RESPONSE TO REVIEW

REVIEWER #1

The study by Desiderio et al. presents a systematic exploration of the effect of viscosity and density differences on the preservation of primordial mantle material as BEAMS and the generation of LLSVP-like accumulations of recycled oceanic crust (ROC). Overall, I think that this is a nice contribution and should be suitable for publication after minor revisions. In my view the most significant issue is the omission of internal heating, which at least needs to be physically justified.

We thank the reviewer for their time and their insightful feedback. We address the comments one by one in the following.

1) Line 28: I do not think that it's reasonable any longer to claim that the nature of the LLSVPs is strongly debated. Masters et al. (2000) described the anti-correlation of bulk and shear wavespeeds in the lowermost mantle and there is no way to reconcile this with a purely thermal origin. More recent work by Lau et al. (2017) using tidal constraints and by Moulik and Ekstrom (2016) using normal mode and body wave tomography make a compelling case for high intrinsic density within (at least part of) the LLSVPs as well. It seems that this debate was vigorous in the 2000s but has been long since settled.

We rephrased the sentence (see new manuscript in lines 27-29).

2) **Line 48: Seismic-tomography images is awkward. Suggest "tomographic images" instead.**

We changed as suggested (line 49).

3) Line 120: The choice to run models without internal heating needs to be justified physically as it seems inconsistent with what we know about the energetics of mantle convection, and the paper aims to understand the preservation of heterogeneity under Earth-like mantle evolutionary scenarios. Under ancient, hotter mantle conditions, mantle heat production was larger than the present day value and the omission of mantle heat production seems even more unreasonable. In general, heat production will destabilize convectively isolated 'blobs' in the mantle because they will heat up and become more buoyant and less viscous (e.g. Becker et al., 1999). It's not clear to me, given all of the complexities of composition-dependent buoyancy and viscosity considered here, how the choice

to run models that are not earth-like in terms of the energetics of convection can be justified. I understand that HPEs are incompatible in Bridgmanite, so they may be concentrated in the later products of magma ocean crystallization. But they are also even more incompatible in ferropericlase and moderately incompatible in CaPv, and they have to go somewhere to conserve mass during magma ocean crystallization! The recycled oceanic crust should also be enriched in HPEs and the harzburgitic residue depleted. The authors could comment on whether this could affect the dynamics of ROC accumulation in the lowermost mantle.

To address both BEAMS survival and ROC accumulation in an internally heated mantle, we ran 4 additional cases and describe the results at the end of the Results section (newly added lines 332-355) and in a newly added Appendix C (starting at line 619-620, plus figures C1-5) - accompanied by an updated video supplement. We also expanded the Discussion accordingly (lines 516-531). We finally point out that, under the bottom-upwards crystallization scenario (implied by our initial layered setup), a negligible fraction of fp and ca-pv cumulates are expected to crystallize in the lower mantle (e.g, see: Boukare' et al., 2015; Caracas et al., 2019; Naibiei et al. 2021). Even for 50% crystallization ('batch crystallization'), almost exclusively bm crystallizes (Caracas et al., 2019). HPEs would then remain in the melt that later crystallizes to become the upper mantle. In other words, for any MO crystallization scenario, the compatibility of HPE in fp and ca-pv does not matter (because they do not crystallize); it is the partitioning coefficient between bm and melt that matters. Later on, after the initial global overturn, the HPEs would be confined in the ambient mantle that circulates around the BEAMS (as predicted by our models), and BEAMS would not be internally heated (unlike the blobs modeled in Becker et al., 1999).

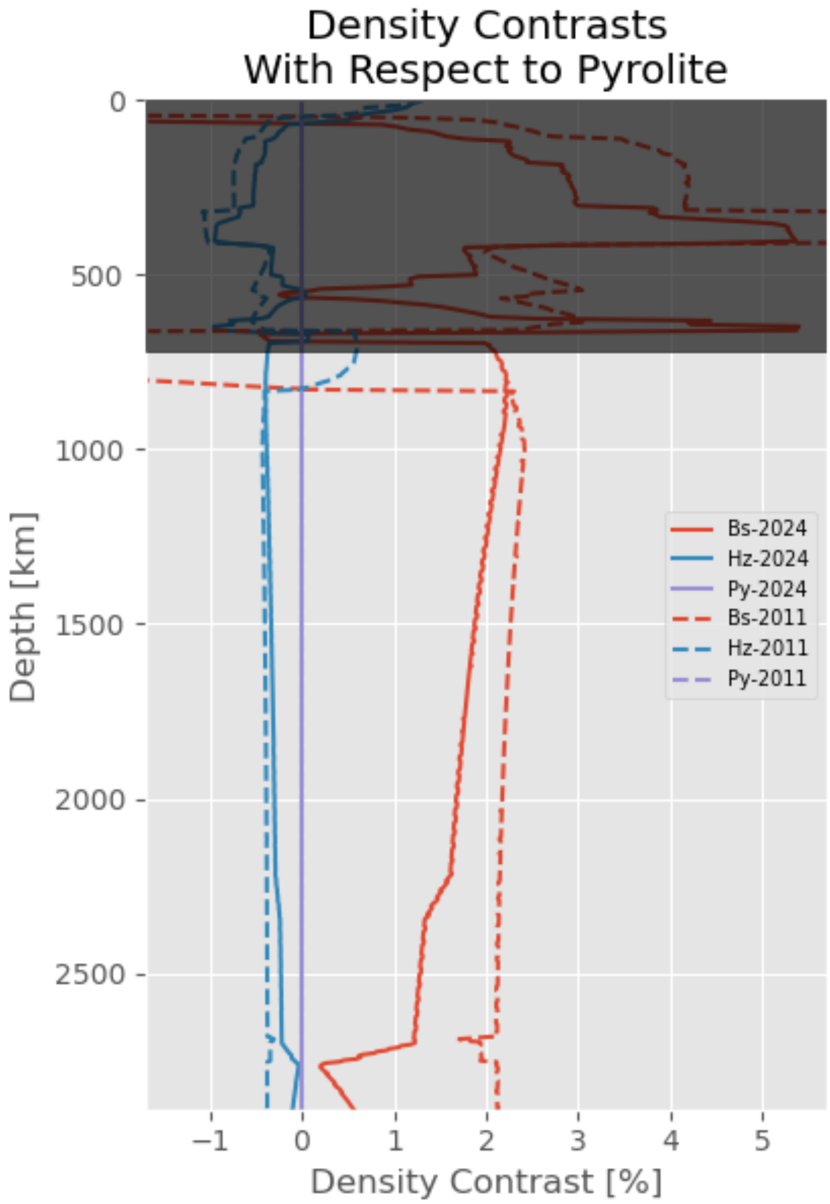
4) Line 130: The treatment of basalt/eclogite buoyancy in the deep mantle is simplified and the dynamics may be quite different if the authors used thermodynamic lookup tables for pyrolytic and basaltic compositions from Stixrude and Lithgow Bertelloni (2024)

We added lines 381-385 to the manuscript to discuss this point.

In addition, we would like to emphasize that we explored density contrasts over a wide range of values, which encompass experimental uncertainties, including the range of thermodynamic databases such as *Stixrude and Lithgow Bertelloni (2011)* as well as *Stixrude and Lithgow Bertelloni (2024)*.

The figure below shows density differences with respect to pyrolite computed using the models of *Stixrude and Lithgow Bertelloni (2011-2024)* - as indicated by the legend -, computed along isentropic profiles with potential temperature 1600K, using Perple_X (Connolly 2005). Note that the scale on the x-axis is dln_rho in %, thereby roughly marking the range of values explored in this study. While the density contrasts obtained in the mid-mantle (~2%) are well within the parameter space explored in our study, the density anomaly systematically decreases towards the CMB for *Stixrude and Lithgow Bertelloni (2024)*: this is a different behaviour than what is used in our study, and may be due to the incorporation of more recent datapoints for compressibility in the database. This difference may change our results in terms of the style and location of Bs segregation. In terms of Bs stability, our case with dln_rho=1% might be most consistent with *Stixrude and Lithgow Bertelloni (2024)*. Even if in this case no large thermochemical piles may be formed from recycled crust, we emphasize that our results with thermochemical piles would still be largely applicable, as long as the piles are formed by another mechanism not included in our models (e.g., BMO cumulates) - see new lines 381-385.

However, we also point out that thermodynamic databases such as the ones by Stixrude and Lithgow-Bertelloni are compilations of experimental and theoretical results that come with their own set of assumptions (and uncertainties, on the order of 2% for densities in the lower mantle, see e.g. Connolly and Khan 2016): indeed, using their model from 2011, we obtain profiles with shallower gradients with depth (see figure). Also, we highlight that the previous geodynamic studies that explored BEAMS (e.g. Gulcher et al 2021) preservation used density profiles designed to match the PREM model (Dziewonski and Anderson, 1981), similar to Tackley et al., 2013: we chose to keep these assumptions in our reference model for consistency with those studies.



5) Section 2.4: The temperature dependence of effective Clapeyron slopes is not considered. I wonder if the authors might at least comment on this? The equations of state developed by Stixrude and Lithgow-Bertelloni (2011, 2024) predict that the effective Clapeyron slope will change over time in a cooling mantle, which will affect the tendency towards layering and certainly the viability of BEAMS preservation.

We addressed this in a newly added paragraph in the Discussion (lines 533-537).

6) Line 170-175: These dynamics might be quite different if mantle heat production was included.

See newly added paragraph at the end of the Results section, newly added Appendix C (specifically lines 342-343, 626-630) and video supplement. Also see response to comment #3

7) Line 197: The mathematical symbol used here is showing up as both a greater than and less than sign, which I find confusing. Why not just define a threshold of f=0.3? Presumably the results are not very sensitive to the choice of this value?

We agree that the symbol is source of confusion (we use indeed a threshold of 0.3), we thank the reviewer for pointing that out. We changed lines 197-199 (see new lines 199-201). We also changed line 211 to: "sub-regimes MP and BP correspond to $\zeta <= 10$ and $\zeta > 10$ respectively" - see new line 213.

8) Figure 3: I found this figure extremely confusing. There's too much going on for me to understand quickly what's being shown and it would almost be easier to look at a data table – the opposite of what a figure is supposed to accomplish. I would find it clearer to have four separate panels with contour plots, and perhaps use the same color scale for all four panels to highlight trends. It's too hard for me to see the trends in the four different quantities on the same plot with four different symbols and four different color maps. Also, why abbreviate everything? The figure would be much more interpretable if the authors show the regime boundaries and give each region a nice simple label like 'Mixed Primordial' or 'deep ROC layer'. You could also annotate the axes – buoyancy ratio and viscosity ratio.

We thank the reviewer for their helpful comments of how to better convey our results. We recreated figure 3 as suggested.

9) 212: I think that radially-averaged depth profiles is redundant and maybe incorrect. I would call this the azimuthal average or horizontal average.

Agreed, we changed it to simply "depth profiles" (line 214).

10) 213: regime -> regimes

We fixed the typo (215).

11) Section 4.3: It would be nice for the authors to also address the preservation of primordial noble gas signatures in the OIB source. Are the authors arguing that the BEAMS are a likely source of primordial noble gas (He, Ne, Xe) isotopes? If so, is this at odds with the idea that BEAMS represent the early crystallization products since the noble gases are highly incompatible during solidification?

We expanded the discussion of this point (lines 455-465). Noble gases are often assumed to be incompatible, but whether this is indeed the case at lower-mantle coditions is still under debate. Indeed, there are few experiments in this regard carried at P/T conditions that are relevant for (B)MO crystallization in the lower mantle (Moreira 2013, Karato 2016) and available experimental results are discordant (e.g., Shcheka & Keppler 2012, Jackson 2021, who respectively report high and low Ar solubility in Bm). In general, we present BEAMS as a viable hypothesis for a primordial geochemical reservoir, albeit one that needs further testing via additional measurements of noble-gas solubilities/partition coefficients at high P and T. In the manuscript, we also posit an alternative pathway for BEAMS to host noble-gas signatures- if indeed these noble gases are highly incompatible- by considering interstial melt/melt inclusions in the (B)MO cumulates (see Jackson 2021).

13) 443: There is also a new global attenuation tomography paper (published while the present manuscript was in review) that shows an interesting feature around 1000 km depth: Sun et al. (2025) A high attenuation layer around 1000 km depth. Earth and Planetary Science Letters 669, 119577.

This an intriguing and relevant publication. We thank the reviewer for highlighting it. We now discuss it in the Discussion section (line 407-408).