Response to Referee report 1 on
Improving subduction interface implementation in dynamic numerical models

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We thank the referee, Dr. Duretz, for the extensive and detailed comments on our manuscript. These have helped us to define the scope of the study more clearly, and also to the express some of the technical details more succinctly. In this reply, the comments of the referee (R1) appear in `typewriter font`, with our responses following directly beneath.

General comments

`This study aims at providing a new approach to model long-term subduction processes, particularly at the plate interface. The authors investigate the widely-used weak layer (WL) approach and identifies some of its limitations. In this light, they propose an alternative approach, termed embedded fault (EF), and show that this approach remedies some limitations of the WL approach. This work is interesting because many geodynamic modelers studying subduction processes are facing these issues. The outcome of this study (i.e. the EF approach) might help subduction modelers designing their models. It will surely help informing the community about the caveats of subduction modeling. That said, I was personally not convinced of using the proposed EF approach. The essence of this approach is to remap the geometry of the plate interface at each time step of a simulation. The idea is thus to overrule the geometries predicted by the numerical simulation in order to facilitate interplate decoupling. I would personally not encourage code users to interfere within simulations by using ad hoc rules. I would rather expect an alternative solution that would not require interfering with an ongoing numerical simulation. This would necessitate an augmented modeling framework, by accounting for thermo-mechanical feedbacks (e.g. Thielmann and Kaus 2012) hydrological (e.g. Dymkova and Gerya 2013) or chemical processes) or an advanced rheological model (e.g. Bellas et al. 2018). This is likely beyond the scope of the current study, however this is a fundamental issue. and these aspects are so far not really discussed. Prior to publication, I would hence recommend the authors to enrich their discussion - potentially around the above-mentioned points. I found the discussion very short and mostly restricted to the difference between WL/EF approaches. I would also encourage the authors to make sure that the figures are cited in increasing order. Youll find below a list of comments and suggestions.`

The summary of R1 acknowledges that our study may help subduction modelers designing their models. We are encouraged by this reflection, as this was certainly one of our major intentions. The primary criticism of R1 here is somewhat philosophical - we would argue - and involves an alternative view on the best way forward for *improving subduction interface implementation in dynamic numerical models*. Referring to our proposed Embedded Fault strategy, R1 proposes 'an alternative solution that would not require interfering with an ongoing numerical simulation. This would necessitate an augmented modeling framework, by accounting for thermo-mechanical feed-backs, hydrological or chemical processes) or an advanced rheological model...'

R1 advocates the development of a more self-consistent approach to modelling the subduction interface, in which a range of feed-back mechanisms, including hydrological processes are implemented. In response, we simply suggest that there are valid reasons for pursuing both strategies. The suggestion of R1 likely involves significant increase in the complexity of models. Moreover, it is not clear that such as strategy is presently capable of allowing long-term, stable, buoyancy-drive subduction to operate. In cases where the aim of the subduction model is to understand, for instance, the long-term dynamics of the slab, such complexity may be undesirable. In other words, where a deliberately simplified representation of the subduction interface is desirable.

We also point out that the WL approach generally represents an ad hoc strategy, rather than an attempt to directly model the complex (multi) physics of the subduction interface zone. Given the ad hoc nature of WL implementations, it seems sensible to at least develop a more thorough understanding of the behavior and characteristics of that approach. This is precisely what our study intends to do.

The points we have briefly covered here have been integrated into the revised manuscript, particularly in Section 3.

Specific comments

`p.1 l. 8 - What is 'fully dynamic'?`

This term has been used in the literature to refer to a numerical subduction model driven only by internal buoyancy forces, rather than being additionally forced by velocity boundary conditions. In a number of places we have simplified to this phrase to 'dynamic models'. The term fully dynamic is clarified in Section 4.1.

`What are the requirements for a model to be 'fully dynamic'?`

See previous comment.

`p.2 l.5 - 'geodyanamics'`

Typo fixed.

`p.3 l. 15 - What is 'full thermal modelling'? How can a thermal model be 'full'?`

The word 'full' was unintended here, now corrected.

`p.5 l.9 - 'an solution'`

Typo fixed

`p.5 l.11 & p.5 l.25 - brackets around citation,`

Syntax fixed

`p.5 l.26 - a more important issue is when the interface locally thins out and becomes unresolved`

We agree that this is an important issue. We discuss this in Section 6.

`p.5 l.31 - ?`

Syntax fixed.

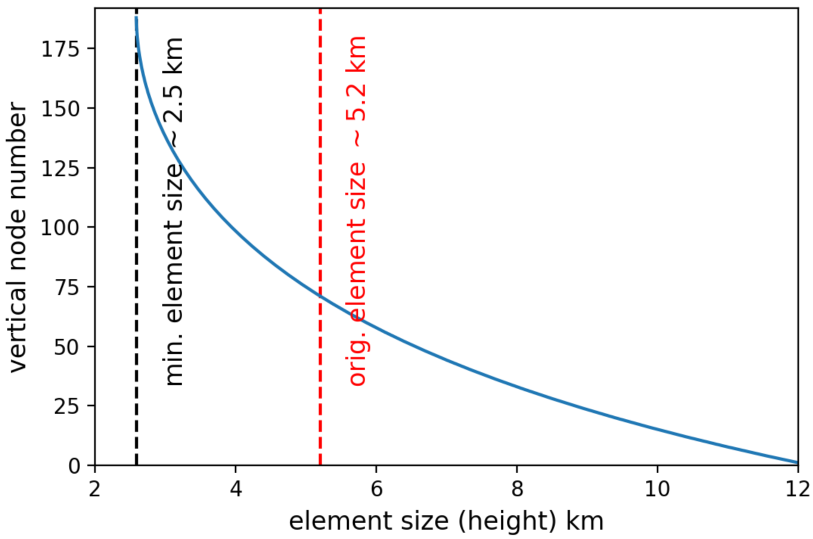

Figure 1: Vertical coordinate shows the vertical node number, for a 192 node mesh, with 192 being the surface node. Horizontal coordinate shows the element size. Blue line show the vertical element size in our highest resolution model.

`figure 1 caption: The layer representing the subduction interface appears to have viscosity variations (top-left) while it is described as having a constant linear viscosity.`

The rheology associated with the weak layer is merged with the background (plate/mantle) rheology at a significant distance (800 km) away from the trench on the subducting plate. The main reason for doing this is so that the weak layer does not extend all the way to the spreading ridge. This has been clarified in section 4.1. We think it is still appropriate to refer to the subduction interface as having a constant viscosity.

`Is figure 1 relevant at all, I dont see it called in the text?`

Figure 1 has been significantly changed and is now referred to in the main text. We think that it is relevant to provide context for the type of simulations we are discussing, as well as to clarify some of the SZ terminology that is used throughout the paper.

`p.6 l.15 - psuedo-brittle`

Typo fixed.

`p.6 l.24- what function you use to refine the mesh?`

Given a initial mesh with N uniformly spaced nodes (y_i), we shift the nodes to new locations (y_i') using an normalised exponential function:

$y_i' = (y_i - 1)e^{\lambda(\bar{Y}_i^2 - 1)} + 1$

Where $\bar{Y}_i$ is the normalised position

$\bar{Y}_i = \frac{-y_i - y_N}{y_N - y_0}$

The constant $\lambda = 0.7$. Fig. 1 shows the element spacing based on this function.

`p.6 l.24-what is the horizontal resolution?`

All models have an aspect ratio of 5 which is reflected in the mesh resolution. So the highest-resolution models (192 elements in vertical dimension) have 960 elements in the horizontal dimension.

`p.6 l.30 - on all side`

Clarified.

`p.6 l.33-34 - not clear whether left and right hand side wall are treated similarly.`

The left and right hand side wall have the same boundary condition, this has been clarified.

`figure 2 caption: Since you mention normal velocity to be zero at boundaries, you may also add that the tangential shear stress is zero.`

We have added this information, and fixed some typos in the Figure 2 caption.

`p.7 l.3 - 'Past studies...' I would add references or delete this statement.`

Statement deleted.

`p.7 l.8 - 'Throughout this study...' this sentence reads weird or incomplete.`

This section has been rewritten.

`p.8 l.1 - 'In this chapter' - is this part of a thesis or report?`

Fixed this mistake.

`p.8 l.4 - Fig. 8 is called right after Fig. 3`

Removed this figure reference.

`From Section 4.3 and Figure 3 caption, it is not entirely clear what the principle of EL is. Do you mean that: (1) you pre-define a channel geometry that will remains constant during model evolution (2) you remap, at each timestep, material types based on their relative position of the particles with regard to the channel? If yes, please state it clearly.`

We mean (2). Section 4.3 and Figure 3 caption have been largely re-written to make this clearer.

`p.8 l.7 'In a number of previous studies...' Do you mean variations in space or time? No sure whether the referencing is sound.`

Primarily we mean spatially. We cannot say with certainly whether temporal variations are present in the studies we refer to, as this level of detail is not provided.

`p. 10 l. 20 'effected'`

Typo fixed.

`p. 10 l. 21 'these feature'`

Typo fixed.

p. 10 from l. 25 on. Better write $W_{\min}$ instead of W_min. Same for 'max' and 'init'

Fixed.

`p. 10 l.30 please add a scale on this figure`

This figure has been updated with a scale.

`p. 10 l.30 'physically inconsistent' do you mean geologically irrelevant? Is this so irrelevant by the way?`

We mean, in a qualitative way, geologically irrelevant. The morphology shown in Figure 7. (to which p. 10 l.30 alludes) is dissimilar to current subduction zones on earth. One could make the argument that the extremely long forearc region (~ 500 km) is also seen in some flat slabs settings (although arc volcanoes may not actually be present in those regions). However flat slabs usually have a subduction hinge with relatively normal curvature, whereas at greater distance from the trench the slab dip reduces and the curvature changes sign. In contrast, the model shown in Fig 7b, the length of the forearc is due to the extremely low curvature, but there is no slab dip reduction (flattening).

`p 10. l.34 - how would a free surface affect this behaviour?`

While a free surface is likely to change some aspects of subduction dynamics, the variation in interface thickness - seen in the WL approach - would still be expected as the argument relating to variation in volume flux remains the same. We cannot guarantee that a model with a free surface would behave identically when the interface is not allowed to thicken (e.g. as in Fig 7 b).

`figure 6 caption: what are red and black dashed lines? Which model is depicted here? The model presented in Fig. 7a or 7b?`

Figure caption has been updated. The model depicted here is the same as in Fig.1a and Fig. 1b. (in the revised manuscript). This has been noted in the caption as well.

`p.12 l.10 - reduce the amount the transient adjustment`

We're unsure what the comment is.

`p.12 . l.16 - 'One advantage of the EF approach is that it offers improved precision in determining the thickness of the subduction interface.' This is confusing, I thought the EL approach was aiming at imposing this thickness. How can it help to determine a thickness when it already imposes it?`

Yes, by imposing a maximum and minimum thickness, we gain precision in the knowledge of its thickness, don't we?

`p.12 l.17 - 'Such precision will be important for studying highly pressure- and temperature-sensiti processes, such as metamorphism and melting near the slab top.' Well, sure. This new parameterization will pre-define everything related to it.`

See previous comment.

`figure 7: very difficult to appreciate the dimensions, a scale is missing. What is the grey line? I think the notion of 'physically-consistent subduction morphology' does not make sense.`

Figure 7 has been substantially updated along with the caption. We have also incorporated R1's suggestion on 'physically-consistent subduction morphology', both here and in the main text.

`figure 8: How is this MDD monitored? Do you measure the stress differences across the plate interface?`

The MDD is monitored by evaluating the vertical component of the velocity at a distance 5 km above the top of the slab (in a slab-normal sense). The vertical velocity clearly reveals the location of the onset of coupling.

`figure 9: do you use the same scale in x and y? figure 9 caption: 'effected'`

Figure 9 has been updated and typo fixed.

`figure 10: at what time do these snapshots correspond? Do you use any particle reseeding?`

Snapshots are at 12.5 Myr, after the WL interface thickness has reached its quasi-equilibrium profile. This is now recorded in the figure caption. Particle reseeding is used to maintain regular particle numbers in the elements.

`figure 10 - 11: Given the fact that the mesh is not distorted, adding element edges would help the reader to realise how well the interface is resolved.`

True, but as the mesh is quite high resolution we feel it would also detract from the clarity of the image. We have tried to convey this information (i.e. the interface resolution) where necessary.

`p.15 l.9 - you mentioned you ran simulations with '72, 96, 128, 160 elements'. You can also include 192 elements as I understand.`

We did, and this has been corrected.

`p. 16 l.5 - 'better' sounds very qualitative. You mean that low resolution models using a WL fails at capturing plate decoupling.`

Agreed. We have expressed this differently.

`figure 13: I dont understand, why do repeated models at the resolution of 192 produce any error? If the models were repeated you should obtain the exact same results, dont you?`

The only difference in the models is the randomness in initial particle locations, as well as the repopulation of those particles. We would note that the error, or drift, is still quite small. We are not aware of a similar experiment previously being published, so we cannot comment on whether this value is unexpectedly high.

`p. 20 l.10 - a mistake here, a mean stress is not lithostatic. It can be split into dynamic and lithostatic components.`

This mistake has been corrected.

`p. 22 - Why using a lithostatic pressure field in the viscosity expression. Are obtained numerical solution of the actual pressure field not accurate enough to be used in the rheology?`

This is not indicative of poor results in the pressure calculation, but rather the assumption that the dynamic contribution of the pressure is expected to have to little effect on the viscous flow law compared to the lithostatic pressure contribution. In contrast, the dynamic pressure component is used in the determining the yield surface (Drucker-Prager criteria). The reviewer is correct that it would be more consistent to use the full pressure, however we doubt that the results of this study would be changed.

References

Thielmann, Marcel and Boris JP Kaus (2012). "Shear heating induced lithospheric-scale localization: Does it result in subduction?" In: *Earth and Planetary Science Letters* 359, pp. 1–13.

Dymkova, D and T Gerya (2013). "Porous fluid flow enables oceanic subduction initiation on Earth". In: *Geophysical Research Letters* 40.21, pp. 5671–5676.

Bellas, Ashley et al. (2018). "Dynamic weakening with grain-damage and implications for slab detachment". In: *Physics of the Earth and Planetary Interiors* 285, pp. 76–90.