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Solid Earth Discussions

Interactive comment on "Influence of the Ringwoodite-Perovskite transition on mantle convection in spherical geometry as a function of Clapeyron slope and Rayleigh number" *by* M. Wolstencroft and J. H. Davies

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Author Response

We would like to thank the 2 anonymous referees for their comments and suggestions. We will address the points raised in order; Referee 1 followed by Referee 2. We reproduce the key section of the comment in bold type and respond below in regular type. Where both referees have raised essentially the same point it will be addressed in most detail at the first instance.

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Interactive Discussion

Discussion Paper

Referee 1

This is a nice systematic study of effect of the endothermic phase transition at 660 km depth on mantle convection, and should probably be published in some form. However, it is unfortunate that another group (Yanagisawa et al. 2010) undertook a similar parameter study at the same time with the same convection code, and published it earlier. This reduces the impact of the work, even though it is still useful to see results confirmed by a different group and the authors do include a good and fair discussion of the comparison with this and other work.

We focus on past mantle convective behavior. To do this the curves that define the different behaviours in the Ra/Cl600 space must be constrained as tightly as possible. In the work of Yanagisawa et al. 2010 the authors were considering the present magnitude of layering related to slab penetration into the lower mantle and thus were not as concerned with the profile of the curves. This is why our curves fit most of the points of Yanagisawa et al. (2010) even though they decided on different fitting curves. The precise equations are important when projecting to the high Ra numbers expected in the past - beyond what is currently practical to model. This was the focus of our study. Our curves are sufficiently different that projections to high Ra are quite different to those of Yanagisawa et al. 2010.

My suggestion for strengthening the paper would be to perform more simulations to actually map out the boundary of the episodic layering regime and characterize what behaviour occurs in the rest of the transitional regime. As the transitional regime is the one most relevant for the Earth, this would much increase the impact of the work.

This is an interesting point that we briefly touch upon in the manuscript. We felt that running the models required to constrain this domain is sufficiently computationally expensive and would require a degree of work such that it would represent a separate study.

The 76 cases in this study represent hundreds of thousands of CPU hours on some of the largest systems available to science (e.g. HECToR). Additionally, when considering such a narrow domain of behaviour, factors related to other physical parameters are likely to become important, further increasing complexity.

The thermal effects of latent heat of the transitions are ignored, consistent with using a Boussinesq approximation in the Stokes equations, which also ignores adiabatic and viscous heating. However, this point is not discussed until section 4.1. This approximation needs to be motivated in the method section.

This information on the latent heat will be moved to the methods section.

Similarly, the experimental constraints on the Clapeyron slopes of the ringwoodite to perovskite+magnesiowüstite transition and olivine to wadsleyite transitions are only mentioned in the discussion. These constraints should be used to motivate the range of Clapeyron slopes studied, especially since the '660' slopes start at the higher end of the experimental constraints and the '410' slope chosen falls below the experimental range.

The literature values for the respective Clapeyron slopes will be moved into the Introduction section to clarify the origin of the values used in the study.

The figures could be clearer. Symbols are quite small and different dashed line styles are similar. And figures 3-6 could probably be condensed into a maximum of 2 figures.

Symbols will be enlarged and line styles chosen to enhance clarity.

We agree that Figure 4 represents duplication and it will be eliminated, with the curves added to figure 3.

We are not convinced that reducing to 2 figures would necessarily make the figures clearer. There is a large amount of information condensed into these figures. The points in figure 5 (from Yanagisawa et al. 2010) could be plotted on Fig 3 but we feel this would confuse the presentation of our results and make the figure unnecessarily busy.

We feel that Figure 6 is required to illustrate our discussion and to allow readers from outside the immediate mantle modelling field to understand the potential implications of our result for Earth evolution.

Referee 2

Many of the parameter setting is in common with Yanagisawa et al., but the treatment of internal heating Rayleigh number is different. In the present study Rayleigh number ratio Ra/Rah (basal heating Rayleigh number/internal heating Rayleigh number) is fixed to be 0.054, while Yanagisawa et al. decreased this value from 0.1 to 0.03 with the increase of Ra. It means that the proportion of basal heat flow for the surface (total) heat flow is set to be nearly constant. In the present study, the relative contribution of basal heating increases for higher Ra cases. I think that each of the treatment for Rah has validity. The difference of Ra/Rah may cause a slight difference between these two studies, for the behavior of the flow in low Rayleigh number region. The authors should note this difference of parameter setting.

A comment to this effect will be added to the text.

The authors should explain further details on criterion of regime classification, especially the boundary between the whole layer and transitional, in relation to the last sentence in 2 Methods. Transitional case may be most important for the application of the Earth. If the authors can provide statistical information relating to the reduction of radial mass flux with negative Clapeyron slope, the value of this paper may be much enhanced.

This comment has highlighted to us that we need to increase the level of detail provided in the methods section related to the classification (final paragraph of methods).

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Identification of the convective regime is a somewhat complex matter as many model cases show time dependent behaviour even once they have evolved beyond the influence of the initial condition. Thus the process of classifying a given model case involves a number of analysis steps.

Cases were classified as follows:

1. Time evolution of the case was plotted (Surface (SHF) and basal heat flux over time) as the cases contain no evolution in heat input these values will show a 'flattening' when balance is achieved between heat input and heat output. Cases were run until this balance was achieved. The transitional class of cases do not generally show flattening but tend to oscillate about a particular SHF value instead.

2. The total absolute radial mass flux across the entire mantle was plotted.

3. If the mass flux at 660 km depth was approx. 90% or greater of the maximum mass flux of the model, the case was classified whole mantle convection

4. If the mass flux at 660 km depth was approx. 10% or lower than the maximum mass flux the case was classified as layered.

5. If the case fell between these classifications and demonstrated significant periodic variability in surface heat flux it was classified as transitional.

6. Additionally, the cases were visualised in 3D using isosurfaces, cross sections and specific depth surfaces. These plots allow judgements to be made as to what proportion of up or down welling features penetrate the phase boundary and what the nature of the coupling is across the boundary e.g. thermal or viscous. This final step was important for correctly classifying boundary cases where summary data values were inconclusive.

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The description above will be included in the methods section as part of a clarified final paragraph with reference to Figure 2.

As the referee 1 pointed out, the information in figures for regime diagram are overlapping. Figure 4 and 5 can be arranged in one figure by using bolder or different types of lines for the boundaries.

Please see also our response to Referee 1. We will reduce our figure count by 1 but feel that we should not plot our results on the same graph as those of Yanagisawa et al. 2010.

Additional Comments

Several other comments have been made to us and we will make the following additions to the text:

In the Discussion:

The range of Ra and particularly P is well beyond most of the boundaries of likely values for Earth. Our reasons for doing this were to be able to better constrain the boundaries of the various convective domains. However, these values may have application when considering other materials (such as water ice) in other bodies such as ice planets.

Gamma (p.717, l.13) is mentioned before explained, this will be corrected.

The caption of Figure 2 will be clarified.

Interactive comment on Solid Earth Discuss., 3, 713, 2011.