Line 28: could use a reference at the end of this sentence (e.g. Spiegelman).  
  
Line 33: could use a reference at the end of sentence about water bufget (e.g. Hirschmann)  
  
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…".  
  
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.

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 of ending the introduction by saying generally what they have done in this study.

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 contrain phase relations…"  
  
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?  
  
Line 137: "…concentrated hydrous melts". Presumably you mean water-rich.  
  
Line 193: typo, should be 'note' not 'notes'  
  
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?  
  
Line 227: You might consider a new section here as currently the segue is a bit clunky.  
  
Line 269: Should read "…between quench crystals and liquidus phases…."  
  
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 by "dash".  
  
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.  
  
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…  
  
Line 312: "…explained by an MgO…"  
  
The concave up nature if the liquidii 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.  
  
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 he 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.  
  
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.  
  
Line 395. Need a ref here regarding channelization.

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. 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?  
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?

Line 476; Figure # should be Figure 9  
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.  
Figure 9 and 10 y-axes: Again, write H2O as subscript to the Ds so that the figures are self-explanatory.