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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.

Shijie Zhong (Referee)

shijie.zhong@colorado.edu

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General comments:

This is a comprehensive modeling study on mantle composition, structure, and mixing. The study includes >100 cases that cover a large parameter space on viscosity, buoyancy ratio and other parameters. This is a significant modeling effort, even though the models are done 2-D. The study based on the modeling results identifies two main regimes: chemical stratification (regime II) and partial heterogeneity preservation (regime III), and each regime has a number of sub-regimes. All the regimes and

sub-regimes have been proposed in one way or other previously mostly as conceptual models of the mantle, but it is instructive to see that the numerical models reproduce these regimes/sub-regimes for different model parameters. In particular, the study highlights a regime where recycled oceanic crust persists as large piles at the base of the mantle, while primordial material exists as blobs in the mid-mantle. The paper is generally well written, especially considering the complicated nature of the modeling. The study is a useful contribution to the ongoing discussion on mantle composition and structure. However, I do have some concerns that the authors should address.

Specific comments:

1) My main concern is about whether the 2-D models have sufficient numerical resolution to resolve the entrainment process that is so key to some of the model results reported here, e.g., the survival of the primordial materials and formation of the piles above the CMB. The 2-D models with fairly high Rayleigh number (∼10ˆ7) have ∼100 grid points in radial direction covering ∼3000 km thick mantle. As a comparison, Leng and Zhong (2011) showed that using adaptive mesh refinement method, mesh resolution corresponding to 256 elements in vertical direction is needed to reproduce the entrainment rate by Davaille’s analytical prediction for Ra=10ˆ5. We tried unsuccessfully for computing entrainment rate in models with variable viscosity. Zhong and Hager (2003) also used the marker chain method to determine the entrainment rate for a plume with fixed buoyancy on the top of a dense layer, demonstrating the requirement of numerical resolution. Van Keken et al., (1997) in their benchmark paper also showed the difficulty in computing entrainment rate. In Li et al. (2014), their 2-D models only cover the bottom of the mantle with super-high resolution (∼3 km?) to study the recycled crustal material interacting with the basal layer. Based on these studies, I have a good reason to believe that the models here (high Ra and variable viscosity) may have over-predicted the entrainment, but I do not know how much it would affect the overall conclusions. It would be very helpful for the authors to pick a case for a resolution test. To this end, I can see the need for at least two calculations using 200 and 400 grid

points in radial direction, given the high Ra and variable viscosity in their models (both making entrainment more difficult to compute).

2) The manuscript acknowledged in the final sub-section the drawback of ignoring the radioactive heating. For a study like the current one aiming at the long-term thermal and chemical evolution of the mantle, this seems to be a significant deficiency. I think that the authors should acknowledge it in the abstract and conclusions sections.

3) It would be helpful to clarify how the viscosity is computed. Equation (1) gives the viscosity for each composition through the pre-factor, lambda. I suppose that in solving the Stokes' equation, the viscosity is assigned to grid points. Then how are viscosities of different composition in a vicinity of a grid point (or within in a grid) averaged and assigned to the grid?

4) To follow point 3 above on viscosity, it seems that the manuscript focuses on the effect of lambda_prim, while not mentioning much on lambda_LM and lambda_ppv. Also, it appears that lambda_prim controls whether a convection is in a stagnant lid (for a small lambda_prim) regime or mobile-lid regime (for a large lambda_prim). Can the authors provide more insight or explanation as to why lambda_prim could have such an effect? Most previous studies show that for this type of models, large activation energy would lead to stagnant-lid convection, but the current study seems to suggest otherwise. It is unclear to me why lambda_prim has such a power. Along this line, does a large lambda_prim mean a more viscous blob (i.e., high viscosity blob)? Here a high radioactive heating for the primordial material may potentially make a big difference, as it would heat up the blob over time.

5) Regime I reported in Gulcher et al., (2020b) does not exist anymore in the current study, as stated in line 168, and can the authors explain why Regime I does not exist in the current models?

6) In Fig. 7, what prevents the primordial materials from entering the upper mantle? The phase change or compositional buoyancy or both? Some explanation would be

helpful.

7) Lines 441-443, " . . . ongoing debate about whether thermochemical piles are intrinsically stable . . .". More appropriate references may be McNamara and Zhong (2005, Nature) and Zhang et al., (2010, JGR).

Technical Comments:

1) Line 35, delete "to".

2) Line 178, Fig. 4a should 3a.

3) Fig. 3's caption, "regime I" or regime II? Fig. 4's caption, "regime II" or regime III? In general, the regime names are somewhat confusing. Another example is in lines 200 (the title of subsection 3.1.2) and 201: the sub-title in line 200 says "regime III", but line 201 says "2nd regime".

4) Fig. 4a, B=64 or 0.64?

5) Fig. 10, BEAMS? What does it stand for?

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