

# ***Interactive comment on “Timescales of chemical equilibrium between the convecting solid mantle and over-/underlying magma oceans” by Daniela Paz Bolrão et al.***

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### Response to the Reviewer #2

We would like to thank the Reviewer #2 for taking the time to review our manuscript so thoughtfully. We took the useful comments and pertinent questions into consideration, which have substantially improved our paper. Reviewer #2 addresses an important point regarding the value of the Buoyancy number,  $B$ . We agree that the value used in our simulations (1.0) is lower than a realistic value (3.0 or more), and we decided to show results for a more reasonable value of  $B$ . Therefore, at the moment we are trying to run new simulations with a proper value for  $B$ . Unfortunately, we don't have the new results yet since our clusters are still facing problems from a cyber attack that occurred late-May. We plan to have the results in the next couple of months.

Therefore, we are asking to the Editor if it possible to have an extension of two months, to address the  $B$  problem properly. The rest of our responses are reported below.

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### Specific comments

**Reviewer Point 1:** Thermo-chemical convection: The number of buoyancy is set to 1 without further explanation. Why was this value taken and how does the choice of  $B$  influence the results? With the present values it seems that the chemical

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density variation has no significant influence.

→ **Authors:** Indeed the value of  $B$  we use is lower than what is expected for Earth. We did not vary the value of  $B$  because our parameter space was already too vast. However, we agree that it is probably better to show results for a more reasonable value of  $B$ . Therefore, at the moment we are trying to run new simulations with  $B = 3.0$  (this value comes from  $B = \frac{\Delta\rho_{ch}}{\alpha\Delta T\rho_0} = \frac{670}{0.00002 \times 3000 \times 4000} = 2.8$ ). Unfortunately, we don't have the new results yet. Our clusters are still facing problems from a cyber attack that occurred late-May. We plan to have the results in the next couple of months.

**Reviewer Point 2:** In the initial setup a homogeneous FeO content of solid mantle and magma ocean is assumed. As the authors themselves write, this is not a realistic initial state but it also not clear how sensible are the obtained time scales depending on initial conditions? In the current setup, the material that forms in the topography depression is depleted in FeO, this would not be the case for a more realistic start condition. An initial unstable gradient in the solid mantle can trigger convection but may also result is a stable configuration after overturn, depending on  $B$  (see above). This can be important for the time scale of chemical equilibrium - if in this case a chemical equilibrium can be established.

→ **Authors:** This is a good point. In the new simulations that are currently running with  $B = 3$ , we also use a more realistic initial condition which leads to material enriched in FeO in newly formed solid. Overall, it is difficult to fully assess how the initial condition would affect results, especially as this makes the parameter space much larger. We chose here to focus on the consequences of the phase change boundary condition to show it is an important ingredient when considering the chemical evolution of our system. We do agree that it would be important to study the effect of the initial condition, but we think it is out of scope of this paper, and better suited in more

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realistic studies about the long term evolution of the solid/magma ocean(s) system.

**Reviewer Point 3:** Two effects have been neglected, but they can also result in chemical equilibration and compositional mixing before final magma ocean crystallization: 1) When the solid mantle grows and has no fixed boundaries, as is assumed here, convection causes the new top crystallized layer, which should have a different FeO content, to sink and mix continuously with the solid mantle. 2) If convection in the solid mantle starts before the solidification of MO, partial melting of the cumulates and ‘feeding’ of the MO with this melt is very likely. Both effects change the chemical equilibrium considerably and do not necessarily require the material to be able to flow through by phase change. However, the latter may further reduce the time scale of chemical equilibrium.

→ **Authors:** These are two important points and we modified the manuscript to address them. Regarding the first one: in this paper we assume fixed boundaries and test different thicknesses of the solid mantle (or magma oceans). Indeed a moving boundary would create a new top crystallised layer, which should have a different FeO content, and sink and mix continuously with the solid mantle. This effect could result in chemical equilibration and compositional mixing before final magma ocean crystallisation. However, we show here that with the phase change boundary condition, chemical equilibration can occur on a timescale that is much smaller than that of crystallization. Integrating the effect of the moving boundary is much more demanding computationally (an evolution model for magma oceans is necessary, as well as some way to deal with moving boundaries). This will be the subject of more complete studies in the future. Note also that in this study we take  $\Phi$  as being constant through time, but because this number depends on the dynamics and thicknesses of the magma oceans,  $\Phi$  may change continuously in a more realistic model with moving boundaries. Variations of  $\Phi$  and a

moving-boundary scheme should definitely be considered in further studies to study the long-term evolution of the solid/magma ocean(s) system.

Regarding point 2: in this study we focus our attention to a phase change boundary condition, that allows material to flow through the boundary and continuously change the composition of solid and liquid reservoirs. Partial melting of solid cumulates can indeed still change the composition of the magma ocean, without the use of this boundary condition. However, we show that with this boundary condition a larger volume of material can (re-)melt and crystallise efficiently at either or both solid-liquid phase boundaries.

**Reviewer Point 4:** Steady-state simulations, i.e.  $\Delta T$  is constant, but also no internal heat sources and a constant viscosity are used – all these effects can influence the strength of convection (and chemical equilibration) and possibly the convection pattern. In particular the influence of internal heat and a temperature dependent viscosity could be tested fast.

→ **Authors:** Indeed these effects can influence the results. But testing the effect of internal heat sources, as well as temperature dependent viscosity, goes beyond the scope of the current paper. These should be definitely taken into account in future projects.

**Reviewer Point 5:** A new phase change boundary condition for convection has been investigated, are there benchmark studies also for sufficient resolution or how do the authors ensure that the calculations are correct?

→ **Authors:** The phase change boundary condition implementation was tested against linear stability analysis (Agrusta et al., 2019 for cartesian geometry; Morison, 2019 spherical geometry). In this study we also check energy

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conservation over the solid mantle, and iron mass conservation over the whole mantle (solid mantle and magma oceans).

**Reviewer Point 6:** A table should be added for all parameters used.

→ **Authors:** Included.

**Reviewer Point 7:** Line 255: It is not clear to me why with small  $\Phi$  the volume of the solid mantle has no effect on the time scale of chemical equilibration. Do the authors have an explanation? I think this also indicates that the material flow is extremely large - is this really realistic? One could estimate the value.

→ **Authors:** Coefficients of the fitting equation indicate that at low values of  $\Phi$ , the ratio between volumes (i.e., the aspect ratio of the evolution scenario), has less impact than at high values of  $\Phi$ . One possible explanation for this is that at low values of  $\Phi$ , convection occurs with low degree, so the geometry of the problem is less important. We address this in the new version of the manuscript. Extremely large material flow is realistic, since  $\Phi^+ \sim 10^{-5}$  and  $\Phi^- \sim 10^{-3}$ , according to Morison et al. (2019) and Morison (2019), for a purely thermal case.

**Reviewer Point 8:** Line 310: In the discussion, the crustal dichotomies of Mars and the Moon are mentioned and associated with the present process. I don't find this so obvious, because according to the model low degree convection is postulated at the beginning of the MO crystallization, but the crustal dichotomy is more likely to occur at the end of the MO phase, when the pattern becomes small scale.

→ **Authors:** We modified the text to address this point.

**Reviewer Point 9:** Line 370: It is stated that smaller planets cool faster. This is not generally true, for example if a blanketing crust is formed during MO crystallization

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before the mantle is entirely solid and the cooling and crystallization of the MO slows down considerably - as postulated for the Moon.

→ **Authors:** This is a good point and we modified the text to address it. Indeed it is not clear what could happen to smaller planets than Earth. In these smaller planets, Ra number is lower, which could imply that equilibration may take longer. However, it also depends on the timescale of crystallisation of the magma ocean. On one hand, smaller planets tend to cool faster, as they contain a smaller total reservoir of heat (and volatiles), but on the other hand, if a blanketing crust is formed before the mantle is entirely crystallised, then the magma ocean would take longer to cool.

**Reviewer Point 10:** Line 380: “for realistic values for the phase change number  $\Phi^+$  smaller than  $\sim 100$ ”. I doubt that we really know the realistic value in view of the simplification of the process and the unknown parameters.

→ **Authors:** Indeed we don't know an exact value of  $\Phi$ . However, Morison et al. (2019) and Morison (2019) estimated  $\Phi^+ \sim 10^{-5}$  and  $\Phi^- \sim 10^{-3}$ , for a purely thermal case. So small values of  $\Phi$  are expected for the Earth. However, our paper includes a compositional treatment, and given that  $\Phi^\pm$  is difficult to constrain, we vary this value between  $10^{-1}$  and  $10^5$  (we use  $\Phi^\pm = 10^{-1}$  as the lowest value possible for  $\Phi^\pm$  because the resolution of the thermal boundary layer is computationally demanding once  $\Phi^\pm$  decreases below  $10^{-1}$ ). In this paper we compare our estimated timescales for chemical (half-)equilibration with timescales of magma ocean crystallisation given by Lebrun et al., 2013. We see that for values of  $\Phi$  lower than  $\sim 100$ , chemical (half-)equilibration occurs before magma ocean crystallisation (previous figure 8, now figure 7). Indeed the sentence “for realistic values for the phase change number  $\Phi^+$  smaller than  $\sim 100$ ” might lead to confusion. What we

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meant is that low values of  $\Phi$  are expected (Morison et al. (2019) and Morison (2019)), and we show that at any value below  $\Phi^{\pm} \sim 100$ , the system reaches chemical (half-)equilibration before magma ocean crystallisation. We changed this sentence to "Moreover, this efficient transfer of FeO renders the timescales of chemical (half-)equilibration between the solid mantle and magma ocean(s) shorter than (or on the order of) 1 Myr."

**Reviewer Point 11:** Figure 7 is difficult to read with the different symbols and lines.

→ **Authors:** We agree that figure 7 may be too complicated due to the amount of information. We remove figure 7 from this manuscript, since the main information is written in the text, and the good agreement of the scaling law with the predictions is made in figure A1.

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