Scientific Testament

of Geoffrey F. Davies

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The sum of my scientific accomplishments seems to have been rather neglected by those who study the Earth's interior. I am known for significant contributions, but there seems to be not much awareness of the coherent larger picture I was driving for, and think eventually emerged. I offer this summary in the hope that the larger picture and the work leading to it are not simply lost, but may be found and used by a new generation.

On mantle convection, I clarified the relationship between tectonic plates and mantle convection, which had been a topic of great confusion and some acrimony for about 25 years: the plates comprise the active, cool thermal boundary layer that drives the dominant interior flow. I also showed how mantle plumes are a second, independent but interacting mode of convection driven by a hot thermal boundary layer at the base of the mantle. Along the way I offered a robust argument that the mantle does not convect in two separate layers, as a majority contended, which was later confirmed by seismology. This work was synthesised in the book *Dynamic Earth*[1].

On mantle geochemistry, I found plausible ways to reconcile the data on refractory trace elements and isotopes and on noble gases with the above physical picture of mantle convection – this had been both confusing and even more contentious for about 35 years. The noble gases were especially enigmatic, but I found how features of mantle convection already established could account for their presence and characteristics, and even offered an approximate quantification of the process.

The noble gas work has been almost completely overlooked, though it completes a unified account of the physics, chemistry and dynamics of the mantle. The most complete summary of this work is in the paper *Dynamical geochemistry of the mantle*[2].

The larger context of this work is that it substantially completes the search, from two or three centuries ago, for a mechanism for mountain building – it demonstrates 'what drives the tectonic plates' in a model that is not contradicted by a diversity of geochemical evidence. My work was placed in this context in the book *Stories from the Deep Earth*[3], which is a non-technical though fairly detailed account of the development of the ideas. The process of mantle convection is the fundamental internal driver of most geological processes, other than those on the surface driven by the sun's energy. In other words it is the fundamental theory of geology taken in the broadest sense - the study of the Earth below the solid surface.

Over many years I also explored the thermal evolution of the Earth's interior, which controls the tectonic evolution. It is still debated when plate tectonics began, and what form of tectonics might have preceded it. I made several pioneering and fundamental contributions to this topic[4].

By 1981 the history of the tectonic plates recorded by seafloor magnetic anomalies had been largely completed, except for some older anomalies in the western Pacific whose relationship to younger anomalies was puzzling. I realised, from a peculiar feature in the younger anomalies, that the Kula plate was younger than the old anomalies and thus could not have formed them. This implied there had been an older plate in the western Pacific, now subducted under Japan. My student Mark Woods named it the Izanagi plate, after a Japanese creation deity[5].

Other work included a heat loss mechanism for Jupiter's satellite Io, thermal evolution of Venus and possible Earth-like exo-planets, mechanisms for major mantle overturns early in Earth history, details of plume sampling of the lower mantle, thermal structure of continental lithosphere, and still other topics.

My PhD work was on the rather esoteric but surprisingly subtle topic of how to correctly formulate a description of elastic properties under extreme hydrostatic stress, as occurs in the Earth's interior. I have been pleasantly surprised to find the main paper from that work[6] has been cited 145 times (over 50 years), so it did not sink entirely into obscurity and perhaps it is a standard reference in that small corner of science.

A lack of attention to the mantle work can hardly be disputed, as my last few, most important papers have been hardly cited. This is documented below.

The earlier work on the physics and dynamics of the mantle did gain some recognition, for which I am grateful: I was selected as a Fellow of the American Geophysical Union in 1992 and in 2005 I received the inaugural Augustus Love Medal of the European Geoscience Union for my work in geodynamics.

Conceivably the work on mantle geochemistry could be found lacking, but it has not been tested by any debate. The reason for the lack of response is a mystery to me, and I can only conjecture some reasons. Several of the main protagonists retired around that time. The siloing of geochemistry and geophysics may be a large part of the reason: I was a geophysical interloper in geochemistry. I was not mentored into mantle geochemistry by a famous supervisor, and so did not inherit any prominence or authority – but neither was I mentored into mantle dynamics. Many geochemists avoid geophysics because they are intimidated by it. It's true everyone is busy in their specialty, but there were people who were thinking about the big picture – that's why there were raging debates. Beyond those possible factors, to simply ignore work that appears in prominent journals and is obviously pertinent to a wide range of Earth scientists is poor scholarship.

This summary is focussed on my work, and of course many others were working on the mantle too, but few seemed to consistently seek a unifying model, as distinct from models or hypotheses that might fit some of the data. Almost no-one seriously crossed the geophysics-geochemistry divide, except Don Anderson, whose ideas lacked coherence. Uli Christensen did insightful physical models addressing geochemistry, in collaboration with Al Hofmann. Of the geochemists, for me Al Hofmann was the standout, developing an alternative and more plausible picture from the geochemistry, though not fully accommodating the physical constraints.

The following is a more detailed summary of my most significant work. There are several systematic expositions of the mantle work, in three books and a paper: *Dynamic Earth*[1], *Mantle Convection for Geologists*[7], *Stories from the Deep Earth*[3] and *Dynamical geochemistry of the mantle*[2].

Mantle convection and its relationship to plate tectonics

Although there was a widespread conjecture that the existence and movement of the tectonic plates was somehow related to convection in the Earth's mantle, there was a great deal of confusion and debate for about 25 years after J. Tuzo Wilson first presented a clear conception of plate tectonics in 1965. One reason for this is that the plates have irregular, angular shapes and a range of sizes and so look nothing like traditional ideas of smooth, fluid convection 'cells' of a regular size and shape. There were many computer models purporting to represent mantle convection that included nothing resembling plates, nor had any but the most vague possible relationship to plates. Some conceived that the plates ride passively on convection cells, though few models got close to such behaviour. Others conceived of the plates being active components but the forces inferred were hard to relate to what the mantle underneath might be doing.

Brad Hager, working with Rick O'Connell, imposed plate geometries and velocities on a viscous spherical shell and showed that the resulting flow did not occur in simple cells, but was globally connected. Hager stated the important insight that the plates 'organise' mantle flow. I had done comparable two-dimensional models, but saw the need to include a plausible lithosphere, which is much 'stiffer' than the underlying mantle. This was difficult to accomplish, computationally, but eventually showed that the lithosphere is stabilised and prevented from 'dripping' down into the mantle until a subduction zone is reached, the latter's location dictated by the plates. Such models accounted for the distribution of topography and heat flow of the sea floor, so the computer models at last connected with robust observations - mantle convection could be science, not just the 'computer games' that some regarded it as.

My colleague Ross Griffiths made the casual remark, obvious to a fluid dynamicist, that convection is driven by boundary layers (thermal or compositional). Well, I knew that too, but his remark focussed my attention on the lithosphere as a thermal boundary layer, and the relationship between the plates and mantle convection became clear: the plates comprise the active cold thermal boundary layer at the top of the mantle, and their sinking at subduction zones drives the main large-scale flow in the mantle. A wide-ranging review in 1992, starting from this premise, synthesised many ideas and observations in a paper called *Mantle Convection*[8]. This became widely used by graduate students and fed into the definitive book *Dynamic Earth* (1999).

Colleagues Ross Griffiths and Ian Campbell meanwhile did laboratory experiments that elucidated the shape and physics of a hot, lower-viscosity column rising through cooler, stiffer fluid. The characteristic structure of a mantle plume, with a large spherical head and a thin columnar tail emerged. The head accounts for giant 'flood basalt' eruptions and the tail for smaller, longer-term volcanic hotspots, as proposed by Jason

Morgan Sr. My colleague Alison Leitch developed detailed numerical models that quantitatively justified the connection with flood basalts[9].

I was able to combine Griffiths and Campbell's insights with a relationship between topography and heat flow, that had emerged from my work on plates, to calculate the heat flow carried by the dozen or so mantle plumes inferred to underlie volcanic 'hotspots' such as Hawaii and Iceland. This was based on the 'hotspot swells' that commonly accompany volcanic hotspots. (Norm Sleep at Stanford had arrived at the same idea independently and we each acknowledged the other's work.) Only about 10% of the Earth's total heat flow seemed to be carried by plumes.

This in turn led to the picture that plumes rise from a hot thermal boundary layer at the bottom of the mantle which is formed by heat conducting out of the hot, liquid-iron core that underlies the mantle. There are robust arguments that such plumes are inevitable.

The picture of mantle convection that emerged from this work is that there are two thermal boundary layers, a cool one at the top and a hot one at the bottom. They behave very differently because of the effects of temperature on rheology. The cold layer at the top is stiff, brittle and a little denser than the underlying, more fluid mantle. The cold layer is broken into plates that sink at their edges and drive deep mantle flow. The hot layer at the bottom is lower-viscosity and forms rising columns that become plumes. The two modes of convection are distinct but they do interact to a degree. Plumes are not the 'return flow' of sinking plates. The relationships between plates, plumes and mantle convection were thus clarified.

Mantle chemistry: layering, heterogeneity and isotopic ages

Meanwhile geochemists were using trace elements and isotopes as finger prints of rock sources, and were concluding that the mantle is layered. The upper mantle (above about 650 km where crystal structures change under the high pressure) is 'depleted' in 'incompatible' elements (i.e. those that would rather be in melted magma than trapped in a tight crystal structure), whereas the lower mantle is 'primitive', i.e. unaltered since the formation of the Earth. This picture was incompatible with the developing geophysical picture, and a great argument ensued.

It had emerged in the geophysical work discussed above that a layered mantle is incompatible with observations of sea-floor topography. The reason is that much of the Earth's heat budget would have to come from the lower-mantle layer, and that heat would then be carried to the surface by plumes that would have to be much stronger than are indicated by the observed, modest hotspot swells. This is a robust argument against such mantle layering, but it was widely ignored. Eventually seismology resolved subducted lithosphere descending deep into the lower mantle and persuaded many geochemists (and some geophysicists) that the original version of the layered mantle was not viable. But a common response was just to push the deep layer even deeper; that was still precluded by the heat flow argument but that constraint was still widely overlooked.

By 1981 I had decided I needed to understand the geochemical evidence and arguments – after all we were supposed to be talking about the same planet[10]. This was a big undertaking because the data were diverse and confusing, more were coming out all the time, and rancorous debate was the norm. Very few attempted seriously to cross the geophysics-geochemistry divide in either direction.

The original evidence for a primitive lower mantle was sparse and soon dropped from view without noticeable comment, presumably because the data were from continental sites and probably contaminated by continental crust during ascent of magma.

Nevertheless the primitive lower mantle idea persisted because samples from mantle plume sites, presumed to be rising from deep in the mantle, had more primitive noble gas isotopic ratios than samples from mid-ocean ridges, which sample the shallow mantle. For example ^{4}He is produced by radioactive decay of uranium, whereas any ^{3}He found must have survived from the formation of the Earth. Some of the 'plume' samples have higher ^{3}He/^{4}He ratios than mid-ocean ridge samples. It was proclaimed that the lower mantle was 'un-degassed', which was nonsense because it had probably lost 99.9…% of its noble gases. The correct statement was that the plume source(s) had retained slightly more of the 'primitive' isotopes, relative to the radiogenic isotopes, than the mid-ocean ridge samples.

Other isotopic data, notably of strontium (Sr) and neodymium (Nd) spread along a linear correlation. The spread of these data was presumed to result from mixing of two 'end members' which were presumed to come from the depleted upper mantle and the primitive lower mantle. The 'end-member' compositions, by implication, would have more extreme compositions than any of the observed data. The data did not really fit the simplest idea of the 'primitive' Sr-Nd composition, and it was eventually replaced by several 'enriched' end members revealed by particular hotspots. How this related to the two-layer mantle was not clear.

In 1984 I joined the debate more seriously, pointing out among other things that the Sr-Nd spread of data could just reflect heterogeneity in a single reservoir, rather than mixing of samples from two reservoirs. (Heterogeneities are being injected into the mantle all the time at subduction zones.) There would then be no need for mantle layering. Most geochemists, who had little understanding of physics and fluid dynamics, presumed that mantle convection would stir any heterogeneities until they were homogenised, so the reservoirs would be fairly uniform in composition. This was in spite of there being considerable heterogeneity even among mid-ocean ridge samples. Geochemists designated some samples as anomalously enriched, possibly from plume influences though there was often no plume nearby. They then exclaimed how 'remarkably uniform' the remaining data were, reinforcing the presumption of a 'uniform' depleted upper mantle. This was classic circular reasoning but the community was not receptive to having this pointed out.

Lead isotopic data were also prominent in the debate. On the one hand the Pb data did not correlate simply with the Sr and Nd data and could not possibly be explained by mixing between two reservoirs. This was waved away by saying the lead was 'decoupled', but I never heard anyone explain what process did the decoupling. To me the lead data clearly indicated a heterogeneous source.

The lead data also yielded apparent age information. They indicated the heterogeneities were around 2 billion years old. Geochemists took this to mean there must be more than one reservoir because obviously (to them) heterogeneities could not survive for that long in a convecting mantle.

I thought that heterogeneities might last a long time if convection occurred in a single large layer, and because the motions were quite slow and in a highly viscous (stiff) fluid with no turbulence. I pursued this over a long period, starting with Mike Gurnis' thesis work[11] through fully convective stirring made possible by advancing computers[12], and later including post-doc Jinshui Huang's definitive work, including modelling in 3D[13]. The answer is that heterogeneities can indeed survive for billions of years. They are homogenised not by stirring but by remelting, as they are drawn up to a mid-ocean ridge, which resets the isotopic clocks. Our work established this quantitatively. Huang demonstrated that the rate of processing through the melt zones is a key parameter, and once models are correctly scaled the average 'processing time' between melting events is around 2 billion years.

Another argument for the layered mantle, used from early on, is that the continental crust is estimated to contain about one third of the Earth's complement of Nd. The upper mantle comprises about one third of the mass of the mantle, so it would fit if the continental crust was extracted just from the upper mantle, leaving the lower mantle 'primitive'.

Because the upper mantle was inferred to be highly depleted, this made sense for a layered mantle so long as there is some 'hidden' Nd to add to the crustal Nd so as to match the Earth's total complement inferred from meteorites. So geochemists clung to the idea of a hidden reservoir containing the 'missing' Nd (and argon, another part of the story).

Eventually I addressed this question by looking at a large database of relevant data[14]. My premise was that the mid-ocean ridges sample whatever is below them, and thus fairly directly reflect the heterogeneity of the mantle source. (Upwelling under mid-ocean ridges is passive except where there is a plume, contrary to a widespread misconception.) The relevant quantity is not the most depleted composition but the *average* composition, so it has to include the 'enriched' data commonly excluded by geochemists. The conclusion was that the ridge source contains 2-3 times the Nd (and other incompatible elements) than conventional estimates of a 'depleted' source. In other words the 'missing' components are hiding in plain sight, sprinkled throughout the (single-layer, heterogeneous) mantle. This is a major conclusion, though it has had very little influence. Presumably getting such a basic point so substantially wrong is not so easy to admit.

This finding helped to clarify the radiogenic heat sources in the mantle, which had seemed inadequate to explain the heat emerging at the Earth's surface, and by implication inadequate to drive the tectonic system. That question is not fully resolved, but the discrepancy is considerably reduced. Similarly a question of 'missing' argon was significantly relieved, though not fully resolved.

A final major question was how the 'noble' gases (helium, neon, argon, krypton, xenon) could survive so long in the mantle, and how the plume sources could contain more ‘primitive’ gases than mid-ocean ridge sources. On the one hand Nd, Pb and other data clearly indicated that the plume source had been melted in the past, yet the noble gas data seemed to say it was un-melted, otherwise it would have lost its gases. A resolution involves two parts. First, not all material that rises and melts under a mid-ocean ridge reaches the surface and loses its gases. Material that rises off-axis will deflect to the side and refreeze without being ‘degassed’. Second, some of the latter re-frozen ‘hybrid’ material would be denser than average mantle. Subducted oceanic crust is also denser, and it had been proposed by geochemists Hofmann and White that some of this old crust sinks to the bottom of the mantle, forms the so-called D″ layer, and is then sampled by plumes[15]. Numerical modelling by Uli Christensen, me and others had confirmed that this was plausible. So the implication for denser ‘hybrid’ material is that some of it also collects at the bottom. The D″ layer would then be a mixture of two distinct kinds of material: old oceanic crust, previously melted and degassed, and ‘hybrid’ material that was not so degassed. The seeming contradiction in the data - processed yet primitive - was resolved.

Our numerical modelling had already established that material that sank into the D″ zone would spend longer there than average mantle. Over time it would rise and be melted less often than material in the bulk of the mantle. So the hybrid material in D″ would be less degassed than average mantle. This argument was quantified and found to be plausible. Thus the longer survival of noble gases in the plume sources can be explained as a consequence of features of mantle processing that had been independently proposed and found plausible[16].

It is striking in retrospect how geochemists’ thinking was channeled by ideas proposed early when the supporting data were still sparse. The layered mantle hypothesis implanted not just one but three key concepts that hindered the interpretation of geochemical data. First was the ‘primitive reservoir’ idea. Second was that observed heterogeneity in mid-ocean ridge samples reflected mixing from distinct reservoirs. Third was the presumption that a convecting mantle would be quickly homogenised, so the putative reservoirs would be ’uniform’ in composition.

The ‘mixing’ interpretation required that the ‘end-member’ compositions of the reservoirs are more extreme than any of the data. This meant the putative ‘depleted upper mantle’ was strongly depleted - more depleted than the most depleted sample. But if the upper mantle was so depleted, then another ‘hidden’ reservoir was required to account for the Earth’s complement of key elements like Nd.

I had to break through these constraints on thinking, and then to find arguments and observations that could support an alternative, more coherent, interpretation.

I do credit geochemist Al Hofmann for breaking away from majority thinking, first in proposing with Bill White that plumes sample ancient subducted oceanic crust at the bottom of the mantle[15], then in arguing explicitly that there is no evidence for a primitive layer, and over a long period for carefully arguing from the data with fewer preconceptions than most[17]. Even so, although he was complementary it was clear to me that he never really got the point of my work, and did not fully understand the physical constraints that his interpretations could not fully accommodate.

Selected citation numbers

The numbers of citations of my late work on geochemistry in the mantle are clearly anomalously low, as the numbers here demonstrate.

According to the Web of Science, January 2025, my papers (103 listed there) have been cited on average over 60 times each. My most cited paper[18] has been cited 350 times.

I have 20 publications that have been cited at least 100 times each, and 40 publications cited at least 50 times each.

I have a Hirsch Index of 44, which means there are 44 papers that have been cited at least 44 times each. This is a rough measure of consistency of influence.

In this context, my 2009 paper advocating a heterogeneous mantle[14] has been cited 22 times.

My 2010 paper on noble gases[16] has been cited 11 times.

My systematic exposition in 2011 of (mainly) the place of geochemistry in the convecting mantle[2] has been cited 6 times.

Time since publication is not the issue: another 2009 paper making a significant but not profound point relating to thermal evolution[19] has been cited 99 times. A 2008 paper on possible episodic layering of the early mantle[20] has been cited 69 times. My more noted publications would typically be cited at least 10 times per year for some years. Thus the key papers from 2009 and 2010 could plausibly have been cited 150-200 times over 15 years, rather than 22 and 11 times respectively.

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