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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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“General comments The authors present a study comparing three numerical implementations adding parametrizations of water transport to computational simulations of subduction zones. The research presented in this manuscript generally is relevant to the numerical study of the dynamics and chemistry of subduction systems. As simulations of fully coupled two-phase flow for the study of fluid transport in large-scale geodynamic situations such as subduction zones are difficult to achieve, many computational studies resort to simplified numerical parametrizations of fluid transport. This study compares three such parametrizations in terms of their influence on first a sinking

sphere model and later on a realistically scaled subduction model. The three numerical methods are based on the same principle of fluid moving upwards at higher rates than solid deformation, thereby hydrating the material they are migrating across. The ability of local rock to de/hydrate, and thus the availability of free water for transport, is determined with the thermodynamic software Perple_X. The three methods are different in what velocity is chosen to govern water migration. The first method chooses a transport rate given by the arbitrary choice of one vertical discrete grid space per discrete time step. The second method uses a given vertical fluid migration velocity combined with the local solid velocity vector, and the third method uses a buoyancy-driven Darcy velocity, again combined with local solid velocity. After comparing the model outcomes using these three numerical parametrizations, the authors conclude that both the model evolutions and their final states do not show significant differences for the three methods tested. From this observation they draw the conclusion that the specifics of the numerical implementation of water transport in large-scale simulations are "not that important" (p. 1791, l. 27), and that even the arbitrary choices made for the first of the three methods should be "equally fine" (p.1792, l. 4). I see some substantial issues with both content and presentation of this research in the current form. Nevertheless, I would recommend this manuscript to be published subject to a number of revisions and technical corrections as listed below. (1) The authors give the misleading impression of presenting a comprehensive overview over various numerical implementations of water migration coupled to de/hydration processes in subduction zone models. What the authors in fact do is comparing three very similar and rather basic parametrizations of water transport, none of which is a numerical implementation of the actual physics involved in fluid transport through a deforming solid matrix. The fundamental physics of this problem set are those described as thermo-chemically coupled reactive two-phase flow (i.e. conservation of mass, momentum, energy and composition). Of course, to compare parametrizations such as the ones employed here to a numerical implementation of fully coupled two-phase flow would go beyond the scope of the present study. Nevertheless, all the discussion of the presented research should be clearly put into that physical context throughout the manuscript, and necessary qualifications should be added."

Our manuscript is not intended as a review of numerical water migration mechanisms, nor did we want to give the impression that it was. We compare simplified representations of water transport, as used before in other numerical studies. We have tried to better place our type of study in the context of two-phase flow studies and to discuss the motivation for using simplifications and the potential drawbacks of those.

"(2) Based on two-phase physics, there are a number of scaling relationships, such as characteristic velocity of fluid percolation, characteristic length scales (compaction length), characteristic over-pressures produced by fluid flow, etc. To take some of these scalings into account both in justifying the model simplifications leading to the choice of parametrizations of fluid transport, as well as in a more quantitative discussion of the results, would greatly benefit this study."

Our study focuses on the effects of basic numerical schemes for free water migration. As mentioned above, we have now better placed our models in the context of two-phase flow. However, we feel that more detailed information related to these mechanisms does not necessarily help our study, as we do not use two-phase flow methods.

"(3) The chosen particle-wise treatment of distributing water content along the migration path raises the issue of mass conservation. The basic unit of discretization in finite element models is the finite element. Marker particles should only be used to track the advection of material properties along the material flow, but these particles are not themselves a consistent unit of spatial discretization, meaning that a particle does not have a defined volume or mass in such a numerical method. To treat hydration processes as particle-wise operation renders the whole treatment susceptible to particle density and thus violates conservation of mass. The issue is further complicated by the insertion/deletion scheme employed to maintaining a more uniform particle per element count. The particle-wise procedure constitutes a fundamental weakness of the

water migration schemes presented in this study, which needs to be addressed. As a basis of this discussion, the authors should state a conservation equation of water content and then discuss which simplifications lead to the particular implementation and how they may be justified."

The reviewer correctly points out a limitation of using particles to transport water. We are very aware of the potential pitfalls here and did discuss our recipe for what to do with the water content for newly inserted particles (these are inserted dry) or particles that are deleted (their water content is distributed over the other particles in the element). We monitor the total water content of the model to ensure that we do not violate the conservation of water content.

" (4) In the light of the issues raised in the first comments, I would encourage the authors to add some strong qualifications to their conclusions. Given that the only difference between the three parametrizations is the different fluid velocity vector applied to the method, the fact that the final outcome of the results is similar is not of great surprise. This observation, however, does not in itself give rise to any conclusions about the general validity of this type of parametrization for large-scale geodynamic models. I would also argue that the use of something as arbitrary as the first of the three methods should not be condoned with the term "equally fine". Rather I would recommend the authors to deal with the question, why the final result of all three methods come to look similar at all to the first method with its frankly odd choice of one grid cell per time step for defining a fluid transport rate."

The three methods investigated in our manuscript are based on schemes used in literature: scheme I from Arcay et al., 2005, scheme II is adapted from Gorczyk et al., 2007 or Gerya et al., 2002, and scheme III from Cargnioncle et al, 2007. We have tried to highlight this better in the manuscript.

" (5) Style of language and clarity of presentation: Although the language use is generally good, the chosen register is perhaps a bit too informal at times. Most notably the frequent use of "our" relating to various parts of the research or to individual model properties perhaps sets a slightly too personal tone. Also, there are a number of sections in the text, particularly during the presentation and discussion of results, that could profit from a reorganization of the content to create a more clearly laid our logic pathway for the reader to follow (see specific comments below)."

We feel that the style of the language is a matter of personal preference. We often use 'our' to emphasize that these are our choices.

" (6) Use of figures: The general quality of figures supplied along with the text is good. However, it would be generally helpful if the authors gave a short introduction to the contents of a figure when they first refer to it. Unfortunately, that is not the case in the present manuscript. The authors are strongly encouraged to add such descriptions to the text, especially throughout the results and discussion sections."

We describe the figures in the figure captions and chose not to repeat this in the text.

"Specific comments A list of specific comments is given below, commenting on some more specific issues. p. 1771 âĂÍTitle: Given the limited spectrum of numerical parametrizations tested in this study, I suggest the title to be adapted to be more narrow in focus. For example: "Testing the effect of three numerical parametrizations of water migration on models of subduction dynamics.""

The title has been changed to: 'Testing the effects of basic numerical implementations of water migration on models of subduction dynamics.'

"p. 1773 Introduction: As mentioned in the general comments, some discussion of the following points should be added to the introduction: (i) Water migration is governed by two-phase flow, for which a full physical theory is available; (ii) some reasons why numerical implementation of the fully coupled two-phase physics is challenging, especially in large-scale geodynamic simulations; (iii) some justification for the choice of the three kinds of parametrized implementation of water migration to be tested; (iv) limits

of the scope of this comparative study imposed by the methodological similarity of the three chosen methods. l. 15: The two kinds of free water listed here seem to refer to the same thing, which is a free aqueous fluids present in, and percolating through the pore space. In two-phase physics, the porosity is traditionally understood as the volume fraction of the continuum occupied by a fluid phase inside a solid matrix, and in a granular matrix that would be naturally be the space along the grain boundaries. Please clarify your statement."

We have rephrased the sentence to: 'Free fluids that are contained in the porosity of the rock and can percolate along grain-boundaries'

"l. 20: This logic is difficult to follow. One of the known processes of bringing water down into the oceanic crust is the extensional stress state along the top boundary of the bending subducting plate. These bending stresses seems to be large close to the trench, so it would rather seem that these conditions would lead to decompaction bringing more free water into the oceaninc crust, rather than expelling it through compaction? I suggest checking the physical argument and reformulating this section for clarity."

The faulting due to the bending of the oceanic lithosphere indeed helps to bring water into the oceanic crust (Faccenda et al., 2008). But once the slab starts to move down, below the overriding plate, the pressure in the slab increases, decreasing the porosity and free water is expelled. This process occurs above 20 km depth (Rüpke et al., 2004). We have reformulated the text to: 'Most of the water contained in the porosity of the sediments is expelled near the trench through compaction and is not transported into the mantle. This is known as the fore arc volatile discharge'

"p. 1774 âĂÍl. 9-10: This sentence suggests that water released from dehydration of the slab forms cold plumes, which is not the case. Rather, such cold plumes consist of mantle material hydrated by water released from the slab, thus creating weak and positively buoyant batches of mantle that may rise as cold diapirs. Please reformulate for clarity.”

We have reformulated the sentence as: ‘Mantle material that is hydrated by water released from the slab may also form weak, positively buoyant ‘wet plumes’ that rise upwards and efficiently hydrate the mantle wedge (Billen and Gurnis, 2001; Billen, 2008; Gorczyk et al., 2007; Richard and Iwamori, 2010)’

“l. 14-15: This comment on subduction initiation seems out of context here, because water that potentially contributes to subduction initiation must be derived from a different source then the subduction fluids that are the subject of this study. I suggest to omit this statement.”

We agree and have deleted the sentence.

“l. 29: "However, exactly how water migrates in the lithosphere and the mantle is not well constrained." I disagree with this statement. The physics of this process is sufficiently well described by the two-phase flow conservation equations. It is true, however, that these physics have not yet been used to full effect in numerical simulations of subduction zone dynamics. Please correct the statement.”

We have rephrased the sentence to describe the lack of large-scale numerical models of subduction dynamics that include two-phase flow: ‘Water migration can be described by two-phase flow conservation equations (Spiegelman, 1993a,b; Bercovici et al., 2001). However, these are not routinely used in numerical simulations of subduction zone dynamics at the scale of the upper mantle as it adds a fairly complex set of equations to an already highly non-linear model. Usually simplifications are therefore made to migrate water in large-scale subduction models’

“p. 1775 l. 2-3: See comment above (p. 1775, l. 15). The two types of fluid transport seem to refer to the same concept. Please reformulate for clarity.”

We have reformulated the sentence, along the lines of our reply to the comment above (p 1773, line 15).

“l. 7-10: This sentence is not quite clear. The authors refer to the effect of advection of both free and bound water with the deformation field of the solid rock phase. Please clarify the statement.”

We have reformulated this paragraph to: ‘Bound water is advected along the solid flow field. Bound water can in addition be affected by diffusion mechanisms which allow water to migrate through the solid phase field as a function of the chemical and temperature gradients (Richard et al., 2006). 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. Free water can migrate in the interconnected pore space of the solid phase (Stern, 2002; Wark et al., 2003; Cheadle et al., 2004; Rüpke et al., 2004), create its own hydrated channels (Katz et al., 2006), or be absorbed by non-saturated rocks of the mantle wedge (Iwamori, 1998) to be potentially transported with the mantle flow into the lower mantle (Bercovici and Karato, 2003; Iwamori, 2007; Richard and Bercovici, 2009; Fujita and Ogawa, 2013). Free water is also advected by the solid flow field through which it migrates and this can result in cases where part of the free water migrates up through the mantle wedge, while the rest is carried with the solid flow and subducted into the mantle (Cagnioncle et al., 2007)’

“l. 22-26: Both in your study as well as in the one quoted from Cagnioncle et al. (2007), Darcy flux is calculated purely from the buoyancy contrast between fluid and solid phase, and not from the actual pressure gradient. Please reformulate accordingly. Also, when referring to the influence of solid material flow on fluid migration, please use the terminology of advection along the solid flow field.”

This part has been reformulated to ‘Free water 100 migrates as a Darcy flow, following the density gradient between the solid phase and the fluid phase in the mantle wedge (Cagnioncle et al., 2007). The term material flow has been changed to solid flow field in the whole manuscript.

“l. 13-26: As the three approaches to parametrized water migration are in fact the same as the three implementations the authors later test, it would be useful to assign some sort of identifier to each type (types A, B, C or I, II, III, or similar), which the authors then consistently use throughout the manuscript to refer to these types of implementation. It would greatly improve the clarity of the following presentation and discussion of model results.”

We have renamed the schemes in the manuscript to scheme I, II and III

“l. 27 to p. 1776, l. 3: This passage is not stated clearly enough. What exactly is meant by the first sentence? The third sentence should more clearly point out that no previous study has compared the outcome of more than one of these parametrizations in comparison.”

We have changed the text to: ‘Studies that use the above water migration schemes show differences in the spatial distribution of hydrated material in the mantle wedge as subduction evolves (Arcay et al., 2005; Gerya et al., 2002; Gorczyk et al., 2007; Cagnioncle et al., 2007). However, as the numerical setup of the subduction models also differs between these studies, it is difficult to evaluate the possible effects of the numerical implementation of water migration. So far, the influence of the basic numerical implementation of water migration on the dynamics of a subduction model has not been investigated’

“p. 1776 l. 4: Be more specific about the method of investigation, which is to take the three types of melt migration schemes introduced above and compare the outcomes of these three schemes for the two model types, sinking sphere and subduction zone.”

We have changed the text to: ‘We aim to investigate the effects of the three numerical water migration schemes described above (schemes I, II and III) on the dynamics of a subducting slab and its overlying mantle wedge.’

“l. 13: "slow flow" is a slightly unusual expression in this context. Please use "Stokes

flow" instead, which is the traditional terminology used for this set of equations.”

‘Slow flow’ refers to velocities being low in these type of geodynamic problems and is not such an unusual term. But we have rephrased to ‘Thermo-mechanical equations’

“p. 1777: l. 4: This quantity ε ÌĞâĂše is in fact the second invariant of the deviatoric strain rate tensor and should be labelled as such for clarity. Accordingly, I would recommend to use the symbol ε ÌĞâĂšII or perhaps ε ÌĞâĂš2 instead. Please propagate this terminology throughout the manuscript.”

εâĂše is the square root of the second invariant. We follow the notation of Ranalli (Rheology of the Earth, 1987). The scientific notations are explained in the manuscript and are consistent throughout.

“l. 11: "...only bound water influences the viscosity...". This is an odd choice. The presence of any fluid phase, be it water or melt, certainly weakens the solid matrix significantly, as the authors state elsewhere. This choice needs some justification, or else models should be rerun with free water weakening added to the method.”

The Hirth and Kohlstedt (2003) flow laws for wet and dry olivine, that we use for mantle materials, determine the viscosity using only the amount of bound water. The influence of free water is not included. Our first focus therefore lies on the effects of the bound water. We discuss the potential effects of free water in section 4.3. To investigate the first order effects that free water could have on our models, in a simplified manner, we have run a model that includes the combined effects of bound and free water on the models of scheme II with an imposed vertical velocity of 5 cm yr=1. In this model, the spatial distribution of bound water is the highest. We limit the amount of (bound plus free) water used in the viscosity flow law equation to 1 wt.% (from 0.4 wt.% for the model with only the bound water effect on viscosity). Fig. 16 shows that the spatial distribution of bound water in the mantle wedge is reduced and less hydrated material subducts to deeper depths. However, the spatial distribution of free water does not change much. As the effect of free water on viscosity is not well established, we prefer to keep the models in this manuscript with the effect of bound water on viscosity only (following creep flow law eq. 4) and to illustrate the potential effect of free water with the new model of Fig. 16 model in the discussion section 4.3

“l. 16: The need of a minimum viscosity condition as well as the value of 1018 P as needs some more discussion. Why is it necessary, and why is this specific value chosen? How would a different (lower) value potentially influence the model outcome? “

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, Billen and Hirth, 2007). 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.”’

“l. 17-18: This restriction of COH to 4000 ppm is not clear enough. Does this only relate to the effect of water content on viscosity or is water content limited generally? Why is this cutoff necessary to the modelling approach?”

In the flow law calculation (equation 4 and equation 5), a water content of 0.4 wt% results in a viscosity decrease by ca. 2 orders of magnitude when using the dislocation or diffusion creep flow law for wet olivine from Hirth and Kohlstedt (2003). This implies a potentially substantial effect of water on viscosity. The magnitude of the change can result in viscosities that are lower than the minimum viscosity of 1018 Pa s which

is imposed in our models. We therefore assume that viscosity no longer decreases further once water content exceeds 0.4 wt%.

“l. 21ff, Eq. (6): As commented on above in the context of strain rates, I strongly suggest to consistently use the terminology of second invariants, especially in the case of shear stresses. The terminology of effective stress usually refers to the effective stress principle after Terzaghi (1923) or Skempton (1960), which is an entirely different matter. Therefore, I again suggest to use a symbol of the type σIâĂŠ I or σ2âĂŠ , rather than σeâĂŠ . Another useful way of referring to second invariants of stress or strain rate tensors is in terms of magnitudes, as their physical meaning is the magnitude of a tensorial quantity apart from their directionality.”

σ ÌĞâĂŠe is the square root of the second invariant of the stress tensor. We follow the notation of Ranalli (Rheology of the Earth, 1987). The scientific notations are explained in the manuscript and are consistent throughout. We have corrected effective deviatoric stress to effective deviatoric shear stress.

“l. 23: For the strain-weakening of the friction angle, do the authors use the full accumulated strain or only the component caused by plastic failure? I believe the latter option is more frequently used and makes more sense physically. Please clarify.”

Strain-weakening of the angle of internal friction is determined from the total plastic strain. We have modified our text as ‘φ undergoes a linear decrease with total effective plastic strain (measured as the square root of the second invariant of the strain tensor) to simulate strain-weakening.’

“p. 1778: l. 12-13: Please give some more information on the particle advection scheme. Presumably all material properties are defined on markers and these markers are then advected along with the solid flow field? As this is an important feature of the modelling approach it necessary to elaborate some more.”

We have added the following: ‘Particles are used to track the material field (through a material identifier) and properties such as particles strain, stress, and water content. The particles are advected with the solid flow field using a second-order Runge-Kutta scheme.'

"p. 1779 l. 10-12: First, what exactly is meant by the term "maximum water content"? Is it the maximum allowed by your method in the sense of a cutoff value, or is it the physical saturation level? If it is the latter, I think saturation water content is the better terminology. Second, the limit of maximum water content to 0.2 wt% is not clear. Why is any such imposed limit needed?"

We have changed the terminology, also following the comment by Guillaume Richard. The maximum water content corresponds to the water storage capacity of the different lithologies used in the model setup. We use a minimum value for the water storage capacity of the mantle because the pressure, temperature and water content phase diagrams determined by Schmidt and Poly,m (1998) give a water content of the mantle of 0 whereas Bercovicci and Karato (2003) show that the average water content for the upper mantle is ca. 0.2 wt%. This has been added in the text.

"l. 13: To mention the conversion rate between wt% and ppm seems to be stating the obvious, so the sentence could be removed. However, the question arises, why the authors don't use one consistent unit for water content throughout the manuscript instead. I would suggest doing so to avoid confusion."

The sentence has been deleted.

"l. 15-16: Omit "In our models", as the advection of bound water content by material flow is a feature of the fundamental physics, not of the model. Please also explain what is meant by "material identifier". If I understand correctly, the bound water content is stored on marker particles and thus advected along with the flow field of the rock. Please clarify this passage."

We have deleted 'in our models'. We have explained the particle scheme in more detail

in section 2.1, following the earlier comment of the reviewer.

"l. 17-18: Perhaps "element-wise vertical transport" would be a better description for the first type of migration scheme."

We use 'one element vertically up per time step' and refer to the scheme as elemental water migration scheme I.

"l. 19ff: Again, does maximum allowed water content mean saturation water content at given PT conditions? If I understand the procedure correctly, what happens in step (2) is not so much the moving of free water, but the determining of the migration path, along which in step (3) the free water is distributed. Please clarify the three steps."

We have changed the text to: '(2) determine the migration path for the free water, if present,'

"l. 21ff: It would be helpful to the reader, if in the following detailed explanation of the procedures involved in the three migration schemes would be rearranged such that they reflect the order of the three steps of procedure introduced above. That would give an improved logic sequence to the following text: (1) description of how saturation water contents are calculated for each particle; (2) description of how free water migration pathways are determined for each of the three migration scheme; (3) description of how free water is distributed along the migration path to hydrate material where possible, and how any left-over free water is treated."

We would rather have a complete description of each migration mechanism by itself as that allows easy back referencing for readers. Also, our results follow the three migration schemes. We would prefer therefore to keep an ordering by migration schemes.

"p. 1780 l. 6-10: This passage is not written clearly enough. Additionally, the point that irregular grid spacing should be avoided in order to keep a constant water migration velocity is nonsensical. Rather, a water transport velocity linked to the grid spacing should be avoided, as it is a purely arbitrary, non-physical choice that renders any model outcome grid-dependent, which generally is not a favourable condition."

We agree that using a grid dependent method is not so ideal. However, this scheme is easiest to implement and thus allows a quick appraisal of the potential effects of water on a model. The scheme has been used in our community (e.g., Arcay et al., 2005). In case such a scheme is used it is necessary to avoid grid variations so that water migration velocities are not variable because of the numerical grid.

"l. 16-17: "...does not necessarily eliminate it totally". This statement is not entirely correct. The change from element-wise vertical transport to an imposed vertical transport velocity removes the grid-dependence of the velocity, but does not remove the grid-dependence of the hydrated migration pathway, which in very high spatial resolution would be close to a straight line, whereas in coarser resolutions, the "staircase" character of the hydration path is much more pronounced. Please clarify the statement."

We have clarified the text to: 'This method reduces the grid dependence of the migration scheme, though does not eliminate it totally as the migration path itself is grid dependent.'

"p. 1781 l. 4: "...pressures are mainly lithostatic". Especially in the mantle wedge dynamic pressures are certainly not negligible. Even in the simplest possible model setup, the corner flow forced by the subducting slab leads to substantial dynamic pressure variation in the mantle wedge, especially close to the corner. I do not quite understand why the authors chose not to use the full pressure solution to determine the Darcy velocity, as it would be straight-forward to do so. I suggest to state Darcy's law in it's full form in the text before making the assumption of lithostatic pressure."

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). We have changed the text to give the Darcy law, the permeability and our modification of the permeability with the efficiency factor.: 'The third migration scheme (scheme III) follows a simplification of Darcy flow where the fluid follows the pressure gradient caused by the difference in density between the fluid and the solid it is percolating through (Turcotte and Schubert, 2002): q=(_f-_s)gκ/η_f where q is the Darcy velocity, s and f the density of the solid and fluid respectively, g the gravi- tational acceleration, ηf the viscosity of the fluid, and k the permeability. The permeability follows the empirical definition of Wark et al. (2003): κ=("d" ˆ2 Φˆ3)/270 where Φ the volume fraction of fluid and d the grain size (same as in Eq. 4). The fluid velocity is the sum of the Darcy velocity relative to the volume fraction of fluid and the solid velocity: v_f=q/Φ+v_s where vf is the fluid velocity and vs the solid phase velocity.âĂÍThe water migration and distribution then follow scheme II. We assume, however, that water migration in scheme III is vertical. This assumption (also previously made by Cagnioncle et al., 2007) is reasonable as the horizontal pressure gradient in our models in the mantle wedge is much smaller than the vertical pressure gradient (Fig. 4). Φ, the volume fraction of fluid, is determined from the initial water content and the grids for pressure, temperature and wt.% H2O. However, this assumes that all the free water that is present in interconnected channels is used to calculate the fluid velocity (Eqs. 8-10). This would result in unnaturally high fluid velocities. We therefore introduce an efficiency factor, ω, that corresponds to the percentage of interconnected channels of the network through which water can migrate. The effective permeability in Eq. 8 then becomes κe = $\omega\kappa$. This reduces the effective fluid velocity as water can only migrate through the interconnected network. The Darcy water velocity is calculated for every particle of the model. The water velocity is not constant throughout the model and areas with higher water content have higher water migration velocities.'

"Eq. (8): Traditionally, the symbol vf is used to refer to fluid velocity, which may be expressed as a combination of the solid velocity vs and the Darcy velocity q as vf = vs + q/φ. This is found from rearranging the definition of Darcy velocity, which is q = φ(vf $-$ vs). Would the authors please clarify their notation and add an equation of how they combine their Darcy velocity with the solid flow field to obtain the water migration pathway. Also, it would benefit the clarity to separate the definition of permeability out of Eq. (8), so that it becomes q = ω kφ Δg, which is more in tune with traditional ηf notation of Darcy's law. The permeability then becomes kφ = d2φ2. Please also supply a reference for your choice of a geometrical factor of 270."

We have changed the paragraph, updating the equation set as suggested by the reviewer (see above). The geometrical factor of 270 is from Wark et al.,(2003) and this citation has been added to the text.

"l. 12-14: The question of how much of the water is present in interconnected pore space is certainly not straight forward to quantify. However, it is well known that fluids can form interconnected pore networks already at very small volume fractions of below 1%. It seems that the main function of the introduced factor ω is to impose a variation on permeability, thus reducing or increasing the speed of water migration. Please clarify the use of ω."

This has been updated in the manuscript to: 'However, this assumes that all the free water that is present in interconnected channels is used to calculate the fluid velocity (equation 8 to 10) and would result in unnaturally high fluid velocities. We therefore introduce an efficiency factor, ω, that corresponds to the percentage of interconnected channels of the network through which water can migrate. The effective porosity used in equation 8 then becomes ke= ωk. This reduces the effective fluid velocity as water can only migrate through the interconnected network.'

"l. 19-20: The last sentence of this paragraph should be reformulated in the context

of the fluid velocity defined by the statement vf = vs + q/φ, as it follows from standard two-phase flow theory. Darcy's law defines a relative flux q of fluid material with respect to solid material flow vs. Please reformulate and clarify."

We have reformulated following the comment with eq. (8) above.

"l. 21ff: I have some concerns about the criteria by which the authors mean to quantify the model results. First, it certainly makes sense to track the topmost extent of the hydrated zone, however the information gained from it does not quantify an "effective water migration velocity", but rather the rate of advancement of the hydration front. These two concepts are not the same. Second, these criteria only quantify the vertical extent of the hydrated zone, but not its structure or its geometry. The author seem to not take quantitative account of such factors. Third, the quantity a root-mean-square water content is not entirely clear. How is it calculated? Why do the authors not use a more physical quantity like the total water mass or volume integrated over those parts of the domain?"

We have changed 'effective water migration velocity' when discussing to top hydrated particle to 'rate of advancement of the hydration front.' We now also visualise the width of the hydrated zone by tracking the leftmost and the rightmost particle of hydrated mantle (Fig. 12). We have added an equation for the calculation of root-mean-square water content (section 2.3) We do not calculate the water mass volume as it gives the same information as the root-mean-square water content.

"p. 1782 l. 7: Please choose a different name for this type of model setup. "Stokes model" is a term often used for any flow model described by Stokes equations. Perhaps "sinking sphere model" would be an alternative option. Please propagate this change to all following passages, where this model type is referred to."

We have changed this model to 'a simple model of a sinking cylinder'.

"l. 13: What do the authors mean by "...do not couple the thermal and mechanical ascpects of the models."? As temperature advection is taken into account, there already is a coupling between thermal and mechanical aspects of the model. Do the authors mean to say, that no T-dependent viscosity is employed here? “

In this particular case, we solve the mechanical and thermal systems separately. The thermal system does need the velocities of the mechanical system, but we do not use the temperatures in the mechanical calculations. We have changed the text to ‘We solve for the advection and conduction of temperature in addition to the mechanical flow, but the temperature does not play a role in the mechanical flow as viscosities are linear viscous.’

“p. 1783: l. 10: The values chosen for the vertical migration velocity should be based on some scaling argument derived from two-phase physics. Please elaborate your choice. p. 1784 l. 4: From the context I assume that the symbol OHrms refers to the quantity of root- mean-square water content. I could, however, not find any passage in the manuscript where the symbol is defined. Please check and correct if necessary.”

OHrms is now defined in section 2.3, where we describe the parameters we use to quantify the model results.

“p. 1785 âĂÍl. 15-17: The meaning of this sentence is not clear to me. Please reformulate the idea.”

We have rephrased the sentence to: ‘The sinking cylinder induces a vertical solid flow field in the area of mantle hydration. The free water migration velocities are also vertical. The combined fluid migration flow is therefore vertical and the difference between the schemes lies mainly in the rate of free water migration.’

“l. 27 to p. 1786, l. 1: The meaning of this sentence is not entirely clear. Please reformulate your conclusion.”

We have rephrased to: ‘Our simple models of a sinking wet cylinder in a dry mantle indicate that the three schemes we investigated for numerical implementation of water

migration lead to very similar results. Within these schemes, the exact implementation might be secondary to the first-order effect that water could have on the system.’

“p. 1788 l. 4-6: This short discussion of vertical grid size variation illuminates the fundamental flaw of the element-wise vertical water migration scheme. Since the authors decide to use the scheme despite its obvious flaw for the sake of comparison, the inherent grid-dependence of the scheme should be pointed out clearly as a reason not to use it in future studies.”

We have included this scheme because it is relatively easy to implement, thus allowing to obtain a quick feeling for the potential effects of water on the model under investigation, and because it has been used in published models. We are very aware of the drawbacks and have pointed these out very clearly.

“l. 8-13: The authors run the first and third scheme with and without water-weakened rheology, but not the second. This choice is not obvious, please explain.”

We have run all models with and without the effects of water on the viscosity and added these to the manuscript (Fig. 12).

“l. 16: Is this really "trench migration" if it occurs at the beginning of the run, before stress accumulation leads to brittle failure? Maybe I did not understand the description correctly, please reformulate for clarity.

The trench has not formed yet, so we agree with the reviewer that trench migration is not the correct term to use in the initial stages. We have changed the text to ‘resulting in ca 15 km of advance of the interplate contact.’

“p. 1789 In general the description of results is rather vague and too phenomenological ("migration schemes cause small differences", "effects...are more substantial", "distribution...is similar", "Larger variations occur..."). Please reformulate with better structuring and more quantitative analysis of results. Introduce each figure before referring to results displayed in it.”

The quantification is in the figures, but we have tried to be more quantitative in our description. The description of the figures is in the figure caption. We refer to the figures when discussing the results they display.

“p. 1790 2nd paragraph: This whole paragraph is too vague and not very clear. Also, consider if a longer simulation run time might give more room for water-weakened rheology to show greater effect, especially as water starts penetrating the overriding plate. Also, one of the main influences of free water is to facilitate melting, which has a significant influence on the dynamics of subduction. Furthermore, a water-weakened rheology may lead to cold diapirism, which again influences the dynamics and the overall water transport in the subduction zone (why do these models not develop cold diapirs?). Please restructure your argument for clarity and include some more factors, perhaps along with some scaling arguments to underline your findings.”

We have tried to clarify this paragraph.

“l. 19-20: "free water does not affect the rheology of the mantle materials". However, it certainly does in reality, so the question remains why the authors did not choose to include the rheological weakening by free water, especially as it constitutes a fairly straight-forward addition to any numerical method.”

We addressed this comment following the earlier comment of the reviewer above. We use laboratory flow laws and have therefore not included the effects of free water on viscosity in our dynamic models. However, we show the potential, very first-order effect that free water could have, in the new model of Fig. 16.

“l. 20: "speculate". I suggest making an informed statement in place of speculation.”

We have deleted ‘we could speculate that’

“l. 26: "This would then decrease the potential effects of free water on pore pressures". Yes, but melt would then build up pore pressure instead of the free water it has dissolved, so the potential weakening effect would remain.”

We have added this comment to the discussion: ‘However, the resulting melt would build up pore pressure instead of a pore pressure increase that would have been caused by the now dissolved free water. Therefore, a potential weakening effect of free water on viscosity could remain.’

“p. 1791 l. 20-22: The logic of the last sentence in this paragraph is not quite obvious. How does a stronger corner flow due to water-weakened rheology lead to more hydrated material being entrained downwards by the slab? It would be more obvious, if the water- weakening would lead to stronger mechanical decoupling between slab and mantle wedge and thus less entrainment of hydrated mantle? Please elaborate.”

We have changed the last sentence to: ‘We show that including the weakening effect of bound water on viscosity increases the amount of water brought down to the bottom of the model domain (Fig. 14). This is because the viscosity reduction causes a stronger corner flow that entrains more hydrated mantle material in the downward flow above the subducting slab’

“l. 24: "Stokes flow model". As mentioned above, the term Stokes flow is traditionally used for any flow problem described by Stokes equations. Please use another identifier for your first series of model runs, such as "sinking sphere models" or similar.”

We have called the model the sinking cylinder model.

“Technical corrections A number of technical corrections are listed below, mainly relating to form and language of the manuscript. Title: In order to be a bit more specific about the content, I suggest to adapt the title to: "Testing three numerical implementations of water migration in models of subduction dynamics". p. 1772 l. 1. Add definite article: "...brings water into [the] Earth’s upper mantle." l. 5. Add definite article: "localisation of deformation in [the] lithosphere and mantle." l. 8. Be more specific: "Therefore, [computational] models use..." l. 15. "...the material flow field also moves the free water..." By material flow field, I assume you refer to the flow of the solid rock phase? Please clarify the statement. l. 22. Consider changing expression to: "Our models [demonstrate that] the bound water..." l. 25. Add coma: "in the mantle wedge[,] which..." l. 26. Consider reformulating as: "[This finding underlines the importance of employing] dynamic time evolution models..." p. 1773 âĂÍl. 14. Add "[oceanic] crust" to be more specific of the context. l. 18. Set expression to plural form: "mineralogically bound fluids in the form of [hy- droxyl complexes]." l. 24. Consider reformulating the sentence for better clarity: "It has been well docu- mented that mineralogically bound water is released when [hydrated minerals undergo certain phase transitions]" p. 1774âĂÍ l. 1: Correct spelling: "... even [greater] depths." l. 21: Change expression: "...the subduction slab [may in turn] increase ..." p. 1775 âĂÍl. 2: Change terminology to "interconnected [pore space]"âĂÍ p. 1776 âĂÍl. 3-4: Reformulate to avoid close repetition of "investigate". âĂÍp. 1780âĂÍ l. Correct spelling: "the [remaining] water".âĂÍ p. 1784âĂÍ l. 25: Correct spelling: "can locally [increase]". âĂÍp. 1786âĂÍ l. 16: Change expression to: "(1) a 7 [and] 8 km crustal layer..., respectively." p. 1787 l. 6: Reverse order of words: "16 particles per element are [used initially]". p. 1788âĂÍ l. 20: Change expression: "...but not [significantly]".âĂÍ p. 1790 l. Change expression: "...free water could reduce [the] plastic yield stress, [thereby] reducing ..." p. 1791 âĂÍl. 13-15: Reformulate to avoid close repetition of sentences starting with "this"."

We have included these corrections. Please note that the manuscript has been through copy-editing and will be copy-edited again before publication. We have changed the title to: 'Testing the effects of basic numerical implementations of water migration on models of subduction dynamics'

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