Second review for: "Timescales of chemical equilibrium between the convecting solid mantle and over-/underlying magma oceans" by Bolrão *et al*.

General comments

While most points I raised about presentation and explanations have been satisfactorily taken into consideration, my concern on the effect of chemical buoyancy could not be addressed due to technical issues that I can appreciate. This is unfortunate because the article would clearly benefit from including the buoyancy number in its parameter space. However the results on the timescale of equilibration between mantle and magma ocean(s) are still valuable, but the discussion should elaborate more on the the composition of the magma ocean (see specific comments). The paragraph which has been added about the effect of chemical buoyancy is not entirely satisfactory in my opinion, and could point out the limitation of having B=1 rather than trying to convince the reader that the effect of this parameter is not important, relying on (what I found) a shaky argument. Finally, I am also puzzled by the use of the term "chemical equilibrium", which, in its common understanding, is something very different from what is dealt with here. I would suggest "mass" or "composition" equilibrium, which doesn't have as strong an underlying meaning.

Specific comments

L 46-47: *Such overturn(s) may lead to re-melting of FeO-enriched material at depth, as the isentrope of such material is steeper than its melting curve through most of the mantle.*

This needs to be supported by a citation. Actually in Labrosse et al., 2015, this mechanism is indeed suggested to produce Fe-rich melts, but not because the isentrope of the Fe-rich material is steeper than the melting curve (which, incidentally, would hinder thermal overturn). Possible re-melting is rather attributed to viscous heating of material already close to their solidus.

L 110-113: *Regarding the buoyancy number, although earth like models point to a value of B∼3.0 today, the value in the early Earth might be different. In this paper we make a conservative choice and consider a buoyancy number of B = 1.0, to attribute similar weight to compositional and thermal effects on the density.*

Here a short discussion on what B could be in the early Earth (at least whether it might be higher or lower) would be relevant. Besides, I don't really understand in which sense the choice of B=1 is "conservative". A value higher than the expected one (2.8 based a the present-day Earth mantle) would be conservative in the sense that one would be sure not to underestimate the effect of chemical buoyancy.
Furthermore, since the X^{S}_{FeO} cannot reach high values (in particular it cannot be higher than $X^{MO}_{FeO,}$ which is lower than ~0.25 in the cases presented in Figure 3), the available density contrast in the mantle is reduced, so taking B=1 attributes an effective weight to compositional effects that is significantly lower than the thermal ones (the total temperature range being ensured to be covered due to the boundary conditions).

L 177: *We calculate the influx of mass corresponding to this dynamic topography, and distribute this mass by the new tracers*

How do you relate the number of tracers to the inflowing mass? Based on their initial density (i.e. number of tracers per cell)? Or do you balance the number of tracers lost at melting boundaries to that of tracers gained at crystallizing boundaries?

L 208-287:

In parts 3.1 and 3.2 you present results in terms of time for half equilibrium for cases having both a 2390-km-thick mantle (either TMO or BMO) and a 2290-km-thick mantle (both BMO and TMO). The time normalization is different for those two cases (one non-dimensional unit time for the former is about 9% longer than one non-dimensional unit time for the later). Since you compare results from both cases and use them to fit the scaling law, they would probably need to be normalized to a common scale; are they? Since the result exhibits variations in orders of magnitude for the tie of half equilibrium, I don't expect the conclusion to change.

L 396 *but this effect would be strongly diminished for a more realistic initial condition.*
This effect could even be reversed if the TMO is sufficiently enriched compared to the mantle.

L 400-402: *Thus, the actual value considered for B is expected to have only a minor effect on the equilibration timescales constrained here.*

I am not entirely convinced by this reasoning. What you observe is that, for B=1, magma ocean(s) and mantle equilibrate swiftly, diminishing the influence of chemical buoyancy. However, in order to conclude that the effect of B is minor, you have to assume that equilibration between magma ocean(s) and mantle will be fast regardless of the value of B, which cannot be justified based on the present work.

Here are a couple of ideas to extend the discussion:

Your results seem to suggest that the magma ocean crystallizes in the equilibrium crystallization regime (where it is in equilibrium with the whole mantle at all times). In this case, you could easily compute the evolution of X^{MO}_{FeO} and the crystallizing material's X_{FeO} as a function of the magma ocean's volume and be more quantitative about the magma ocean's enrichment. Moreover, you could do the same for lower partition coefficient and extend the discussion to other species (e.g. volatiles: how do your results compare with the assumptions for volatiles partitioning from Lebrun *et al.*, 2013 and the other studies?).

I would also suggest, for the available simulations, to study the time evolution of X_{FeO} in the crystallizing material (I guess it can be easily derived from the value of X^{MO}_{FeO}) and to compare it to the average value in the mantle. This could help discussing the effective chemical density contrast.

Technical corrections

L 271-272: *One possible explanation for this is that at low values of Φ, convection occurs with low degree, so the geometry of the problem is less important.*

Is it a "possible explanation" for the previous statement ("this conclusion is independent of the evolution scenario") or for the weak influence of the solid mantle's volume?

L 349: *it is conceivable that a thermally-coupled TMO and BMO crystallise more slowly than expected*

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L 394: *(i.e., a more realistic initial condition at least for the TMO)*
I don't understand what you mean here.