Solid Earth Discuss., 5, C1155–C1159, 2014
www.solid-earth-discuss.net/5/C1155/2014/

Solid Earth Discussions

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

M. E. T. Quinquis and S. J. H. Buiter

matthieu@quinquis.net

Received and published: 28 April 2014

" 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."

Dear Guillaume, Thank you for your feedback on our manuscript, which has helped us clarify several of the points listed below.

"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"

Thank you for pointing out our mistakes in referencing. We have removed the references to Richard & Bercovici (2006), Richard et al. (2007) and Richard & Iwamori (2010) from the description of the first water migration method. We have changed "maximum water content" to water storage capacity and added a reference to Férot and Bolfan-Casanova (2012).

"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."

The use of a minimum viscosity cut-off (and a maximum viscosity cut-off too) is common for finite-element models like ours that are applied to geodynamic problems with a potentially large range in viscosity variations. The reason is that the numerical system is best solved for viscosity contrasts that do not become overly large (say 8 orders of magnitude). We have constrained our minimum viscosity to 1018 Pa s. This value is in line with other dynamic subduction models (that have given their minimum viscosity value, e.g., 1018 Pa s in Billen and Hirth, 2007 or 1019 Pa s in Gerya et al., 2002). We had also run a model with a lower minimum viscosity that showed that changing the minimum viscosity in our series of models have little to no effect on the dynamics of the system as the cut off is only very locally reached. We have added the following in section 2.1: “The minimum viscosity of 1018 Pas is low enough to capture almost all viscosity variations in our model. The cut-off value is only reached very locally in the mantle wedge.” We use the effects of bound water on viscosity in the models, but not the effects of free water. The pyrolitic material has an imposed water storage capacity of 2000 PPM following the upper mantle water storage estimates given by Bercovici and Karato (2003). This means that the imposed minimum viscosity does not influence the dynamics of the system as the effective minimum viscosity observed in the mantle wedge is above the minimum viscosity cut-off. High concentrations of free water are indeed observed in the mantle wedge. The potential influence of this free water content on viscosity is discussed in the manuscript, in section 4.3

"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."

As we focus on the role of free water migration, we have kept our models relatively simple and indeed ignored the diffusion of bound water. We have added this in the Introduction: ‘Because we focus on first order behaviour of free water migration on subduction dynamics, we keep our models relatively simple and neglect the effect of bound water diffusion.’

"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."

We agree. Our models assume vertical migration of water and we are therefore implicitly arguing that the horizontal pressure gradient, dP/dx, is small. We now illustrate this with (new) figure 4 of the manuscript, that shows the contoured values of variations in the pressure field following the horizontal component (dP/dx) superimposed on the bound water content for the model using scheme I. The models run using the three water migration schemes all show very similar pressure fields in the mantle wedge. dP/dx

is low, varying with 1 MPa over a distance of 20 km where the bound water content is high, which is negligible in comparison to the vertical variations in the pressure field. We therefore ignore the dynamic component of the pressure gradient, reproducing the water migration scheme of Cagnioncle et al. (2003).

"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."

We did keep the sinking cylinder model simple, but we chose to add a lithosphere on top of the mantle, as we aim towards subduction models that also include an overriding lithosphere. The lithosphere is a strong layer and does not add so much complexity to the model.

"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 ?"

The efficiency factor is defined in section 2.3 and the effect of the efficiency factor is tested in the sinking cylinder models (section 3.2 and figure 7 G, H and I).

"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."

Our models focus on the effects of simple implementations of numerical water migration mechanisms, following simplified approaches that are being used in our community. As geodynamic models on the scale of the upper mantle are often already rather complex, an approach that captures first order effects of the effects of water, without introducing two-phase flow models, is sought after. It is not our aim to cover the full range of processes that may modify large-scale subduction dynamics. We highlight that the different first order approximations that we tested do not provide large variations in the models. But clearly more work is necessary if we are to fully understand the effects of dehydration on subduction dynamics. We better motivate our approach in the Introduction and have narrowed down our conclusions.

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