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Interactive comment on "Improving subduction interface implementation in dynamic numerical models" *by* Dan Sandiford and Louis Moresi

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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

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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. ad 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. You'll find below a list of comments and suggestions.

Best regards, Thibault Duretz

p.1 l. 8 - What is "fully dynamic"? What are the requirements for a model to be "fully dynamic"? p.2 l.5 - "geodyanamics"

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

p.5 l.9 - "an solution" p.5 l.11 - brackets around citation p.5 l.25 - no brackets around citation p.5 l.26 - a more important issue is when the interface locally thins out and becomes unresolved p.5 l.31 - & ?

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. Is figure 1 relevant at all, I don't see it called in the text?

p.6 l.15 - psuedo-brittle p.6 l.24- what function you use to refine the mesh? p.6 l.24- what is the horizontal resolution? p.6 l.30 - on all side p.6 l.33-34 - not clear wether left and right hand side wall are treated similarly.

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

p.7 l.3 - “Past studies. . .” I would add references or delete this statement. p.7 l.8 - “Throughout this study, . . .“ this sentence reads weird or incomplete.

p.8 l.1 - “In this chapter” - is this part of a thesis or report? p.8 l.4 - Fig. 8 is called right after Fig. 3

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.

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

p. 10 l. 20 “effected” p. 10 l. 21 “these feature” p. 10 from l. 25 on. Better write W_{min} instead of W_min. Same for ‘max’ and ‘init’ p. 10 l.30 please add a scale on this figure p. 10 l.30 “physically inconsistent” do you mean geologically irrelevant? Is this so irrelevant by the way? p 10. l.34 - how would a free surface affect this behaviour?

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

p.12 l.10 - “reduce the amount the transient adjustment” 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?

p.12 l.17 - “Such precision will be important for studying highly pressure- and temperature-sensitive processes, such as metamorphism and melting near the slab top.” Well, sure. This new parametrization will pre-define everything related to it. 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 8: How is this MDD monitored? Do you measure the stress differences across the plate interface?

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

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

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.

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

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

figure 13: I don’t 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, don’t you?

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

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?

References

Marcel Thielmann, Boris J.P. Kaus, Shear heating induced lithospheric-scale localization: Does it result in subduction?, Earth and Planetary Science Letters, Volumes 359–360, 2012, Pages 1-13,

Dymkova, D., and T. Gerya (2013), Porous fluid flow enables oceanic subduction initiation on Earth, Geophys. Res. Lett., 40, 5671âĂŘ5676doi.

Ashley Bellas, Shijie Zhong, David Bercovici, Elvira Mulyukova, Dynamic weakening with grain-damage and implications for slab detachment, Physics of the Earth and Planetary Interiors, Volume 285, 2018, Pages 76-90,

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