

## ***Interactive comment on “On the thermal gradient in the Earth’s deep interior” by M. Tirone***

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I am truly grateful to reviewer#1 for taking the time to review the manuscript. All the comments are significant and I'm gladly responding in some detail here. But first a couple of important remarks. In a previous post I mentioned a mistake labeling the x-axis P/P<sub>g</sub> (fig.2 and fig.3). The new figures now correctly show P/P<sub>g</sub>. Instead of renaming the labels, the plot has been recalculated to match the label. In this way the plots show the parameter "zeta" discussed in the manuscript. The comments of the reviewer prompted me to look at the data used to make the figures in the manuscript and I noticed that in fig.3 I mistakenly taken the data file from another simulation to make the upper right panel plot of T vs depth along the plume axis. The new plot shows the correct results. I apologize for the gross error.

Together with this reply I am also posting the new version of the manuscript (revised

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text, new color figs etc).

Here below the specific answers to the reviewer's comments.

2D convective simulation.

1. Yes indeed viscous dissipation would have an effect on the thermal boundary layers, however the reversible and irreversible effects have been computed in a second stage after running the convective mantle model. The dynamic model serves the only purpose to determine the velocity field and pressure variation with depth that are subsequently used to compute the JT irreversible effect and the viscous dissipation effect. Clearly there would be a feedback between these thermal contributions and pressure and velocity in the dynamic model, but for this first example here they have been decoupled evaluating them only after the dynamic simulation. This point I believe has been (briefly) discussed on line 7-13 section 3 (old version of the manuscript). For completeness I am attaching to this reply a plot of the thermal profiles (upwelling and downwelling) from the convective model showing the thermal boundary layer and the (almost) isothermal gradient in the convective area (fig.R1).

2. I am not a big fan of color figures (sometimes they are used to impress not to communicate), but I respect the point of view of the reviewer, new version of the manuscript with new color figures.

3. My apologies for the mismatch of the temperature units, the revised version only includes degree Celsius. The statement that the temperature at the base of the convection model is 3500K is not correct, the true temperature for the model shown in figure 2 is 3600K or 3327C. This can be seen also in fig.R1 included with this reply. The irreversible effect discussed above (point 1.) is simply added to the thermal profiles shown in fig.R1 in the convective region. This was done for upwelling starting at z~2970km (T~2700C) and for downwelling starting at 200 km depth (T~1200C).

For the computation of the mantle geotherm (and plume temperature)

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1. It is true that the bulk composition for all the computations is missing. I have added this information in the manuscript. The bulk composition in the MgO-FeO-SiO<sub>2</sub> system is MgO=44.72, FeO=9.48, SiO<sub>2</sub>=45.8 (wt%) which approximately reproduces a peridotite, as reported in Saxena's paper (GCA, 1996). It is also true that little information is given on the choice of the bottom temperature for the plume model (fig.3 in the manuscript) and the mantle geotherms (fig.4). At the base of the plume model, the temperature of 2827C (or 3100K) is chosen according to an on-going study unpublished yet. The choice is made based on the consideration that plume temperature in the upper mantle has to be consistent with our understanding of the depth and temperature of plume melting (intersection of the thermal gradient with the peridotite solidus temperature). Regarding the starting temperature of the geotherms, fig.4 in the manuscript (1150C or 1425K for downwelling and 2727C or 3000K for upwelling \*\*note\*\* upwelling temperature has been revised), the downwelling temperature is slightly lower than the temperature chosen in the mantle convection model. In general the downwelling temperature is a bit an undefined quantity when it is referred to a subducting slab. Clearly the slab temperature varies from top to bottom depending on many factors, therefore there is an arbitrary component in the choice of only one thermal profile for illustration purposes. As for the upwelling temperature in fig.4, it has been lowered by 100C in comparison to the earlier version of the manuscript to cover a mantle temperature range that may be consistent with peridotite/plume melting temperature. A brief explanation on the rationale for the chosen temperatures in the various sections has been added in the manuscript.

2. It is shown in fig.2 and fig.3 that the effect of viscous dissipation may be significant in plumes and in general upwelling and downwelling regions. It is beyond the scope of the paper to make general conclusions on the effect of viscous dissipation on mantle convection. As stated by the reviewer in the general comments, the main purpose of this work is to assess the validity of the JT formulation to describe the irreversible thermal effect that contributes to the thermal gradient in certain dynamic regions. To know more on the relation between mantle convection and viscous dissipation, see for

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example "Mantle Convection in the Earth and Planets", Schubert, Turcotte & Olson, 2001.

3. I am not quite sure I completely understand the point made by the reviewer. If I guess correctly the question is why in fig.2 the adiabatic (reversible) thermal change for downwelling is only ~600C (from ~1200C to ~1800C)? The short answer is that it depends on how the properties that define the isentropic gradient (thermal expansion,  $C_p$  and density) change as a function of P and T (bulk composition is fixed). One observation that can be made is that the combined pressure effect on these properties and the isentropic thermal gradient changes with temperature. In fact one can see that for upwelling the isentropic thermal change top to bottom is ~900C (~3000-200 km depth range) in the convection model and ~950C (3000-300 km depth range) for the plume model. Regarding the mentioned work of Stixrude and Lithgow-Bertelloni (I believe the full reference is "Thermodynamics of mantle minerals - II. Phase equilibria", GJI 184, 1180-1213, 2011), the reviewer probably asked for a comparison with fig.15 in the aforementioned paper. It is easy to see that the isentropic thermal change in fig.15 varies quite substantially depending on the starting point (commonly referred as potential temperature) ranging from 2000-3450K to 1000-1700K. There is no indication how these thermal gradients have been computed in this paper, my guess is that the most likely reason for the variations of the thermal change is again related to how the thermal properties defined in their study vary with P and T.

4. About the "zeta" value in fig.4. I am commenting on the new plots based on the correct panels showing  $P/P_g$  in fig.2. and fig.3 (see remark at the beginning of this reply). In the new fig.4 I removed the plot with "zeta" = 1.03 and added the plot with "zeta" = 1.005. It is true that the two example models (mantle convection and plume) suggest that the ratio  $P/P_g$  for upwelling is hardly greater than 1.01. However in my mind there are two reasons to show in fig.4 thermal gradients computed with "zeta" greater than 1.01. First for extrapolation or interpolation purposes we need to have a sense on how "zeta" affects the thermal variation, and to accomplish that, at least

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few thermal plots are needed. The second reason is that the two dynamic models presented in this study by no mean cover all possible conditions that are relevant to understand mantle dynamics, and the parameters that have been used to compute these models are far from definitive. If this wasn't true, there would be no need for geodynamic studies anymore!

5. Regarding the excess temperature and "potential temperature" perhaps a clarification is required. The excess temperature in the upper mantle as inferred from Schilling and others (200-300K) represents (reasonably well in my opinion) the increase of the temperature with respect to the ambient mantle temperature at the same depth/pressure. Some people (e.g. Putirka et al, "Ambient and excess mantle temperatures, olivine thermometry, and active vs. passive upwelling" *Chemical Geology*, 241, 177-206, 2007) defines it as the difference between the potential temperature of the plume and the potential temperature of sub-mid-ocean ridge (MOR). Regardless of the definition, in this study I really never discussed the concept of excess temperature because I never considered a reference temperature that is needed in order to compute the excess temperature. In the lower mantle the excess temperature has been usually considered in a different way, that is the temperature increase across the thermal boundary layer at the CMB as the result of the mismatch of the \*estimated\* temperature on the core side and the \*estimated\* adiabatic gradient at the bottom of the lower mantle. Once more the concept of excess temperature in the lower mantle does not concern this study because a discussion on the thermal boundary layer or the CMB temperature is beyond the scope of this study, which mainly aims to show that the JT irreversible formulation gives comparable results to the full scale dynamic thermal model. I am not sure what the review means with "A large "potential temperature" of the upwellings as predicted here (1800C or larger)". If that refers to the projection of the reversible adiabatic gradient to the surface, none of the models show a potential temperature of 1800C. In general I think that if we accept that the irreversible contribution to the thermal gradient is not insignificant, then we should start considering to move away from the concept of potential temperature as I also summarized in the final

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section of the manuscript.

6. In the plume model the transition ppv to ppv+pv occurs at ~2850km depth ( $P \sim 1320$  kbar),  $T = 2750$ C. These conditions seem quite reasonable and within the uncertainty of the pv-ppv transition (see for example, fig.7 in Andrault et al, "Experimental evidence for perovskite and post-perovskite coexistence throughout the whole D" region", *EPSL* 293, 90-96, 2010; fig.4 in Shim "The Postperovskite Transition", *Ann. Rev. Earth Plan. Sci.* 36, 569-599, 2008; fig.3 in Shieh et al "Equation of state of the postperovskite phase synthesized from a natural (Mg,Fe)SiO<sub>3</sub> orthopyroxene", *PNAS* 103, 3039-3043, 2006).

As for the transition temperature, the exothermic reaction is generally associated to a large Clapeyron slope (6-13 MPa/K) (e.g. Post-Perovskite: The Last Mantle Phase Transition, Hirose et al. (Eds.), *Geophysical Monograph* 174, 2007) and recently even greater than 13 MPa/K (Li et al, Effects of the post-perovskite phase transition properties on the stability and structure of primordial reservoirs in the lower mantle of the Earth, *EPSL* 432, 1-12, 2015), at least until they'll change it again. For geodynamic implications of the pv-ppv transition, see for example Matyska and Yuen, "Lower mantle dynamics with the post-perovskite phase change, radiative thermal conductivity, temperature and depth-dependent viscosity", *Pepi*, 154, 196-207, 2006, or Cizkova et al, "Implications of post-perovskite transport properties for core-mantle dynamics", *Pepi*, 180, 235-243, 2009. The Clapeyron slope can be used to estimate  $\Delta T$ . The temperature change has been estimated to be as great as 290 K by Spera et al. (pag.242 in Spera et al "Tradeoffs in chemical and thermal variations in the post-perovskite phase transition: Mixed phase regions in the deep lower mantle?", *PEPI* 159, 234-246, 2006), however smaller value is shown in fig. 15 of the paper by Stixrude and Lithgow-Bertelloni mentioned earlier. The Clapeyron slope in the model of this study at the P,T conditions of fig.3 should be ~12 Mpa/K (just my quick calculation from the thermodynamic database). I have included in the manuscript (section 4) the missing thermodynamic data related to Mg-ppv and Fe-ppv. But I'd like keep this discussion on

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ppv within this reply (which is online anyway) rather than in the manuscript since the pv-ppv transition does not directly concern the main objective of the study.

7. About the assumption on  $C_p$  vs pressure, I think we are on the same page here. My attempt was to decrease  $C_p$  with pressure towards the value defined by the Dulong Petit limit at some high pressure, which is what the reviewer correctly predicted from re-evaluating the Mie-Debye-Gruneisen model (thanks for taking the time to look again into the MDG model!). The statement in section 5, line 16-24 (old version of the manuscript) is all about this issue, if it is not clear, please let me know.

Technical comments,

1. Yes the problem with the temperature units is acknowledged and fixed!
2. Plots of  $P/P_g$  have been replaced with the "real" ones.
3. Figures with colors are good (sometimes).
4. Yes it was an unwanted odd selection of ticks spacing, fixed!
5. Thanks!
6. Thanks!
7. Agreed.
8. Agreed.

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/7/C1382/2015/sed-7-C1382-2015-supplement.pdf>

Interactive comment on Solid Earth Discuss., 7, 2501, 2015.

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Fig.R1

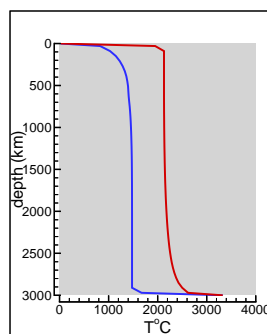


Fig. 1. fig.R1

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