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Discussions

Interactive comment on "Dynamical geochemistry of the mantle" *by* G. F. Davies

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I thank Matthew Jackson and Igor Tolstikhin for their constructive comments. They enable me to clarify some important points and include additional material.

I agree the recent work on the non-chondritic mantle needs to be covered. As a result I would shift the focus of the paper a little, to emphasise processes, and to accommodate both the old and new "primitive" benchmarks. With that shift, I think the paper's arguments about how a heterogeneous mantle should be interpreted remain valid, relevant and important. Old interpretations have been biassed, and although the non-chondritic interpretation may be less strongly affected by the bias it is still important to properly understand mantle processes and their interpretation. As well, some important points have not been fully understood by the reviewers, which is not too surprising given the substantial shift in perspective proposed in the paper. They show me where the expla-

nations can be clarified. I would also note that the paper is a review or consolidation of previous work, and so was not intended to explore new topics, though of course it needs to accommodate relevant new material. I will begin with Jackson's comments.

Response to Jackson comments

A central point is that "Depleted MORB Mantle" (DMM) and "MORB source", as used here, are not interchangeable terms. What has been called DMM is biased by the way it is conceived and by the resulting incomplete selection of data. The MORB source, on the other hand, means simply the source of whatever is erupted at mid-ocean ridges. The mass balances and total heat generation depend, quite rigorously, on the mean composition of the mantle; this mean is not just "useful indeed". In the conception of the mantle being tested here, which is based on the seismic evidence, the MORB source is all of the mantle except the D" zone, the only region that is clearly distinguishable and therefore plausibly not fully involved in the circulation of the main mantle. Where the paper deals with mass balances and heat production, it is necessarily the mean composition of the MORB source that is relevant. I am not redefining DMM so much as pointing out that it is ill-defined, not a very useful concept, and not relevant to the big questions if the mantle is heterogeneous.

I was aware of the Boyet and Carlson work, but not of some of the more recent follow-up, some of which post-dates the work that is reviewed in my paper. The non-chondritic mantle clearly needs to be accommodated. It goes a significant way to resolving the mass balance problems. The interpretation of noble gas observations is shifted and sharpened. However the main arguments of the paper remain valid and relevant: the implications of heterogeneity and physical processes have not been properly appreciated, the mass balances still need to be based on proper mean values, and the differences among noble gases still need to be explained.

Another key point is that of course mixing will occur at smaller scales, but here the range of isotopic data is explained as reflecting a comparable range of heterogeneity in the mantle source, rather than mixing between mantle-scale reservoirs or layers with end-member compositions.

I will now go through the issues raised in the order listed.

I.1 I understand that Workman and Hart used a melting model to test their composition. However they also invoked "mineral/melt partition coefficients" (p.54-55) to infer whole-rock compositions from clinopyroxene compositions, so I think my point about invoking equilibrium melting is appropriate.

I.2 Covered by the opening paragraph above. I would not say it is simply "OK to include any MORB data", rather all data should be included, weighted in proportion to the volume of the source they represent, as best can be estimated. This is a straightforward implication of needing the mean composition, neither "casual" nor "extreme".

I.2.A The main purpose of the paper, and the precursors it reviews, is to lay out a new conception of the mantle and how to analyse the observations, and in some places it offers no more than indicative and illustrative data. A fuller exploration of the data would be an on-going consequence of adopting this point of view. So Fig. 16 is one of the few histograms I could find that illustrates the spread of helium data in the way I needed. It is not intended to include the most up-to-date observations. The additional observations Jackson refers to do not change the general picture being portrayed.

I.2.B Similarly the PetDB data used are illustrative rather than definitive, and they are described as such in the text. It will be a continuing and far from trivial task to develop refined estimates of the mean MORB source composition.

I.2.C It is crucial here to distinguish "MORB source" from DMM. The MORB source does indeed include significant contributions from plumes (Section 7.3), and by implication from D". Nevertheless D" can still have a distinct composition of its own. Although it is counter to most thinking to date, Iceland should be included in the MORB source, appropriately weighted, because the plume column will be contributing to heat-

ing the mantle and its complement of trace elements needs to be counted in the total inventory.

II.1 I agree the higher Sm/Nd implied by Boyet and Carlson's 142Nd anomaly is important. If the implied depletion is reflected in trace element abundances, as inferred by Boyet and Carlson 2006, then some of the difficulties addressed here are reduced, but not eliminated. However other difficulties are magnified, particularly the heat source content of the Earth. This paper points out implications of mantle heterogeneity that have not been fully appreciated, and those implications remain. The geochemistry of the mantle still needs to be interpreted in a more sophisticated way than has been the norm until now.

II.2-4 I agree the new version of "primitive" is more compatible with "undegassed", or less degassed, and the distinction from the old, chondritic version of primitive needs to be clear. The new version can actually fit comfortably in the mantle picture developed in my paper, because the D" is found in numerical models to contain material of virtually all ages, including ancient (Section 3.9 and Davies 2008 Fig. 8). The physics still leads to a picture where D" contains a mixture of ancient or less-processed material and depleted, degassed subducted oceanic crust. The samples with lower 3He/4He and non-primitive Nd still need to be explained, along with MORB-OIB differences.

III End members and mixing versus sampling heterogeneity. Yes, I think I see where the confusion comes from. Many of my arguments apply to large-scale reservoirs in the context of addressing mass balances and heating. At smaller scales (magma chambers, the melt extraction zone, i.e. up to $\sim$100 km) mixing between heterogeneous components will certainly occur. Also if one is trying to identify the source of a particular kind of heterogeneity that comes from outside the main mantle reservoir, such as recycled crustal material or non-chondritic primitive material from D", then an end member is relevant.

However the most depleted mantle components are just the extremes of the depletion process, without an external source, so the depleted MORB mantle “end member” does not have global significance. I argue therefore that continuing attempts to define or use a “depleted end member” of the mantle compositional distribution is misguided and seriously misleading when large-scale relationships are the focus. It is misguided because it cannot be clearly separated from the whole distribution, as Hofmann has also been stressing. It is misleading because it is not representative of the mantle composition, because less-depleted components are arbitrarily excluded. To put it another way, the focus on a depleted end member (DMM) is a hangover from the layered mantle conception. Although most geochemists readily agree that the mantle is heterogeneous, the notion of DMM continues to limit and distort their interpretations of the observations.

III.1-2 The context here is mass balances and mantle heating, so the above comments apply. A spread of isotopic values need not be due to mixing between material from different layers, in other words between material from different large-scale reservoirs. Each point in the spread can instead be a more local (average) value from a particular locality in the mantle.

III.3 I agree that the re-emergence of the primitive source hypothesis means this discussion needs to be qualified. There are two questions: (1) is there an identifiable original source of helium? (2) how much helium has been retained in the mantle, and can that be accounted for in a dynamical model? My comments still apply to the second question.

III.4 Consistent with my statements here that mixing will certainly occur at smaller (up to $\sim$100 km) scales, the mixing referred to here is between components of a heterogeneous source, not between material from different layers or physically separate reservoirs.

IV The context of the quoted passage is the possibility of “major differentiation and stratification” formed from a magma ocean. Despite recent evidence of “vestiges of a

beginning”, they are still only vestiges, and the mantle is not obviously grossly stratified. I agree however that the idea of heterogeneity developing early can be supported by the evidence mentioned by Jackson.

V Of course the context determines whether one focusses on the 97% extracted or the 3% remaining. The context in this paper is the use of the melt products (containing the 97%) to estimate the composition of the residue (containing the 3%). If the residue contains 3% rather than the 1% estimated from equilibrium partition coefficients then the latter estimate is seriously in error. My point is that the physics of melt migration can change the 1% to the 3%, therefore I stand by my statement that chemical partition coefficients may be of secondary importance.

Moderately important issues:

1. Noted, point will be qualified.

2. Noted.

3. MOR topography is not due to buoyant upwelling, but to cooling and subsidence of lithosphere, i.e. to negative buoyancy leading to downwelling. This will be clarified.

4. Noted.

5. Will clarify.

6. Will clarify: the calculation refers to the MORB source, which is most of the mantle except for D”, and is not “DMM”. Explicitly accounting for D” makes little difference to the result.

7. Will note. But it makes little difference to the point that long residence times can account for unradiogenic helium.

8. Will clarify: D” “includes” oceanic crust, equivalent to a layer of pure oceanic crust $\sim$100 km thick.

9. Will clarify: in physical terms, the "source" of plumes is the interface through which heat conducts into the MORB source. This could be the top of D", or it could be the top of the hypothesised thick, enriched Kellogg layer (or the EER discussed by Carlson and Boyet 2008). I will will also note that the EER is unlikely on physical grounds, and therefore the Earth as a whole would be non-chondritic.

10. My interpretation has followed Hofmann's, but I will note the qualification.

11. Noted. It reinforces my point.

12. Kellogg assumed the usual strongly depleted MORB source. If you allow more U in the MORB source you don't need the Kellogg layer. If you reduce enrichment in the Kellogg layer to be consistent with observed plume strengths it could probably not survive as a distinct layer.

Minor comments: points will be clarified as appropriate.

Response to Tolstikhin's comments

General comment

If I understand the comments correctly, I need to more clearly acknowledge the idea of a primordial distinct layer as an alternative hypothesis, with reference to Tolstikhin's later papers, and to note that the Xe observations require distinct degassing timescales and sources, implying the survival of distinct source types in the mantle, a point I had not appreciated. (I did acknowledge the Tolstikhin and Hofmann 2005 paper more clearly in Davies 2010, and it was a little negligent not to be as clear in this paper - I was focussed on presenting my own story. Also I had not read the later work.)

I think these points are complementary to mine and not mutually exclusive. In particular I would see the distinct 129Xe signature as surviving as an ancient remnant in D", much as the recent He and 142Nd data suggest an ancient remnant that can be sensibly located in D".

Regarding the use of the U-Th-He system, my point is not that it is definitive but that the source of unradiogenic helium has been enigmatic and, apart from the T&H paper I was not aware of any explanation that survived critical examination (Davies 2010). I agree the Xe observations provide important information.

I will qualify the noble gas calculations to acknowledge the possibilities regarding the earliest Earth. Although there are large uncertainties in possible initial concentrations, there are corresponding uncertainties in degassing rates and degrees. I will be clear the calculations are indicative, not definitive, and I think the demonstration of the later evolution remains valid.

Specific comments

p. 255, l15-25. I have acknowledged the likelihood of an early period of fast cooling and fast mantle convection (p.275, l25-6). Indeed it is a feature of my previous thermal evolution calculations. During this period, the cooling rate is not controlled by the radiogenic heat production but by the high initial temperature, so there is no contradiction in my statements.

p. 269, l14. The estimate is for a cooling time (to a stiff slush) of a magma ocean, not the early transient cooling of the solid mantle, which indeed would take several hundred million years. Stevenson also arrived at a similar time.

p. 269 l20. I mean detectable heterogeneity would be expected, I will clarify.

p. 269 l27 - 270 l5. I don't agree that "both MORB production and composition are known". MORB production is very different in a heterogeneous source, and therefore it cannot, at present be accurately specified. I agree that subduction is a major control on mass balances. I am not arguing that mass balances are entirely controlled at MORs, but that the estimates made at MORs are inaccurate. If previous estimates are inaccurate, then the MORB source can indeed be less depleted than previously estimated. Nor is MORB composition "known" if only a biassed (depleted) sample of

MORB has been included in previous estimates.

p. 271, l15. I was making a general, qualitative point. Earlier interpreters made the unjustified assumption that the presence of some 3He in the mantle required a (large) surviving primitive source. I will include the additional estimates of the initial abundances, but Table 1 is an independent estimate of present abundances.

p. 272, l5-14. As noted in the response to Jackson, I will distinguish between the old “primitive lower mantle” hypothesis and the new primitive non-chondritic vestige proposal. I will acknowledge the different possible early abundances, but I do not think these strongly constrain the present abundances, given the uncertainties in early rates of mantle convection, melting and degassing.

p. 273, l8-9. True, the formation of hybrid pyroxenite will not change the local U/He ratio. However the ratio will be increased in those portions of the mantle that erupt, degas and are subducted again. This is how the differences in U/He and 4He/3He would arise.

p. 275, l27. This will be acknowledged.

p. 276, l25-28. I will modify and clarify this section about the implications of Xe isotopes.

p. 283, l5-10. (This comment seems more pertinent to my earlier comment about MORB supposedly being “remarkably uniform”, p. 260, l22, an unhelpful qualitative statement). The subject on p. 283 is the internal variation in convecting mantle sources, so comparisons with the crust and continental lithosphere are not relevant. The lesser differences among various estimates of MORB source composition still have large implications, so it is important to get it right, conceptually.

p. 288, l25-27. I agree if one subtracts the continental crust from a chondritic mantle one infers a depletion of around 50%. The problem has been that estimates of mantle composition based on MORB have yielded much greater depletion, and it is that

discrepancy I am addressing. (This is modified if the new non-chondritic Earth is considered.) As noted above, I am not claiming MORs control mantle composition, I am merely quoting others’ estimates of mantle composition based on MORBs.

I will address the remaining minor comments and note the additional references.

Interactive comment on Solid Earth Discuss., 3, 249, 2011.