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Interactive comment on “Testing the effects of the numerical implementation of water migration on models of subduction dynamics” *by* M. E. T. Quinquis and S. J. H. Buiter

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Dear Matthieu, Dear Susanne,

I am not sure about the form that this short comment should take, so I'll do it as a quick review of your paper. Hopefully it will help to improve your manuscript.

To begin with I have noted some minors mistakes or unclear sentences. page 1775 Line 13: In Richard & Bercovici, 2006 and Richard & Iwamori 2010, I did not modeled the free water, only bound water was considered. In Richard et al. 2007 free water was considered and the model was a two-phase model with full coupling between the fluid phase (water) and the matrix (the subducting slab entering the lower mantle). These

three references have nothing to do in this sentence. p. l779 l. 10-12 : It would be clearer to talk about water storage capacity instead of maximum water content and to refer to Férot & Bolfan-Casanova, EPSL 2012. p. 1780 l. 15 : again we did not modeled the free water in Richard & Bercovici, 2006

There are a few simplifications that you have made that are to me not enough discussed/justified. p. 1777 l. 16-18 : you are imposing the viscosity to be larger than 1018 Pas. This cut off may have large effect on the large dynamics. Your model often displays water concentration larger than 4000 wt ppm (fig. 5) and in this region the viscosity is overestimated. I don't know how much the large scale dynamics can be changed by the missing low viscosity regions but I think that you should better assess this approximation. p. 1779 l. 16 : You completely neglect the diffusion of the bound water. Effects maybe negligible but it would be nice to make sure or at least justify this assumption. p. 1781 l. 2-4 : You neglect the dynamic component of the pressure gradient arguing that in your subduction models pressures are mainly lithostatic. What does mean 'mainly lithostatic' ? I am not sure that locally in the corner flow this assumption still holds. Maybe it is the case but you should somehow prove it. p. 1782 l.21 : Why do you add a cold lithosphere at the top of the mantle in this setup? It makes the model more complicated and seems unnecessary regarding the test you are making. p. 1788 l. 10 : I am wondering how you chose the efficiency factor you are using. It comes down to modify the permeability and can be discussed. Your definition of permeability looks like the one given by Wark et al. [2003] but I don't see why 0.1 is a good efficiency factor. In the subduction zone model a larger permeability may change the large scale dynamics, no ?

In fact, my main concern is that the different implementations that you have tested are not covering the full range of processes that may modify the large scale subduction dynamics. To use your word (p. 1785 l.28) none of the implementations tested is 'exact'. As you mentioned p.1790 l.19 the effect of free water on mantle rheology is not taken into account in your models. Also melting is deeply coupled with water transport at subduction zone and is likely to change the entire story. Two-phase models taking into account the coupling between mantle and geofluid (water and melt) dynamics already suggest that fluids have a big effect on large scale dynamics. This, to me, deserves to be more discussed in your paper. To conclude I would suggest to be less categorical in your statement that simple numerical implementations are sufficient if one wants to understand large scale subduction dynamics. I believe the points I have mentioned above and that you also have partially mentioned in your paper are numerous enough to somehow mitigate this conclusion.

Interactive comment on Solid Earth Discuss., 5, 1771, 2013.