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Interactive comment on "Coupled dynamics and evolution of primordial and recycled heterogeneity in Earth's lower mantle" *by* Anna Johanna Pia Gülcher et al.

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Review of Gülcher et al.

This is a well written and presented paper that covers the mixing of heterogenous mantle reservoirs in Earth evolution. In particular, it articulates clearly the difference between LLSVP reservoirs, and BEAM-like or ribbon-like mid-mantle reservoirs, showing how they could form independently and represent relict mantle of different antiquity, potentially reconciling some geochemical and geophysical anomalies.

I think, on the whole, it's quite publishable. The assumptions and simplifications behind

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the modelling are quite clear, and the results and discussions follow clearly from that. I think the presentation (and ordering) of the information could be improved, and I offer some suggestions for that below - mostly around the ordering of the results, presentation of snapshots, background material appearing in the discussion section, etc. It's probably easiest to approach this sequentially, so I'll do that below.

Line 50-54: I note you mention 3D mixing in the discussion later, but I feel it is worthwhile bringing that content into the background here. In particular, Coltice and Schmalz showed the differences between 2 and 3D for high-Ra regimes were not that significant, so you could bring that discussion here. For more realistic convecting systems, you might considering looking at these 2:

O'Neill, C., Debaille, V. and Griffin, W., 2013. Deep earth recycling in the Hadean and constraints on surface tectonics. American Journal of Science, 313(9), pp.912-932.

https://www.ajsonline.org/content/313/9/912.short

O'Neill, C.J. and Zhang, S., 2018. Lateral mixing processes in the Hadean. Journal of Geophysical Research: Solid Earth, 123(8), pp.7074-7089. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JB015698

Which I somewhat bashfully suggest as I was involved in them (thus - take it or leave it, but relevant if you want to justify using 2D c/w 3D).

Line 66-67: "robustly predicted in many experiments" - better add a cite here.

Line 69: "a hybrid state" - do you think it is terribly different from marble cake with scale variance?

Line 76: "512.96" looks like a decimal, expand to "512 x 96 cells"

Table 1. The activation volume and energies are the same for the upper mantle (mostly olivine) and bridgmanite (PV). This is a bit of a stretch, and it was worth noting this in the review (although I'm sure nothing new to the senior authors). The energies themselves

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are very low compared to lab measurements (c/w Hirth and Kohlstedt, or Karato and Wu for olivine, E∼375e3), and the activation volume is appropriate for bridgmanite, but not for olivine. This certainly makes the models easier to run as things converge, but is it realistic? There is some token justification (lower mantle/grain size/stress) but this is not very well developed. I would like to see a little more text on justifying using these parameters properly, and what the potential implications are of this choice (eg. for the upper mantle, it results in very different behaviour near the lithosphere).

L147-148: "density (FeO enrichment)": can you expand on the exact density difference between FeO and bridgmanite in the lower mantle, as this is quite relevant here. It sounds like you are saying FeO is denser - I encourage to revisit this statement carefully (and calculate the density profiles of bridgmanite vs FeO in burnman to see what I mean).

Figure 2: I feel like this figure should come after the figures showing the timeseries of behaviour, which probably feeds into the whole structure of the results. Show the models first, and describe the mixing patterns, and then present a regime diagram of mixing. Showing this first is topsy-turvy and throws the structure of this section off.

Line 252-253: If they are not directly related to the Earth, it does beg the question of whether you want to include them or not. When I read this part, it strikes me as student project (I know, I know, but best avoid that impression in a published paper), where you made them again and went back and had to do them again. I mean, that's how we work, but often a paper is improved by leaving out the bits that are not really relevant to the final message. Could these be an appendix?

L264: initial primordial layer thickness: can you justify this better? why 1844km thick?

L285-287. Could you expand on this more? I'm familiar with the work, so I get what you're saying, but not everyone will. Why exactly does a thinner bridgmanite layer lead to lower melting? Is it a fertility issue? Or dynamic? And maybe expand on why hotter mantle leads to more melting and thus a cooler mantle - those thinking in terms of plate

cooling only will be thrown by this point.

L351-356: Disproportionation of FeO during core formation is the most obvious (and increasingly commonly cited) cause of lower mantle heterogeneity. It's not explicitly mentioned here - I recommend Frost and McCammon (2008) for a summary.

L362-364: This goes off on a tangent here - is it needed? I feel this opens a can of worms you don't have space to really cover.

L375: I think Rhodri thought he could explain the anticorrelation with the thermal (PPV) mechanism.

L382-391: The discussion in generally is a bit more disjointed than a lot of the rest of the paper, and skips from point to point without much continuity. The section in particular is hard to follow, and just really needs a rewrite.

L408-411: Can you explain better what the iron spin transition has to do with bridgmanite structure size/scale? This is not clear.

L432-434: MgO-rich komatiites are very much more dense (at the surface) than basalt - are you talking about after the eclogite transition? If so, this statement needs to be much clearer, as written it is quite wrong. See van Thienen et al (2004) for effect of MgO content on mafic rocks.

L456-457: I think you need to cite the old original Stein and Hoffman (1994) mantle overturn model here: Stein, M. and Hofmann, A.W., 1994. Mantle plumes and episodic crustal growth. Nature, 372(6501), pp.63-68.

L477-479: A disjunct with seismic models seems problematic. It's one thing to point it out, but as it stands you seem to be letting it undermine the whole work. You should really advance a reason for the discrepancy (are the differences - quantitatively - so large in terms of T? Is your core evolving? (No? Then it may overestimate heat flow. Etc etc).

L483-487: Again I think this whole discussion of 2D vs 3D is best nipped in the bud at the start - see previous comment.

L490: Without radioactivity or core evolution, these aren't strictly evolutionary models, they are static steady-state models, started at adiabatic conditions, that are run for long periods of time. Apart from the destabilisation of heterogeneities, there is not much 'evolutionary' in them, and this point needs to be made clearer, and earlier.

L508-510: Again, this statement needs to be couched. The timescales for systems with significant radioactive decay are likely very different from the steady-state systems here. A CMB temperature of 4000K is probably ok for some of the early Earth - it's a little higher than estimated today - but probably underestimates early evolution significantly. I think needs to be tackled more explicitly in the discussion, along the lines: "What can we tell about mixing timescales on the Earth from steady state models? Well, we select TCMB and adiabat values around ballpark (average for this period - acknowledging these values evolved rapidly during this time). Based on this we can say..."

I don't think this is too much overhead, but at least shuts down the critique of the evolutionary aspect missing here.

Ok, that's all from me. Nice job - and good luck!

-CO

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