. J. M. Derit and Co., London, 319 pages.

Thomson, Sir William (Baron Kelvin) (1894). *Popular Lectures and Addresses. Vol. II, Geology and General Physics.* Macmillan and Co., London, 599 pages.