Solid Earth Discuss., 3, C171–C185, 2011
www.solid-earth-discuss.net/3/C171/2011/

Solid Earth
Discussions

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

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Received and published: 25 May 2011

Summary:

The manuscript provides a synthesis of the current state of understanding of the geochemistry and geodynamics of the Earth's mantle. The author has made a solid effort to merge geodynamics and geochemistry, and has attempted to resolve lingering conflicts that result from a merging of these two disciplines. I enjoyed the manuscript, and it provides a way forward in our understanding of the physics and chemistry of the mantle. That said, the manuscript suffers from some weaknesses and will require substantial revision.

In particular, I am perplexed by the author's approach to re-defining the composition of DMM. The author suggest that the "[MORB] mean must include all components, whether they have been considered enriched, anomalous, plume-related or whatever."

The author uses this casual approach to then justify a MORB source with U concentrations twice as high as previous estimates. There are inconsistencies with this approach (the author's own criteria would suggest that basalts from the Iceland and the Reykjanes ridge be included in the MORB average, but these basalts are not seen in MORB histograms in Fig. 16).

The author also ignores the result of the Boyet and Carlson (Science, 2005), who found small but measurable differences in 142Nd/144Nd between chondrites and modern terrestrial lavas. This is perhaps the most important result in mantle geochemistry in a decade, and is revolutionizing the way geochemists think about the composition and evolution of the Earth. Without some discussion of the Boyet and Carlson work and its implications, the author is missing out on an important advance in our understanding of the mantle.

The author will need to clarify where and how mixing occurs, in the mantle or in magma chambers. The author will also have to clarify what is mixing: the author seems opposed to endmembers, but what else can we call the most extreme melt compositions that mix in magma chambers (or in the mantle)?

These issues and other are outlined below, organized in order of descending importance.

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Major Issues:

I. Redefining DMM: An Ultra-enriched MORB source?

1. Page 288, lines 13-16. The author writes: "The approaches of Salters and Stracke and Workman and Hart evidently were intended to avoid the need for such estimates, although each invokes equilibrium melting models at some stage in the chain of reasoning." The author misrepresents the work of Workman and Hart; the DMM composition of Workman and Hart was constructed independently of a melt model. The

isotopic composition of the MORB source requires specific Rb/Sr, Sm/Nd, U-Th/Pb, Lu/Hf parent-daughter ratios to achieve its present-day isotopic compositions. In simple plots of abyssal peridotite trace element data, trends formed by peridotites (e.g., Sm concentration vs. Nd concentration) will intersect the line that defines the required parent-daughter ratio to generate the 143Nd/144Nd; the point of intersection defines the absolute trace element concentrations in DMM (Sm and Nd in this case; see Fig. 5 of Workman and Hart, EPSL, vol 231, 2005). It was only AFTER the DMM source was constructed that a plausible melt model was used to show that the calculated source could generate melts with trace element patterns quite similar to MORB. In no way did the "chain of logic" utilize a melt model to generate the DMM source. Contrary to the claims of the author, the Workman and Hart DMM source was independently of the melt model.

2. Page 283, lines 12-16. The author writes: "Various demarcations of normal MORB (nMORB) seem to be used, such as that it does not come from an unusually shallow ridge crest, that it does not contain an "obviously" enriched signature, that is it not too close to a hotspot, or that it is the most common composition of the distribution (the mode, in statistical terms). However none of these criteria prescribes a clear-cut boundary." This statement is followed by the following text (page 284, line 1-3). "For considering both mass balances and mantle heat generation it is the mean composition of the mantle that is important. That mean must include all components, whether they have been considered enriched, anomalous, plume-related or whatever." In essence, the author is suggesting that it is 'ok' to include any MORB data in a global MORB average, and the only criteria is that the MORB is erupted at a ridge. This rather extreme approach has some serious consequences, and several inconsistencies, all of which will have to be dealt with. I have three general comments:

A. First, consider Iceland, where topography of the ridge has been lifted above sea level. The author suggests that topography of the ridge should not play a role in determining whether a sample should be included in the MORB database. Are Icelandic

lavas to be included in the MORB mean as well? An immediate implication of this approach is apparent in the MORB histogram in Fig 16: the highest 3He/4He lavas (up to 37 Ra) erupted at hotspot is associated with the modern Iceland plume. But, the author insists that the "mean must include all components, whether they have been considered enriched, anomalous, plume-related or whatever." If we accept his criteria, then Fig. 16 must include the very high 3He/4He values from Iceland, yet the author has not included the high 3He/4He lavas from Iceland in the MORB 3He/4He histogram. Even if the author modifies the all-inclusive approach to exclude Iceland, will the Reykjanes ridge also be included in the MORB average? The Reykjanes ridge is clearly modified by Icelandic mantle material, so if the author excludes Iceland, the Reykjanes ridge should also be excluded. As a result of the author's all-inclusive approach to the MORB dataset, there are many shallow ridges, many that are hotspot-influenced, that the author will need to include in the deplete mantle mean. I am not convinced that the author's casual approach to redefining MORB to include everything at ridges is really an advance at all: "That [MORB] mean must include all components, whether they have been considered enriched, anomalous, plume-related or whatever."

B. The author's use of a MORB database seems a bit casual. He writes (page 286, lines 3-5), "These factors may be minima, because plume-affected ridge segments may have been excluded from the "all-MOR" category of PetDB, though this is not clear from the PetDB summaries." If the author is going to revise the U abundance in DMM up to 10 ppb (a factor of 2 higher than DMM in Salters and Stracke and a factor of 2 higher than the E-DMM in Workman and Hart), the author cannot simply rely on PetDB summaries that the author does not seem to trust. Some time has to be invested looking at the actual data to determine whether plume-influenced ridges are or are not included in the PetDB averages.

C. In Table 2, the author treats D" (the plume source) and DMM (the rest of the mantle, in the author's view) separately, both in terms of their heat budgets and in terms of their geochemistry. In the text, however, the separation between DMM and D" is "blurred".

If plumes contain a component of D", as the author seems to argue throughout the paper, then the inclusion of plume-influenced ridges in the MORB average is tantamount to adding a D" component to DMM, but still calling it DMM! However, plume-influenced DMM is no longer DMM sensu stricto, as a component of D" has been added to it. This gives rise to an apparent inconsistency: The author treats DMM and D" separately in Table 2, but in his geochemical revision of DMM (Section7) he suggests that mixtures of D" and DMMâĂŤseen at plume-influenced ridgesâĂŤsomehow constitute DMM. The author does not let D" and DMM intermingle in Table 2, and should not let them intermingle in his geochemical treatment of MORB data. The bottom line is that plume-influenced ridges cannot be included in a MORB average the represents DMM, particularly if the author wants to define D" and DMM as separate entities. (One reason for excluding plume-influenced ridges is to determine a MORB source as a distinct entity separate from the D" component found in plumes). Again, I would ask that the author modify his somewhat casual criteria for selecting samples for the MORB average: "That [MORB] mean must include all components, whether they have been considered enriched, anomalous, plume-related or whatever."

II. Boyet and Carlson (Science 2005): Implications for a non-chondritic Earth.

1. Page 290, Section titled, "Cosmochemical abundances and thermal evolution." The author includes a section titled "cosmochemical abundances" and ignores the Boyet and Carlson (2005) paper, which shows that modern terrestrial lavas and chondrites have different 142Nd/144Nd. The Boyet and Carlson paper is revolutionizing the way geochemists think about the evolution of the silicate Earth, and the discovery suggests that, within a few tens of millions of years of Earth formation, the portion of Earth's mantle that serves as the source of modern volcanism had a Sm/Nd ratio 4 to 7% higher than chondritic. There are two models for the origin of this ancient non-chondritic reservoir. The first is that it simply may represent a non-chondritic bulk-Earth composition. In the second model, a chondritic bulk mantle may have undergone an early differentiation event forming complimentary incompatible element depleted and enriched

reservoirs, called the "early depleted reservoir" (EDR) and "early enriched reservoir" (EER), respectively; the EDR would be the predecessor to all modern terrestrial mantle reservoirs, and is therefore effectively a primitive mantle reservoir. It is not yet known which model is correct, but they are the two possible outcomes of the Boyet and Carlson result. The Boyet and Carlson discovery is arguably the most important in mantle geochemistry in the past decade (already cited >150 times). The author is missing out on an important aspect of the field by ignoring the Boyet and Carlson (2005) work.

2. Page 271, lines 18-23. The author writes: "In the conventional interpretation the OIBs are taken to be tapping a "primitive, undegassed" reservoir (meaning a reservoir that has not been degassed since early in Earth history, so it has retained essentially all radiogenic products). However this assumption was an extension of the now-abandoned assumption that the lower mantle is primitive in all respects (Wasserburg and DePaolo, 1979)." The concept of a "primitive undegassed" reservoir was largely abandoned when it was realized that lavas with (primitive) high 3He/4He have non-primitive (nearly MORB-like) 143Nd/144Nd. However, with the advent of the Boyet and Carlson (2005) discovery, the 143Nd/144Nd of the primitive mantle (or EDR) may well be 0.5130, a value that coincides with the 143Nd/144Nd found in high 3He/4He lavas. As a result, a series of papers now suggest that the high 3He/4He reservoir in the mantle does in fact represent a primitive (albeit non-chondritic) mantle. See Caro, G. & Bourdon, B. Non-chondritic Sm/Nd ratio in the terrestrial planets: consequences for the geochemical evolution of the mantle–crust system, Geochim. Cosmochim. Acta 74, 3333–3349 (2010); Jackson et al., Nature 466 (2010).

3. Page 272, lines 8-10. The author writes: "The higher values of 3He/4He in fact occur in samples whose lead and neodymium isotopes are most like the more depleted MORB samples." The lavas with the highest mantle 3He/4He values on Earth (Baffin Island, see Stuart et al., Nature, vol 424, 2003) do have unradiogenic Pb's, but the Pb's plot on the Geochron and are consistent with an old mantle age (∼4.5 Ga). Additionally, and as a result of the Boyet and Carlson (2005) discovery, the 143Nd/144Nd of the

Earth's primitive mantle may plots closer to MORBs than it does to chondrites. If the Earth's primitive mantle is not chondritic (and this is strongly suggested by the Boyet and Carlson result), then high 3He/4He lavas are now the clearest candidates for being melts of Earth's primitive mantle (albeit non-chondritic). The author may not like this interpretation, but the author also cannot simply ignore the Boyet and Carlson result and the implications that follow.

4. Page 274, lines 10-15: The author writes: "The apparently contradictory signatures of the OIBs with the least radiogenic helium, namely depletion reflected in refractory element isotopes and a "primordial" component reflected in the noble gas isotopes, would thereby be accounted for." Enrichment or depletion of refractory elements depends on the reference frame (i.e., what is considered primitive). If one is to accept the results of Boyet and Carlson (2005), then the 143Nd/144Nd found in high 3He/4He lavas may very well be primitive (albeit non-chondritic), not depleted.

III. The existence of mantle end members and mixing (melts vs. solids) different mantle components:

1. Page 261, lines 5-6. The author writes: "Rather than mixing, the spread of data may directly reflect the spread of data in the source region." This might be true, if it weren't for magma chambers! Before lavas erupt, magma chambers are guaranteed to mix the various source components that are melted. The author even states on the previous page (page 260, line 9-12) that "Large variations have also been found within single hand specimens (Hofmann, 2003), suggesting that such variations are present in the source, though they will tend to be homogenized within magma chambers during extraction." Either "the spread of [geochemical] data directly reflects the spread of data in the source region (stated by the author), or the data reflects mixing in magma chambers (also stated by the author). This apparent contradiction is confusing for the reader.

2. Page 261, lines 11-13. The author writes: "If end members are disposed

of...the more meaningful measure is the mean value." The author suggests that endmembers are less meaningful than the mean. Endmembers do mark extremes in mantle heterogeneity, and so help define variability and therefore help quantity the time/magnitude/extent of processes that generate the variability in the mantle. If mass balance is the goal, then averages are useful indeed. However, if one seeks to describe the mechanisms/processes responsible for generating the most extreme geochemical compositions in the mantle (aka "endmembers"), then considerations of endmembers is quite useful. The mantle is compositionally variable, and representing the mantle with "mean" values ignores the importance of extreme processes that must have generated the most extreme compositions. It would seem that the "meaningfulness" of an endmember vs. mean would then depend on the question one is trying to address. As an aside, it seems that the author is defining his philosophy in opposition to a particular endmember philosophy? It would be helpful to the reader if paper or two were cited here (Zindler and Hart, 1986?), so that the reader knows which endmember philosophy the author suggests is less meaningful.

3. Page 272, lines 16-18. "This reflects the underlying assumption that distinct reservoirs must be involved, with the distribution reflecting mixing from two (or more) reservoirs. In that case it is the extreme values that are important (Fig. 7a). However if the observed distribution merely reflects a distribution of values in a hetero2geneous source (Fig. 7b), then the extreme values are of no great significance, and it is the mean and spread that are of primary interest." It is not clear from the text, but the author seems to question the idea that mixing of distinct reservoirs occurs in the mantle. Has anyone has convincingly shown that mixing of distinct reservoirs is not a process operating in the mantle? If not mixing in the mantle, then distinct reservoirs certainly mix in the melting column. But can the author really "see through" melting and subsequent mixing and say with certainty that no mixing of distinct reservoirs occurrs before melting? How well are diffusivities of the elements known in the mantle, particularly at the high temperatures and pressures (120 GPa) operating in the lower mantle? H and He isotope will certainly mix between reservoirs, even in the solid state, owing to their

high diffusivities (Albarede, Science v. 319, 2008; Hart et al., EPSL, 2008). This cannot be ignored. Even if the diffusivities are much slower in the solid state for the higher mass elements, then melt layers in the mantle will speed up the mixing process: If melt exists in D", then mixing of distinct reservoirs will certainly occur there. The bottom line is that I don't think that the author can claim that reservoirs avoid mixing in the mantle.

4. Page 273, Lines 11-20. The author writes: "It was argued in Sect. 4 that mantle heterogeneity, including subducted mafic crust and hybrid pyroxenite, will have originated early in Earth history. This implies that successive generations of both materials will have formed. As such materials are carried into a MOR melting zone, it is plausible that their melts will mix to a significant degree, so that melt from subducted oceanic crust, previously degassed, will regain some noble gases. Some of the resulting melt mixture will erupt and degas, so that part of the complement of noble gases will be lost. The remaining melt mixture will be trapped, forming a new generation of hybrid pyroxenite that will contain the balance of the noble gases that entered the melting zone. In this way the noble gases will come to reside in the hybrid pyroxenite component of the mantle." In this paragraph, the author is mixing two compositions and erupting the resulting mixed composition at the surface. But earlier statements in the paper give a distinct impression that the author argues against mixing two compositionally different materials (or two endmembers) as a mechanism for generating isotopic diversity in the mantle. Or, is it that the author does not want mix isotopic heterogeneities in the solid state, but mixing of isotopically-different melts is ok?

IV. Early-formed reservoirs in the mantle

1. Page 269, lines 17-18. The author writes: "The difficulty of finding any surviving remnant of such differentiation provides some evidence for this claim." Quite the contrary. There is significant evidence from 142Nd for the survival of early-formed heterogeneities. Please refer to the following papers: Harper and Jacobsen, Nature 360, 1992; Boyet and Carlson, Science 309, 2005; O'Neil, J. et al., Science 321, 2009. 2. Page 269, lines 21-23. The author writes: "In other words, a heterogeneous mantle,

more or less like the present, may date from very early in Earth history. Consequently we could expect the mantle's incompatible elements to have been concentrated into mafic mantle heterogeneities from quite early in Earth history." The author suggests an early heterogeneous mantle as a hypothesis. There is tremendous geochemical evidence supporting a strongly heterogeneous mantle in the Hadean, and the author should at least mention some of the evidence. The oldest zircons (4.0-4.4 Ga) display Hf-isotopic heterogeneity (Amelin, Y., et al., Nature of the Earth's earliest crust from hafnium isotopes in single detrital zircons, Nature 399, 252–255.; Blichert-Toft, J., Albarède, F., 2008. Hafnium isotopes in Jack Hills zircons and the formation of the Hadean crust. Earth Planet. Sci. Lett. 265, 686–702.; Harrison, T.M., et al., 2005. Heterogeneous Hadean Hafnium: evidence for continental crust at 4.4 to 4.5 Ga. Science 310, 1947). Additionally, there is evidence from 142Nd that the early mantle was heterogeneous in the Hadean (e.g., Harper and Jacobsen, Nature 360, 1992; Boyet and Carlson, Science 309, 2005).

V. Mantle melting and incompatible elements: Partition coefficients don't matter?

1. Page 269, lines 7-9: "The other is that the bulk extraction of incompatible elements will not be governed only, or even mainly, by local chemical partition coefficients." The author claims support for this statement on the following page (page 270, line 6-12): "If an element has a partition coefficient of 0.01, then only about 1% of it will remain in the solid phase, and 99% will partition into the melt. However if only 1% of that melt fails to migrate out of the residue zone, then the amount of the incompatible element remaining in the residue zone will be doubled. If several percent of melt remain trapped, then the bulk abundance of the incompatible element will be several times that predicted by chemical equilibrium partitioning." The flip-side of this calculation is that, even if 1% of the melt fails to migrate out of the residue zone, melting has still depleted the original source of an incompatible element by 98% of its original budget. If 3% of the melt fails to migrate, the depletion relative to the original unmelted mantle is 96%. It seems that the mantle is overwhelming depleted in the incompatible element by melting, even

if 1 or 3% of the melt stays behind! Therefore, the authors statement that "...the bulk extraction of incompatible elements will not be governed only, or even mainly, by local chemical partition coefficients..." is misleading and, based on the author's own calculations, untrue. It is true that the "...bulk abundance of the incompatible element will be several times that predicted by chemical equilibrium partitioning." But the trace element budget of an incompatible element in the mantle source still be greatly reduced (by 96-98%) from its original concentration, even if there is some melt (1-3%) that fails to migrate.

Moderately important issues:

1. Page 250, Line 23-25. The author suggests that there are no correlations between He-Ne-Ar with the "refractory-element" enrichments in OIBs. First, the author must distinguish whether his statement is meant to describe local or global trends. In the first case, geochemical data from individual hotspots are know to exhibit wonderful trends between He-Ne and the lithophile radiogenic isotopes. In the second case, evidence is emerging that Ti may be enriched (relative to elements of similar incompatibility on a primitive mantle-nomalized spidergram) in high 3He/4He lavas (Jackson et al., G-cubed, vol 9, 2008). 2. Page 252. The paragraph (line 8 to 15) regarding noble gases (He, Ne and Ar are mentioned in the paper) and the paragraph (line 17-22) outlining U, Th, and K are linked. U and Th decay to 4He; the resulting alpha particle can generate nucleogenic neon; K decay generates 40Ar. It is worth mentioning this fundamental link: One paragraph describes daughter isotopes systems, and the other discusses parent concentrations. 3. Lines 15-17. The author writes: "...would generate surface topography comparable to the topography of the mid-ocean ridge system." The author writes that no such topography exists, then writes that such topography exists at mid-ocean ridges. This is confusing, and it would help if the author would explain what kind of topography is expected from the "buoyant upwelling" scenario. 4. Page 254, line 15-19. It would be good to mention here, and elsewhere in this paper, that a layer composed of heat-generating oceanic crust will be U-Th rich and will have great heat-

generating capacity. This will favor its incorporation into regions of mantle upwelling. 5. Page 255, line 4. The "dry solidus" is one endmember, but it is thought the DMM has 100 ppm water (see Salters and Stracke, G-cubed, vol 5, 2004). This should move the solidus to greater depths (see Kushiro, 1969; Green, 1973; Gaetani and Grove, 1998; Asimow and Langmuir, 2003). Also, what about the low degree carbonatite melts at depths of 300 km (Dasgupta and Hirschmann, Nature, vol 440, 2006) 6. Page 256, line 2-4: The author writes: "Thus, most of the mantle will have been processed through the melting zone, and we cannot expect a significant amount of primitive, unmelted mantle to have survived in the MORB source." This is confusing, as the author is modeling the ENTIRE mantle, not just the MORB mantle. (At least, it is apparent from page 255 that the model is intended to describe the entire mantle.) However, if the author is alluding to a model proposed much later in the paper, where he suggests that 99% of the mantle is considered to be depleted MORB mantle (DMM), then the author needs to state explicitly on page 256 the he thinks nearly the entire mantle is DMM. Otherwise, the sentence is very confusing to the reader. 7. Page 276, line 7. Please update the maximum 3He/4He observed in OIBs. Stuart et al. (Nature 2003) reported widely-accepted values of $\sim$50 Ra in Baffin Island flood basalt lavas. If you would prefer to use OIB lavas, Hilton et al. (EPSL 1999) report a value of $\sim$37 Ra in an Icelandic lava. 8. Page 279, line 1. The author uses a deep mantle layer that is 100 km thick. However, in the introduction (page 251, lines 14-15), the author indicates that D" is 200-300 km thick. It would be nice to have this inconsistency resolved in the model. At the very least the author could mention in the text that the layer is not as thick is suggested for D". Perhaps the proposed layer has nothing to do with D" at all? 9. Page 278, lines 1-9: The author writes: "The difficulty with the geochemical argument is not simply that such geophysical evidence as has been offered for a deep mantle layer is not compelling (van der Hilst and Karason, 1999). The larger difficulty is that the geophysical observations preclude much of the Earth's heat flow from coming from deeper than the source of mantle plumes, otherwise plumes would be much stronger than they are observed to be. The further implication is that much of the Earth's heat is generated within the MORB source. This implication is inconsistent with conventional estimates of the composition of the MORB source, which has been inferred to be strongly depleted of incompatible trace elements, including heat source elements." This is confusing. It seems that the goal of this paragraph is to "rule out" a deep primitive geochemical layer. First, the author rules out a significant heat source deeper than the source of mantle plumes (i.e., deeper than D"), which would rule out the core as a major heat source. But I don't see how this rules out primitive layer above the core, but below DMM? 10. Page 258. The author presents on hypothesis for the apparent 1.8 Ga age of the mantle. Another interpretation for the apparent 1.8 Ga secondary Pb-isotopic isochron comes from the observation that the average age of continental crust is $\sim$2 Ga. The continental crust is extremely Pb-rich compared to most mantle reservoirs, and subduction of continental crust or continentally-derived sediments will have a powerful leverage on the Pb-isotopic evolution of the mantle. For example, subduction of average continental crust (the author is more interested in averages than endmembers, after all), or marine sediment (which are dominated by terrigenous material) into the mantle would then generate a mantle isochron of right age. This alternative interpretation has little to do with residence times in the mantle, and more to do with crustal formation times. 11. Page 271, line 16 and 17 . The author writes: "A second difference in all three systems is that the OIB means differ from the MORB means. In the case of He, the difference is less than a factor of two." Owing to bias of sample/location measurement, helium is extremely difficult to evaluate so simply. When a noble gas geochemist finds a high 3He/4He ratio, s/he tends to measure all of its "buddies" from the same location. This community's obsession with high 3He/4He lavas has likely inflated the difference between OIBs and MORBs in the author's histogram. 12. Page 279, lines 25-27. The author writes: "The deep layer proposed by Kellogg et al. (1999) (Fig. 20a) would contain about half of the Earth's radioactive heat generation. This would generate strong upwellings, in the upper layer that would in turn generate substantial topography. . ..". This argument depends on the U, Th and K concentrations of Kellog's deep layer. If the Kellogg mantle has DMM concentrations that are as high as the author suggests in

this manuscript (10 ppb), then Kellogg's deep layer wouldn't require as much U and it wouldn't produce as much heat.

Minor comments:

Page 250, Line 22. The word "gassy" seems awkward. Perhaps replace with "gaseous" or "gas-rich"? Page 252, line 6: "appropriate residence times"? What does the author mean? If he means 1.8 Ga, this should be stated. Page 253, line 5-7: Please rephrase this sentence. It is confusing. What "sketch"? What "gradient"? Page 257, lines 3 to 5: Cite Hauri, Nature, vol 382, 1996. Major element heterogeneity has already been inferred geochemically. Page 260, line 9 and 10: The author states, "Large variations have also been found within a single hand specimens". Is the author referring to melt inclusions in a single MORB hand specimen? If no, what does the author refer to? If yes, this might be a good place to mention that melt inclusions, which provide isotopic evidence revealing significant magma chamber heterogeneity exists before complete melt aggregation and homogenization (e.g., Saal et al., Science vol 282, 1998). Page 260, line 13-15. A more recent example of this kind of modeling exercise can be found in Brandenburg et al., EPSL, vol 276, 2009. Page 267, line 22. Cite a more recent paper dealing with eclogite-pyroxenite melting inferred from magma compositions and olivine chemistry: C. Herzberg, Identification of Source Lithology in the Hawaiian and Canary Islands: Implications for Origins J. Pet 2010. Page 270. Define "partition coefficient". Page 271, line 5: Cite Gonnermann and Mukhopadhyay, Nature 459, 2009. Page 273, line 21. "Nucleogenic" is not correct for helium. Use nucleogenic for neon, radiogenic for helium. Page 273, lines 5-7. The following sentence is confusing: "Hofmann has long emphasized that geochemically oceanic crust is part of the dynamic mantle system, rather than analogous to continental crust; Hofmann, 1997, 1988." White and Hofmann (Nature, 1982) were among the first to suggest that recycled continental crust plays an important role in generating mantle heterogeneities. Hofmann (Nature, 1997) modified this claim, and suggested that, while continental crust plays a role in generating mantle heterogeneity, its role is limited. Page 275, line 15. ". . ..are also too low. . .” Too low in what? MORB? Hybrid pyroxenite? MORB-source material?

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