

May 12, 2016

Dear editor,

Please find attached our revised manuscript entitled *Hydrous melting and partitioning in and above the mantle transition zone: insights from water-rich MgO-SiO₂-H₂O experiments*, written for potential publication as an Article in *Geochimica et Cosmochimica Acta*.

We have addressed all of the comments made by the reviewers. I have also made some minor changes to the text to improve readability, and simplified some of the analysis so that it can be more easily reproduced by others. The simplification (using the intermediate composition melts from de Koker et al., rather than creating a solution model) has a negligible effect on the figures and conclusions in the paper.

Responses to editorial and reviewer comments

Line 28: could use a reference at the end of this sentence (e.g. Spiegelman).
Two references added

Line 33: could use a reference at the end of sentence about water budget
(e.g. Hirschmann)
Reference added

Line 60: I think you might want a better segue here into the approach you take in this study. Something like "Here we use the approach of bracketing the liquidus surface to determine melt composition as a function of temperature in the system MSH".
The end of the introduction has been rewritten, starting with this suggestion.

I found the introduction to be well written but in the end it fails to let the reader know exactly what the tack is and what the goals of this paper are, and even what system you will be working in and why. You could leave the business about how to make starting materials with high water contents until the methods, and end the introduction by telling the reader that you will use the liquidus phase relations in MSH to develop thermodynamic models for water solubility in melts and partitioning models, which you will apply to understanding hydrous melting at transition zone conditions.
I agree. I've rewritten this paragraph

Lines 70-75 (end of the introduction): Again, I agree with Mike that the end of the intro section is a bit abrupt and suddenly becomes too technical about the choice of starting mix water source. Such information should become part of the methods (starting mix) section.
The authors are better off ending the introduction by saying generally what they have done in this study.
Paragraph rewritten.

Line 78: "Starting compositions in the system MSH"
It also might be good to provide some explanation of the rationale

for choosing the starting compositions, i.e. "We chose compositions along the bounding binaries, and within the ternary in order to constrain phase relations"

Good suggestion. Detail added.

Line 105: Any idea about water loss in any of the experiments?
How much time do you have as a function of temperature, or is this intuition from experience?

Added reference to Novella et al. (2016), which has more details

Line 137: "concentrated hydrous melts". Presumably you mean water-rich.
Changed to water-bearing silicate melts

Line 193: typo, should be 'note' not 'notes'
Typo corrected

Line 203: I am probably just being thick, but here and other places you talk about "the experimentally determined value".
What value are you referring to and from what source?
These values are from Stixrude and Lithgow-Bertelloni (2011).
I cite the study for periclase (line 190), but have added another reference to the paper here.

Line 227: You might consider a new section here as currently the segue is a bit clunky.
Section split into endmember and solution model subsections

Line 269: Should read "between quench crystals and liquidus phases."
Corrected

Table 1: It took me a little while to get my head around the way the results are tabulated in Table 1, but in the end I think it works. You may want to add in the table caption that melts are present in all run products. There is also a typo in the caption "en-dash" should just be "dash".

Done

Line 276: The invariant is drawn at ~1250 C rather than 1200.
Also, why call it a fluid here I would stick with liquid. You may also want to clarify the key either by adding e.g. Brucite-L (or maybe br-L), etc.

Text changed to 1250C rather than 1200.

Reference to fluid changed to liquid. I haven't changed the key, as all legend entries implicitly include liquid. The addition to the caption of Table 1 should be sufficient (although I can change this if you think I should).

Line 304. I am not understanding the use of the term 'metastable' here. It may be a good idea as well to be clear that Fig. 4 is not a binary join, but is a T-X section depicting equilibrium crystallization. Actually, the figure is not drawn correctly based on Fig. 7, as there should be a fields for fo-en-liq and en-liq

This sentence has been rephrased. I've also added the low temperature en-bearing fields to Figure 7

Line 312: "explained by an MgO"
Corrected!

The concave up nature of the liquidus in this study is very interesting and convincing. Is there any chance this in part reflects H₂O loss in higher water content experiments? I don't think so, but just a thought.

Interesting thought. Given the different temperatures at which the liquidus curves turn over in the fo-H₂O, en-H₂O and stv-H₂O systems, I suspect not. Yamada et al. (2004) saw something very similar, and we didn't see any evidence for melt loss. I've made a comment to this effect at the end of the MgSiO₃-H₂O subsection.

Fig. 7. I don't like the way you label the T contours on the sidelines. I think you can label them within the ternary as is typically done, and then you should plot the phases that occur on the bounding binary joins (e.g. br, fo, en). The light grey points are not explained in the caption and are a distraction. I also found a few inconsistencies with the liquidus phase relations depicted and the results in Table 1, but it was not clear to me whether this diagram is based on your best guess or is made from the thermodynamic model.

Changed labelling

Removed grey points

Added description of how the figure was created

Line 388. I am not sure about this sentence. What you say is true, but the amount of melt will always depend on the water content of the source, and so a rapid increase in mass fraction is a relative thing. If the source contains a few hundred ppm water, then there will never be a lot of melt, even though the mass fraction may increase from say 0.0001 to 0.01, it is still small and hard to detect.

Seismic detection will depend mostly on their alignment

The change in composition and density is of course very important to its mobility.

I've reworked these sentences to focus on the channelisation, which will be promoted by the increase in silicate solubility in the melt and increase melt fraction and connectivity.

Lines 385-390: Similar to the comments of the reviewer, I also note that this argument is unlikely to be correct. The melt proportion is controlled by the bulk water content of the source and the D of water.

So even if water content in melt changes significantly, that does not necessarily mean generation of seismically detectable melt fraction.

See above

Plus, a temperature change of 1300 to 1700 degree C is huge. Is it likely that such a large change in T is realized even for deeply subducted slab-mantle interface, which should already be thermally

equilibrated more than in typical subduction zone conditions?
I've now changed the sentence to be more specific about temperature ranges in the mantle at ~400 km.

Line 395. Need a ref here regarding channelization.
Reworded to link the two comments on channelization.

Conclusion: This may simply be a writing style issue, but the discussion section seems to end somewhat abruptly. Would the paper benefit from a small concluding remarks section or something similar?

Agreed. I've added a concluding remarks section, as suggested.

Line 476; Figure # should be Figure 9
Corrected

Figure 9 and 10: In the figure captions write that these are partition coefficients for H₂O. Of course you mentioned in the text but from the figures one can't tell its partition coefficient for what.
Done

Figure 9 and 10 y-axes: Again, write H₂O as subscript to the Ds so that the figures are self-explanatory.
Corrected

Best wishes,

Robert Myhill
School of Earth Sciences
University of Bristol
Wills Memorial Building
Queens Road
Bristol BS8 1RJ
United Kingdom
Tel: +44 (0) 117 954 5400
bob.myhill@bristol.ac.uk