



EGUsphere, referee comment RC2
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Comment on egusphere-2022-418

Anonymous Referee #2

Referee comment on "Influence of heterogeneous thermal conductivity on the long-term evolution of the lower-mantle thermochemical structure: implications for primordial reservoirs" by Joshua Martin Guerrero et al., EGU sphere,
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In this paper the authors carried out numerical experiments of thermochemical mantle convection by varying the spatial changes in thermal conductivity to investigate the temporal changes in the distributions of dense "primordial" materials initially imposed above the bottom boundary. My honest impression is, unfortunately, that the manuscript is very hard to follow because of its poor organization and description from the reasons summarized below. I therefore strongly suggest that the authors should thoroughly revise the manuscript before the reconsideration.

1. One of my major criticism is that the main issues in this study were not well described either in the abstract or introduction section. In my understanding the major intention of the authors is to reduce the thermal conductivity in the deep mantle to much lower levels than those simply expected from the dependence on temperature and pressure (or depth), in order to induce an "instability" from the initial layer of dense materials within a sufficiently short period of time. The authors should clearly indicate their ultimate motivation in earlier parts of the manuscript.

2. I am not well convinced of the meaningfulness of the onset time of "instability" from the initial layer of dense materials. It seems to me that the authors had assumed that the deformation of the basal layer of dense materials occurs only in an "intrinsic" manner owing to the thermal buoyancy. However, several earlier studies had demonstrated the ultimate importance of "extrinsic" deformation induced by the cold subduction from the top surface. I do not therefore think that the onset time of "instability" can be a good measure to investigate the influence the thermal conductivity at depth on the thermal buoyancy in the basal layer of dense materials.

3. I felt quite odd with the authors' exaggerated claim on the overall profiles of thermal conductivity including "We find that the temperature- and depth- variations combined characterize the mean conductivity ratio from top-to-bottom" in the abstract. It is quite obvious from the assumptions made by the authors themselves.

4. In Section 2.1 the authors should state the reason why the phase change from perovskite (pv) to post-perovskite (ppv) is ignored in their numerical model. If they believe that the pv-ppv phase change is of little importance on the dynamic behaviors of the basal layers of dense materials, the authors should make clear the reason why.

5. I was quite disgusted to see that the profile given by equation (2) is denoted by

"KD=9.185". Such a denotation should be used only for the profiles given by equation (1) !!

6. Near equation (3) the authors should indicate the magnitude of the reduction in thermal conductivity due to the increase in temperature within the modeled domain by the choice of $n=0.5$ and $n=0.8$.

7. Near equation (4) and later, "compositional correction" should be rephrased with "compositional dependence". The word "CORRECTION" could imply that the numerical experiments without the compositional dependence in thermal conductivity are meaningless.

8. In Section 3.1 "QCMB" is used without explicitly defined.

9. To show the 2-D distributions of thermal conductivity in Figures 4 and 5, the authors should show the ratio of thermal conductivity to its surface value (K_S) rather than the values of conductivity itself.

10. The paper by Marzotto et al. (2020) has not been cited anywhere in the main text.

11. Near equation (8) of the Supplement, the spatial coordinates should not be in Cartesian (x,y,z) but in 2-D polar (r,ϕ) in this study. Similarly in equation (12) of the Supplement, the coordinate is not in 3-D polar (r,θ,ϕ).

12. In Section 3.1 of the Supplement, I think that the authors can use the potential temperature instead of the "adiabatic correction" $a(z)$.

13. The paper by Xu et al. (2004) has not been cited anywhere in the Supplement.