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Discussions

Interactive comment on "Can subduction initiation at a transform fault be spontaneous?" *by* Diane Arcay et al.

Anonymous Referee #2

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General comments:

The study presented in this manuscript addresses the issue of spontaneous subduction initiation via a parametric numerical study. The question of how subduction initiates is of great importance in geodynamics, as it touches on the core of plate tectonics. To date, this study is the most comprehensive parameter study I have seen. By varying a large number of material and model parameters that may have a potential impact on the occurrence of subduction initiation, the authors delineate the physical parameters that result in old plate sinking (OPS). Results show that spontaneous subduction initiation OPSe at a transform fault is very unlikely at present Earth conditions. This result is not entirely new, as the difficulty of initiating subduction has already been pointed out by other authors (e.g. [McKenzie, 1977; Cloetingh et al., 1989; Mueller and Phillips,

1991]). With the exception of [Mueller and Phillips, 1991], these previous studies did not specifically address subduction initiation in a transform fault setting. The amount of numerical models that have been conducted for this study and the wealth of information about the influence of different parameters that have been investigated add an important new perspective on the issue of spontaneous subduction initiation and make this manuscript suited for publication in Solid Earth.

The introduction is structured in a clear manner. In the model setup section, I would suggest some rearrangements to make it more concise (see comments below). Most importantly, a large fraction of the results is described in the model setup section. I strongly suggest moving this description to a separate results section.

The results are presented in a two-fold manner: first, all simulations that do not exhibit OPS are described in detail. Several different regimes are identified. After that, all simulations that exhibit OPS are described and two different modes are identified. After that, model limitations are explored and results are compared to natural examples. The structure of sections 2-4 where model setup, descriptions of the results and discussion are mixed makes it at times hard to follow the paper and should therefore be improved. Additionally, the language needs improvement, as sentences are often phrased in a confusing manner.

I think that the manuscript would benefit greatly from an additional section (to be included after the Introduction) that explains the basic physics/mechanics of OPS, similar to what is done in the study by [Mueller and Phillips, 1991]. In my opinion, this would make it much easier for the reader to understand the influence of the different physical parameters that have been varied in this study. The authors do a good job deciphering the impact of each investigated parameter on OPS, but I think the combination of different parameters is most likely as important. A section which explains the potential interaction between forces resisting and promoting OPS at the outset of the paper could be used to discuss the interplay between the different parameters.

Specific commments:

Abstract:

In my opinion, the study does not really represent a completely "new" exploration of the spontaneous subduction inititation concept, as there have been quite a few numerical studies looking at subduction initiation at transform fault. What distinguishes this study from other studies is the extent of investigated parameters.

Introduction:

The introduction is well written and concise. It contains both information on natural candidates for spontaneous subduction initiation as well as an overview of existing numerical studies. In section 1.2 I am missing references to [McKenzie, 1977; Cloetingh et al., 1989; Mueller and Phillips, 1991]. In particular, [Mueller and Phillips, 1991] should be referenced.

Model Setup:

2.1:

As this is a numerical paper, I would suggest stating the governing equations in the beginning for completeness. Personally, I also prefer the numerical description not to be the first part of the model setup, as the numerical code is simply a tool to solve the governing equations for a given model.

For this reason, I would suggest to move the description of the numerical solution (method, number of tracers, resolution) to the end of the Model setup section (maybe after section 2.4.) and focus on the governing equations including the rheology. In my opinion, it would also be good to include a description of the boundary conditions (they are only depicted in fig.2). I was also missing a description of how density is computed in the model, which should be added in the model setup section (potentially together with the governing equations). In geodynamical models, it is also common to introduce viscosity cutoffs to avoid numerical problems. Were any cutoffs used here? If yes, this

information should also be included.

2.2 :

p.6, l.6: ... overestimates a bit ... What is "a bit"? This seems to be a vague statement. Could you provide numbers?

p.6 ,l.8: Where does the factor 0.75 come from? Is there a reference that compares the heat flow from such models to observations?

p.6,l.9: Assuming a constant temperature gradient between the surface and z_lb seems to be at odds with the assumption of half space cooling, which was used to determine the lithospheric thickness. How do you justify the use of such a thermal gradient? As the temperature field will have a significant impact on the viscosity structure of the lithosphere, assuming such a thermal gradient will result in an overall stiffer lithosphere, which could potentially have a large impact on OPS.

2.3:

eq.(1) Is there a particular reason why you chose the Byerlee criterion instead of a Mohr-Coulomb criterion?

p6,l.26: Could you add a reference to justify the way you approximate the brittle strain rate?

p.7, eq.(3): Is there a particular reason why you use the lithostatic pressure in this equation and not the total pressure?

2.4:

I would suggest to use "Model geometry" instead of "box composition" in the title. As the choice of test parameters is of particular importance in this study, I would also suggest to merge the description of the model geometry together with the description of the initial thermal structure and merge the choice of tested physical properties with section 2.5.

When it comes to the description of the investigated physical parameters, I was missing a bit the motivation for the specific choices made. For example, why did you choose the density of the TF as a parameter to be investigated? Is there any field evidence for such variations? Also, I was wondering why the properties influencing the ductile strength of the lithospheric mantle were not considered at all here. As the lithospheric mantle makes up a large part of both the old and young plate, I would expect that it may have a significant impact on OPS. I am aware that this would add a large number of additional parameters to the existing study. For this reason, I think it is important to clarify why only the brittle parameter was changed for the lithospheric mantle and not any other parameters. I m aware that some of this motivation is given later in specific subsections, but while reading the manuscript, these questions arose for me when reading section 2.4. For this reason, I would suggest to remove the description of the choice of tested physical properties from section 2.4 and merge it with section 2.5.

2.5.2:

p.9,l.5: You mention here that densities are a function of temperature in the model. This should be mentioned in the model setup section.

2.5.3.

p.9,l.8-10. This sentence should be rewritten as it was very hard to read. I understand that you rescale the activation energy to account for the changed value of the stress exponent. I may have missed it, but I did not find any corresponding expression in Dumoulin et al. (1999). If I read correctly, they also use a different form of the rheological law. Could you therefore clarify how the activation energy rescaling is done?

p.10,l.2: "Still as a weakening mechanism..." Which weakening mechanism do you refer to? Why would a weakening mechanism imply a low activation energy?

2.5.4

p.10, l.10: Here, an introduction to first results is given. As this subsection still belongs

to the Model Setup section, I feel that a new section "Results" is needed.

2.6:.

In general, this section is well structured. In terms of comprehensibility, I would suggest to use the exact same terms for the different regimes as are used in fig.4. This would make it easier to relate the description in the text to the figure. I would also suggest to color code the boxes (or something equivalent) in figure 4 according to the percentage of simulations that show the respective behaviour.

2.7:

p.11, l.13: Here the authors correctly state that OPS occurs when driving forces overcome resisting forces. Is there any way to estimate those forces beforehand for all simulations? As you have all the input, I think a rough estimate should be possible. Doing so would in my opinion add a very important aspect to the paper, as it would give us a better insight into the physics of the OPS problem. An estimation of those forces following the lines of [Mueller and Phillips, 1991] should be enough here.

p.11, l.30: "...very probably..." should be replaced with "most likely".

2.8.2:

p.15, l.2: "... is supposed to be localized..." I think the authors rather mean "... is localized...".

p.15, l.3: "... crust weakening laterally spreads out away from the TF..." I did not quite understand what the authors mean here. The sentence sounds as if they include a kind of weakening process in the models, which is not the case. I think the authors are referring to different simulations where they vary L_w? In this case, they observe a switch from YP vertical subduction to a gravitational instability. In this case, I think not only the extent of weakened crust plays a role, but also the chosen upper boundary condition (free slip), which inhibits plate sinking. The authors shortly discuss this issue in section 3.3. However, I think it has to be taken into account here that the mechanical impact of the weak crustal material may be overestimated due to the choice of the upper boundary condition. I think it would be enough to run a single simulation with "sticky air" to see if this is the case or not.

2.8.4.

Here the authors state that an additional weakening of the lithospheric mantle is required to allow for OPS. This is a very important point in my opinion, as it highlights the importance of the lithospheric mantle in this process. Could the additional weakening not also arise from a weaker ductile rheology?

p.16, l.8: Here it is stated that some simulations were also run with a lowered activation energy for the lithospheric mantle. I may have missed it, but I could not find any reference to the supplementary beforehand. I think it would be helpful to state before that a large number of additional simulations were run to test other physical parameters and that you chose to only focus on some of them.

2.8.5

It is interesting that a plume-like thermal anomaly does not trigger any OPS in the simulations presented here, but seems to be a very important process in other studies (e.g. [Burov and Cloetingh, 2010] [Crameri and Tackley, 2016] [Stern and Gerya, 2017] and others). Is it potentially related to melting processes (which are not modeled in the simulations presented here?) I think this issue is worth discussing.

2.8.6.

This is a very interesting section, as you list additional parameter that might have an influence on OPS, but did not turn out to have a first order effect. Together with the results from section 2.8.4. ,this indicates that the strength of the lithospheric mantle may be crucial in enabling OPS. For this reason, I think the potential effect of mantle rheology should be discussed more, e.g. with respect to other rheologies such as low temperature plasticity. Additionally, the hinge may be weakened by e.g. grain size reduction

and thus a switch to diffusion creep could potentially help to initiate OPS. I am not saying that you should run additional simulations, but a more detailed discussion would be nice to highlight this issue. What you could do is to extract the effective viscosity in the hinge, which should be affected by brittle failure for low values of gamma_m. This should give you an estimate of the effective strength of the lithospheric mantle that is needed for OPS. You could then discuss which processes or parameters other than brittle failure could result in such effective viscosity values.

3 Analysis

I was not sure why you started a new section here, as you continue to describe model results. I would therefore merge this section with the description of previous model results.

3.1:

Judging by the title, the question of which parameters result in OPS is the main focus of the manuscript. Therefore sections 3.1 to 3.3 are in my opinion the most important results sections. For this reason, I would suggest to not refer to figures in the supplementary only, but to move some figures from the supplementary to the main part of the manuscript to better illustrate the distinction between mode1 and mode2 OPS.

3.3:

I liked that this section summarizes the different parameters and classifies them into resisting and promoting OPS. As suggested above, I would move part of this discussion to a separate section after the introduction where the basic physics/mechanics of the OPS process are explained (following the lines of [Mueller and Phillips, 1991]).

p.18, l.19: the necessity of a low brittle yield strength in the mantle is discussed her. In my opinion, weakening of the lithospheric mantle does not necessarily have to occur via brittle failure, but may also be due to different weakening processes, such as shear heating, grain size reduction and/or fluid infiltration. Additionally, a different creep mechanism such as low temperature plasticity could be crucial to weaken the lithospheric mantle. However, I think that this discussion should take place in the actual discussion section and not here.

p.18,l.23: The free surface/free slip discussion should also be moved to the discussion section. Moreover, I am not really convinced by the arguments here that a free surface/sticky air approach would result in similar results. It is true that models with a weak crust and a free slip upper boundary condition show similar kinematics compared to models with a free surface/sticky air layer. However, I have the feeling that the importance of the strength of the crust is overestimated in the models shown here, as it not only resists bending, but also has to decouple the plate from the upper boundary. As a stick air layer is relatively simple to implement, a few simulations should be enough to show whether this is correct or not.

3.4:

p.19, l.10: Actually, the initiation process can be very fast in models without a prescribed weak zone when elasticity is included, as elastic stresses within the lithosphere are released at initiation (see e.g. Thielmann & Kaus (2012)). However, these simulations studied subduction initiation under compression, thus it is not clear if the same would happen for the model geometry used in this study.

p.19, l.13: I do not completely agree here that elasticity only plays a minor role in the OPS process. [McKenzie, 1977] did show that elasticity may play a major role in this process, although his assumptions may have overestimated the impact of elasticity (see also discussion in [Mueller and Phillips, 1991]). As the models in Farrington et al. (2014) already start with a downward pointing slab, the initiation of free subduction is not fully included in their model, which is why I think it is difficult to draw any definite conclusions for the initiation of OPS from their simulations. Their study shows however, that the stress field in the hinge of the subducting plate is significantly altered if elasticity is included, in particular close to the surface. To me, this indicates that the

importance of crustal parameters, in particular the brittle parameter of the crust may be overestimated when elasticity is not considered. However, this is just a hypothesis and only further studies could shed more light on this issue. In any way, I don't think that the influence of elasticity should be dismissed.

Discussion

4.1 This section is clear. I would add the discussion points from previous sections here.
4.2 This section is also clear. The high plate velocities observed in the simulations after subduction initiation are indeed quite large and may be a result of the chosen mantle rheology. However, as this manuscript is focused on the subduction initiation stage, I feel that this topic has to be left for future work. As it is anyway still debated whether the Yap subduction zone initiated at 20 Ma or whether it initiated earlier, it is reassuring that the simulations do not support its spontaneous initiation. I also do not find it surprising that subduction initiation due to OPS is not very probable, as earlier studies had also already hinted at this.

Conclusions

The conclusions sum up the main results of this study quite well. Although it may seem to be a negative result, I think it is very important to show that OPS is not easy to achieve at present day conditions (within the model assumptions). I would also add that the results highlight the importance of weakening processes within the lithospheric mantle, as these may significantly contribute to the occurrence of OPS.

Tables

Table 3: Would it be possible to group the different simulations according to the resulting deformation regime? I think this would make it easier to grasp the influence of the different parameters.

Additional references

Burov, E. B., and S. Cloetingh (2010), Plume-like upper mantle instabilities drive subduction initiation, Geophysical Research Letters, 37, L03309.

Cloetingh, S., R. Wortel, and N. Vlaar (1989), On the initiation of subduction zones, Pure and Applied Geophysics.

Crameri, F., and P. J. Tackley (2016), Subduction initiation from a stagnant lid and global overturn: new insights from numerical models with a free surface, Prog. in Earth and Planet. Sci., 3(1), 30, doi:10.1186/s40645-016-0103-8.

McKenzie, D. (1977), The initiation of trenches: a finite amplitude instability, Island Arcs, Deep Sea Trenches, and Back-Arc Basins, 1, 57–61.

Mueller, S., and R. Phillips (1991), On the initiation of subduction, JGR, 96, 651–665.

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