

## ***Interactive comment on “Effective buoyancy ratio: a new parameter to characterize thermo-chemical mixing in the Earth’s mantle” by A. Galsa et al.***

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Here the authors present a series of calculations of mantle convection starting with a dense layer at the base, and propose a new diagnostic quantity – the effective buoyancy ratio – for determining when the layer becomes unstable and overturns/mixes into the mantle above. The basic setup is not new – there have been a number of laboratory and numerical studies on the long-term stability of a dense basal layer (basically it heats up while at the same time its compositional buoyancy decreases until they potentially become equal and it overturns), but as far as I am aware nobody has examined the behaviour in terms of this effective buoyancy ratio. The results are also not very surprising – they basically show that when the positive thermal buoyancy of the (hot) layer becomes equal to the negative compositional buoyancy, then it is unsta-

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ble to overturn. Nevertheless, it’s good to see that this new diagnostic works, the study is systematically done, it’s interesting to see the relationship between time to overturn and buoyancy ratio (Figure 4), so overall I recommend acceptance after several things have been addressed.

There are some limitations of the current study and other things that should be addressed/discussed.

1. The effective buoyancy ratio is used in a diagnostic manner but not a predictive manner. That is, it is used to understand why the layer has gone unstable, but not to predict layer longevity in advance. This study would be much more powerful if the authors also developed a parameterised convection model that predicts when the layer goes unstable, then compare it to the full numerical model and use this for calibration of uncertain constants etc.

2. The initial condition is not realistic for Earth evolution (although it has been used in the past). In this study a dense layer is suddenly placed on top of preexisting whole mantle thermal convection, and therefore starts off “cold” (or at least, the same  $T$  as the layer above). In contrast, the Earth started off hot, with the cooling/solidification of a magma ocean that might have resulted in the assumed dense layer. Another endmember would be to start off with the layer being hot. Such a condition could be generated by forcing layering by running convection including an impermeable boundary 300 km above the CMB (ie  $v_r=0$  but continuity of shear stress and tangential velocity) and running until a statistically steady-state.

3. They need to discuss how various quantities are defined and calculated. In particular: (i) How is the effective  $\Delta_c$  calculated? After a while the dense layer has a radial gradient as well as lateral variations in thickness so  $\Delta_c$  varies with position. What is the mathematical definition of  $\Delta_c$  that is used? (ii) A similar question for  $\Delta_T$ . (iii)  $q_{DC}$  – the mass flux of dense material. How is this measured? (iv) Likewise  $q_D$  – the heat flow at top of dense layer – how is this measured? (first it is necessary to find

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the top of the dense layer at each azimuth). (v) The TCC model: need to give precise details so that others can reproduce it if they wish!

4. Numerical issues. The rate of entrainment of layers into each other is very sensitive to the numerical scheme, resolution etc. For example see Figures 6, 7, 10 in (Tackley and King, 2003) as well as (van Keken et al., 1997). In particular, grid-based methods (as used in this study) suffer from numerical diffusion. So, how good is Comsol at treating non-diffusive fields/interfaces? The simulations develop a region of intermediate composition in the top part of the layer as well as gradual entrainment of material above (Figure 3). How much of this is physical and how much is due to numerical diffusion? I think they need to add some tests in which results using different resolutions, and if available different advection schemes (e.g. a particle-based method, or field based including the Lenardic filter (Lenardic and Kaula, 1993)) are compared to each other, to get an idea how quantitatively robust the observed timescales are. This could be done in supplementary material.

5. Q – the internal heating rate - is not given in Table 1. Does this mean it is zero? If so, I would comment that purely basal-heated convection both magnifies the importance of a basal dense layer and is not realistic for the Earth, which is thought to be primarily driven by internal heating + secular cooling. It would be good to see cases with some internal heating.

6. Paper structure: They introduce a new model (the TCC complex model) in the “discussion & conclusions” section – this really belongs in the “results” section.

Minor points:

(i) Line 18: p-wave stands for primary wave not pressure wave.

(ii) The Figure 1 caption needs to list what all of the graphs on the left plot.

(iii) Another study highly relevant in this context is (Gonnermann et al., 2002).

An explanation of my overall assessment: Scientific significance: good (old problem but

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new analysis; could be made excellent with addition of a semi-analytical parameterised model) Scientific quality: fair (need more discussion of various points as discussed below). Presentation quality: good.

References:

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