Solid Earth

Discussions

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Interactive comment on "Chemical Heterogeneities in the Mantle: Progress Towards a General Quantitative Description" *by* Massimiliano Tirone

Tirone

max.tirone@gmail.com

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I am truly grateful to the reviewer for taking the time to review the manuscript. My apologies for not posting a reply earlier, I was hoping to prepare a comprehensive response to both reviews and a submit a revised version of the manuscript all in once.

Given the difficulty to find a second reviewer, I decided to post a preliminary reply before revising and re-submitting the manuscript. Hopefully it will foster a further discussion.

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On the comments of Referee#1:

Since the manuscript and the related work was done quite some time ago, I took the opportunity to read the abstract with fresh eyes and I have to agree with the reviewer that it may be helpful to make it more clear following the reviewer's feedback. This will be done in the revised version of the manuscript.

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As for the application of the model to geological problems, this is the main objective of this study, however in my opinion at this stage it is premature. The reason is that we need experimental data for two purposes: 1- validate the model in the manuscript, 2- put a constraint on the extent of the equilibration process. Without such experiments and a reliable model to quantify the experimental results, it would be extremely difficult to apply the study of the manuscript to real problems. But this was clear to me since the beginning, in fact the manuscript is part of a larger project described in a proposal submitted recently (Nov/08/18) to the ERC-2019-SyG, ERC Synergy Program, "GEO-DIVE: Experimenting and Modeling Chemical GEO-DIVErsities in the Solid Earth". The project specifically describes the acquisition of the set of experimental data that are needed to constrain the process. These would be the first experiments of this kind applied to a coupled system peridotite-eclogite/dunite at high P,T in a piston cylinder and multi-anvil press (a preliminary experiment has been already conducted some time ago, I have the data). The only similar study that I found is the one by Martin et al., although the system and the objectives were different (Martin, A.M., Laporte, D., Koga, K.T., Kawamoto, T., Hammouda, T., 2011. Experimental Study of the Stability of a DolomiteCoesite Assemblage in Contact With Peridotite: Implications for Sediment-Mantle Interaction and Diamond Formation During Subduction. J. Petrol., 0, 1-27, doi:10.1093/petrology/egr066).

I was aware of the work by Yaxley and Green, the problem is that in their experiments, melt seems to be present in all cases, which makes the interpretation of the results

more difficult and perhaps not relevant for the problem described in the manuscript (and in the project).

The studies by G. Pearson are very interesting. These are the type of data that I am planning to use (if the project gets funded). In fact one of the applications described in the proposal is the study of the thermo petrological evolution of cratons. For brevity I won't get into details here but I'll be happy to share privately with the reviewer the ERC-2019-SyG proposal where more info have been included.

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I'll keep polishing the text and try to make it as clear as possible, thanks for the feedback! Just a few comments here below:

1- the meaning of "bulk compositions of the two assemblages separately" and "weighted average of the bulk composition of the two assemblages in proportion 1:1" is actually quite simple. The manuscript will be revised to make this point more understandable. Let's say that in assemblage A there is 50% of a certain oxide, and in assemblage B the abundance of the same oxide is 40%. If the two assemblages are put together to create a combined assemblage in proportion 1:1, the abundance of this oxide in the combined assemblage will be 45%. If the proportion is for example 2:1, the oxide in the combined assemblage is 46.667%. Just to complete the description, 45% (1:1 proportion) or 46.667% (2:1 proportion) does not mean that the oxide % at every spatial location in the combined assemblage will be 45% or 46.667%. These % numbers are only the *spatial average* for the combined system. The oxide on some location will be, say 42% or 44% or some other % (determined by the model).

2- the end-member components can be negative (eg. Sp(Hc) in table 3). This is how Mark Ghiorso decided to describe the solid mixture of certain mineral phases, like spinel. It is strange but not wrong, because what matters is that the sum of each oxide in the mineral is greater than zero. For example in Table 3, first column, component Sp(Hc) = -0.0014341. Now let's consider Al2O3, since Al2O3 is only present in

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Sp(Hc, Fe2+Al2O4) and Sp(Spl, MgAl2O4) in the same proportion, as long as Sp(Spl) > Sp(Hc) the Al2O3 abundance in the mineral phase spinel will be greater than zero. This is indeed true in this case because Sp(Spl) = 0.0020765 > Sp(Hc).

3- in Figure 2, the zigzag patter of MgO in certain minerals is induced by a numerical error. The transport model determines the bulk oxide distribution in space and time, for example panel 2-B) shows the bulk Mgo, all good, no zigzag here. The next step is to use the bulk composition with the program AlphaMELTS to determine the equilibrium assemblage at every location (mineral abundance and composition). I believe that this is were the irregularities are created (numerically). I am not sure exactly about the reason. It could be that the abundance of certain mineral components is very small and not precisely determined by AlphaMELTS at the equilibrium point. I will investigate further and see if I can come up with a more precise answer, although I don't think I will be able to fix the problem, if it is truly related to AlphaMELTS.

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I'll be happy to provide further details or clarifications, there is still some time before the second review will materialize. In any case I promise to deliver a more prompt response!

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Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2018-67, 2018.

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