Response to Referee report 2 on
Improving subduction interface implementation in dynamic numerical models

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We thank the referee for their critical reading of the manuscript, which raised a number of important issues and has helped us to clarify the central arguments in the study. In this reply, the comments of the referee (R2) appear in `typewriter font`, with our responses following directly beneath. Note that in our updated manuscript the figure order has changed slightly. Unless otherwise stated, figure numbers here refer to the first submitted manuscript version.

General comments

`This paper explores different parameterizations of a weak layer that is used to model one-sided subduction. It particularly explores the controls on the width of this weak layer and finds that, within the context of the models presented, there is a preferential width. In general I think this is a nice, albeit technical, contribution to the literature describing fully dynamical modeling of subduction zones. I have a few somewhat broader comments and a fair bit of more detailed comments.`

A continuously-entrained weak layer (WL) is a common strategy used in numerical models to provide decoupling between the slab and upper plate. In the models presented we show that significant thickness variations develop when a uniform thickness WL is initially prescribed. We argue that changes in the volumetric flux in the weak layer lead to these spatial and temporal variations in thickness. The comment that a preferential width is found 'in the context of the models presented', seems to imply that our results are model specific and not generalizable. In later parts of the review, R2 suggests that mesh resolution might be a contributing factor to the observed thickness variations and that such effects would be mitigated with increasing resolution.

We feel that these suggestions are made without any real justification, and do not critically engage with the argument we make. Our explanation for the evolution of the interface thickness invokes one main assumption, which is a first-order characterization of the kinematics of the WL, namely that uniform flow of material on the incoming subducting plate transitions to simple shear (Couette flow) within the decoupling region. We show that the interface evolves to an equilibrium thickness profile close to that predicted under this assumption. To help further clarify these processes, we introduced a simple boundary-driven example (e.g. fig. 5) which shows analogous thickness changes in a weak horizon due to a changing boundary condition and concomitant gradients in downstream volume flux. Hence, we have already gone to some effort to show that our analysis reveals an intrinsic behaviour of this kind of flow.

Of course, depending on other aspects of the model setup (e.g. the subduction interface rheology, use of adaptive meshing), width variations of the WL will effect the evolution of the simulation differently. However, the width variations themselves are unlikely to be specific to our models, nor are they an artifact of low resolution. We feel that we may not have communicated these arguments as clearly as we could have, and have therefore re-written substantial parts of Section 5.1.

Specific comments

`Section 2 contains a fair bit of information that was already mentioned in the introduction or other superfluous. A bit of careful editing can shorten this section and make it clearer.`

Based on comments from all reviewers, we made minor adjustments to this section, with some simplifications.

`The citations in the text dont seem to follow any chronological or alphabetical order (see example in P4.l9-l11).`

This has been remedied.

`The weak fault seems rather wide (10 km as mentioned on page 7). If you have a 2 km grid resolution then that it is probably a good idea to smear it out over such a distance but what are your thoughts about FEM grid refinement around this fault to be able to bring it to more realistic thicknesses?`

We chose a WL initial thickness that was representative of recent long-term numerical subduction models, e.g. (e.g. Čížková, Hunen, and Berg 2007; Garel et al. 2014; Agrusta, Goes, and Hunen 2017). Again, as our paper is methodological, we are content with using a model setup that is appropriate for the problem. Of course, in the highest-resolution models we showed (e.g. element width of $\sim 2.5.$ km) we could resolve a thinner interface. But we doubt that doing so would alter any of the conclusions of the study.

Based on our analysis of the resolution convergence, we would suggest that the ratio of prescribed WL thickness to element size should be greater than three. This is not to say that a model with less resolution won't produce physically consistent subduction, however it may not show satisfactory resolution convergence. Testing resolution convergence is obviously the best way to ascertain this for a specific setup. We argue that in the WL approach, there is a component of 'wasted' resolution that is required only to properly resolve the transient phase of interface adjustment. This is why, we argue, the error accumulation tends to decrease significantly at around the time the interface thickness reaches quasi-equilibrium (see Figure 13.).

`I wonder if the improvements you find when choosing the initial and maximum width of the weak zone to be 20 km is just a matter of the numerical resolution (mesh and particles) that you use. Maybe you need at least 10 elements in this zone for things to stabilize. You mention that a lot of small wavelength variations disappear compared to using 10 km. This might be a symptom of discretization issues...`

The improvements we mention (using the EF approach) are specifically improvements relative to a standard WL setup. We assessed both implementations at several resolutions. We showed that the EF approach has better resolution convergence. This means that as we increase the resolution, the error (relative to a high-resolution reference model) is always smaller in the EF approach than in an equivalent WL approach. We expect that further adjustments to the EF approach (or similar strategies) could improve this further. R2 is correct to point out that the EF method always starts out with a higher effective resolution in the decoupling region - hence the comparison between the EF and WL approaches does have a limitation.

The EF approach has two separate features, one is the imposition of a variable initial thickness, the other involves controlling the maximum and minimum thickness. Both of these are targeted at different behaviours we observed in the WL model. The first tries to limit the amount of transient adjustment of the interface thickness, the second helps to dampen the short-wavelength instabilities at the boundary of the subduction interface and the upper plate. We could, of course, investigate these aspects individually. However, our main goal here was to design and communicate an approach that achieves better outcomes (e.g. better resolution convergence) for a given computational expense. We think that the EF approach achieves this goal.

`It would be very helpful if you could repeat your exercise with higher resolution. I suspect your maximum width will go down with increasing resolution. In 5.3 you do a divergence test but I am not sure that this is very meaningful given that you show that coarser resolution meshes lead to worse results. It is far more exciting to see what happens when you start converging.`

As we discuss in our reply to the preceding comment, there are some limitations in the convergence (divergence?) test, and therefore some ambiguity in how these tests are interpreted. We hope that we have already answered these criticisms satisfactorily. The second point made here by R2 is that the pattern of width variations in the WL approach might be resolution dependent, and that such processes would be mitigated with increasing resolution.

Our reply here essentially follows from our answer to the comment in paragraph 1. The width variation are not likely to be a dependent on resolution or 'context' (see paragraph 1), because the width variations are an intrinsic outcome of the fluid dynamics (see reply to paragraph 1).

The referee has provided no physical argument to back up the expectation that these processes should be resolution dependent. Hence, we are skeptical that it would be helpful to reproduce our results with higher-resolution models.

We accept that we may not have communicated these arguments as clearly as we could have, and have therefore re-written substantial parts of Section 5.1.

`Figure 11. I am not certain it is useful to show under-resolved results.`

In this case we feel it is useful, because the style in which the models degenerate actually reveals something about the way that the interface thickness is dependent on the kinematics of the flow. In particular, when the interface is under-resolved and does not adequately decouple the plates, it is the induced partial-coupling that drives further thinning of the interface. This feedback cycle is a limitation of the WL method, and may be very relevant when additional processes are included - such as subduction of buoyant crust / continents.

`P1 footnote 1. why footnote?`

We have removed the footnote.

`might be good to add a few original references as to why the slab-mantle wedge coupling appears to start at 80 km (that seems to be the case in most SZs; please just clarify why we think this is the case)`

We have cited the paper of Wada and Wang 2009. In the scope of this study we feel this sufficient.

`P2.l5. typo`

Typo fixed.

`P2.l25. typo`

Citation fixed.

`P2.l25-27. Full sentence here. You can probably rephrase this a bit better as in that sediments may be important amidst various other issues controlling plate velocities.`

We have reorganised this paragraph.

`P2.l33. 10s -> tens`

Changed.

`P3.l12-14. I am confused about this full sentence here. The seismogenic zone is generally not characterized by ending in the mantle (maybe between slab crust and mantle, but not slab mantle and mantle).`

We have simplified this section.

`P3.l14. Average stresses: where?`

The value we quote (from Lamb 2006) is based on an average stress taken along the entire subduction interface.

`P3. line starting at l22. This seems to take a single point of an antigorite-out boundary as some extreme. Its a bit more variable/complex than that. You can probably delete this sentence.`

We have reconfigured this paragraph substantially based on the comment.

`P4. Figure 1. last line 'representative' of what?`

Figure 1 has been changed and captions and labels redone.

`P4.l14 missing article`

Changed.

`P4.l5 missing plural`

This paragraph has been condensed.

`P4.l9 typo`

Changed.

`P5.l11 typos`

Fixed.

`P5.l25. What is 'over-resolving?`

We mean providing resolution larger than is necessary to resolve an interface of a given thickness. This would ensure that the interface remains adequately resolved when the interface thins (as occurs in the deeper part of interface under WL approach - e.g. Fig. 4).

`P5.l30 just benchmarking`

Changed

`P5.l31. '1 km, 20K' Not sure what this means. The benchmark study referenced here showed that finite element models agreed on temperature predictions along the slab surface to within about 1K for 1 km resolution. There were some finite difference models that had larger differences.`

We have shortened the paragraph and excised this sentence.

And here and below. It is just 20K not 20°K.

Fixed.

`P7 three sentences starting at l20. Dont you say essentially the same thing here? You might be able to condense this into a single sentence.`

We have simplified this paragraph.

`P8.l1 I hope a cosine taper is easy to implement in many subduction models. Is it not?`

Past studies have used different approaches to effectively inhibit the subduction interface from extending to arbitrary depths. In the context of the Lagrangian particle method, we could simply (arbitrarily) replace the weak layer material with background material at a given depth. We choose a rheological transition, based on a cosine taper, to allow a somewhat graduated change.

`P9.l12ff. Maybe use subscript ‘conv?`

We have simplified the mathematical symbology in this section.

`P9.bottom. MDD reference is repetitive`

Changed.

`P10.l6 You are citing Figure 8 way out of order`

Yes. We have omitted this reference.

`P10.l21 typo`

Fixed.

References

Lamb, Simon (2006). “Shear stresses on megathrusts: Implications for mountain building behind subduction zones”. In: *Journal of Geophysical Research: Solid Earth* 111.B7.

Čížková, Hana, Jeroen van Hunen, and Arie van den Berg (2007). “Stress distribution within subducting slabs and their deformation in the transition zone”. In: *Physics of the Earth and Planetary Interiors* 161.3-4, pp. 202–214.

Wada, Ikuko and Kelin Wang (2009). “Common depth of slab-mantle decoupling: Reconciling diversity and uniformity of subduction zones”. In: *Geochemistry, Geophysics, Geosystems* 10.10.

Garel, Fanny et al. (2014). “Interaction of subducted slabs with the mantle transition-zone: A regime diagram from 2-D thermo-mechanical models with a mobile trench and an overriding plate”. In: *Geochemistry, Geophysics, Geosystems* 15.5, pp. 1739–1765.

Agrusta, Roberto, Saskia Goes, and Jeroen van Hunen (2017). “Subducting-slab transition-zone interaction: Stagnation, penetration and mode switches”. In: *Earth and Planetary Science Letters* 464, pp. 10–23.