Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-88-AC2, 2020

Interactive comment on "Impact of upper mantle convection on lithosphere hyper-extension and subsequent convergence-induced subduction" *by* Lorenzo G. Candioti et al.

Lorenzo G. Candioti et al.

lorenzo.candioti@unil.ch

Received and published: 4 August 2020

We gratefully thank the reviewer for the very constructive criticism. Implementing the suggestions helped to better visualize the simulation results, focus on the main findings and significantly improve the manuscript. During the review process, we have changed the structure of the manuscript significantly. In the results section of the revised manuscript, we present the evolution of the reference model and the wet olivine model separately. The results of the remaining models are presented in comparison to the results of the reference run for the distinct deformation stages. We then discuss the implications of our findings on several aspects, such as for example the impact of

the viscosity structure on the convection, the onset of convection and the impact of convection on subduction, in the discussion section. The order of the figures and the style of visualization has been adapted accordingly.

Below we have answered to all the comments from the reviewer. Our answers marked with an "A:" and are below the original comments.

Major points: 1. Clarity of the manuscript. The model is very complex and it has lots of details, but the authors haven't always explained the concepts clearly or properly. I have written down some specific examples that the authors can fix easily. However, they should try to verify that their findings are backed by arguments that are explained in a logical way. The abstract should be shortened to include the top 3 most important results, and be revised for clarity and shorter sentences. For example, what do the authors want the paper be known/cited for?

A: The Abstract has been shortened and reformulated during the review process in order to address the reviewer's comment.

In the introduction, the link between mantle convection and lithosphere deformation is quite abrupt (with a sentence about age of the Earth that is irrelevant to this study). The question 'why is convection important?' is not satisfactorily introduced or linked to coupled lithosphere-mantle deformation.

A: Convection regulates the long-term temperature and mechanical structure of the lithosphere: the strength of the lithosphere is inter alia temperature dependent. Thus, convection may have a direct impact on the deformation of the lithosphere. Coupling convection to lithosphere deformation in numerical models can therefore improve our understanding of lithospheric scale processes, such as rifting and subduction. Also, convection can generate forces due to up- and down-welling of mantle material, which can affect lithosphere deformation. A paragraph has been added to the introduction for better explanation.

General suggestion: too many commas. Try to rephrase/split sentences with more than 2 commas or that are longer than 2 lines.

A: This has been implemented in the revised version of the manuscript.

2. Results section. I think the reference model (M1) should be described separately (evolution between extension, relation, convergence). Then compare models M2-M6 with M1 to highlight the effect of various factors. Figures should be adapted accordingly. The reason for this are the following: - in current form, the comparison is all over the place and it is confusing. It is not very clear which simulations the main text is referring to sometimes. - the current arrangement of figures is random. It starts with 2, 5, 8, 4, 10 etc. Their placement should follow a logical order of arguments.

A: We have changed the structure of the manuscript as follows: (1) Results section: (i) The evolution of the reference run and M6 is now described separately (two standalone figures) (ii) M2-5 are then compared to M1 at each deformation stage. (iii) Figures have been modified and reordered accordingly (2) Discussion section: We have restructured the discussion section and are now discussing implications of the models for several geodynamic problems including: (i) Spontaneous vs. Induced subduction initiation (ii) Mantle convection stabilising single-slab subduction (iii) Onset of upper mantle convection and thermo-mechanical evolution of the lithospheric plates (iv) Impact of mantle viscosity structure and effective conductivity on passive margin formation

The comparison between M1-M6 should be done in terms of Ra. The k, viscosity cutoff, flow laws, they essentially affect the Ra.

A: We have largely implemented this suggestion in the revised version of the manuscript.

3. Thermal softening. A quick search in the manuscript finds 'thermal softening' only in the abstract, very late discussion and conclusion, yet it is suggested as a key process that controls subduction initiation. I'm pointing out that it is incompletely described

and linked to the hypothesis of the study and results. For example, Line 425: thermal softening is introduced only now. not clear why bring it up here? Line 461: say that structural and thermal softening are important, but they were intro- duced late, without much context. Moreover, the authors suggest in multiple places that it is the structural softening (in- heritance) and convection (slab suction) that help initiation. The authors need to clarify what are the main findings, and arguments need to be revised. One finding that I think is important: the required driving force to initiate subduction is much smaller, when convection and structural inheritance are considered.

A: We have discussed thermal and structural softening in more detail in the revised version of the manuscript.

4. Modelled vs parameterised convection. In Line 126, 3 types of simulations are introduced: 1) model convection with a weak asthenosphere, 2) parametrised convection, by scaling the thermal conductivity to the Nusselt number 3) impact of different viscosity structures First, the treatment of the mantle convection is not clear in the main text (Lines 124- 130). What drives convection? How is the applied parametrised convection different? When is the onset of convection? Is convection only during the thermal relaxation stage? What controls the size of the convection cells? Also, explain how the Ra_avg is calculated.

A: This has been addressed during the review process. The Rayleigh number is calculated locally at each grid point. Ra_avg is the arithmetic average of all local Rayleigh numbers > 1000. We have clarified in the revised version of the manuscript.

In point 3) above which approach are you using: modelled/parameterized convection? While it is explained better in Appendix B, the differences between them are not clear in the main text. For example, 1) would be M1, while 2) is M, and 3) is M6?

A: This was explained in lines 130-135, but we changed the numbering of the models in the revised version of the manuscript, for clarity.

5. Other questions. The geodynamic cycle modelled: 1) 30 Myrs extension at 2cm/yr 2) 70 Myrs thermal relaxation 3) 20 Myrs convergence at 3 cm/yr What is the motivation behind these choices: 1) why thermal relaxation 2) why those time intervals 3) why those extension/convergence rates? Also, what are the boundary conditions during thermal relaxation?

A: The aim is to model the opening of a ca. 400 km wide oceanic basin without formation of a mature oceanic crust in an ultra-slow to slow spreading rift system. The durations of the periods and boundary conditions are chosen to allow for comparison of model results to orogens that formed from the collision of magma-poor hyper-extended margins, such as the European Western and Central Alps. We have clarified this in the revised version of the manuscript.

Why the choice of those parameters to change?

A: The viscosity structure of the mantle is poorly constrained and has a direct impact on the convective flow of the mantle. The effective conductivity approach is used to stabilise the thermal field in numerical simulations, but its impact on the deformation of the rigid plates and on self-consistent subduction initiation has not been tested yet. These statements have been added as a motivation in the introduction.

Are the surface processes important? Have you run models without? Do they introduce further heterogeneities in the model that affect the outcome?

A: Testing in more detail coupled surface processes to the deformation in the lithosphere and convection in the upper mantle is beyond the scope of this study. We have included only a simple parameterisation of surface process into the model to avoid unrealistically high and low topography. This has been clarified in the revised version of the manuscript.

6. Subduction initiation. It seems like symmetric vs asymmetric spreading also controls to a large extent subduction initiation, whether it is single/double subduction. I feel very

little discussion is about that, and more on structural and thermal softening.

A: Subduction is always initiated during convergence. Most inheritance from the spreading is restricted to the margin geometry and is thus structural. Localisation occurs in the lithospheric mantle beneath the margins due stress concentration at the beginning of the necking zone. The heterogeneity in the upper mantle introduced by the convection and, therefore, the suction force of downward directed material flow seems to influence whether there is single or double-sided subduction. This has been clarified during the review process.

Also, there are other previous efforts to model extension/compression to obtain structural inheritance and subduction initiation (i.e. Gulcher et al 2019). The authors discuss simpler treatments of subduction initiation in paragraph 280, but do not relate to newer efforts to avoid the use of artificial features. So, are these newer models better for studying subduction initiation?

A: (Gülcher, Beaussier, & Gerya, 2019) investigate detachment faults as potential weak zones for intra-oceanic subduction initiation, not at a passive margin as wee do. Nevertheless, such detachment faults might be a geologically observable weak zone at which subduction could be potentially initiated. This topic has been addressed in the introduction of the revised manuscript.

Minor points: Line 8-10: revise sentence

A: We have incorporated this suggestion in the revised version of the manuscript.

Line 10: only from the abstract it is not clear what the parameters were used, so saying that a viscosity of 5e20 Pa.s was used (as compared to what?) is not very meaningful. Rephrase

A: We have rephrased the abstract in the revised version of the manuscript

Line 20: multiple use of 'geodynamic' in the same sentence Line 29-30: rephrase.

A: We have rephrased this sentence in the revised version of the manuscript.

Line 31: while it is an interesting fact - the calculation of the age of the earth - is not very relevant to the manuscript.

A: We have deleted this sentence in the revised version of the manuscript.

Line 35: rephrase

A: We have rephrased this sentence in the revised version of the manuscript.

Line 40: unlikely to be problematic

A: We have changed this accordingly in the revised version of the manuscript.

Line 41: delete likely

A: We have changed this accordingly in the revised version of the manuscript.

Line 55: authors relate to numerical aspects such as time step size, without mentioning why? The context was on physical aspects of convection.

A: This has been moved to the appendix in the revised version of the manuscript.

Line 64-65: should be in the first paragraph of introduction

A: This suggestion has been implemented in the revised version of the manuscript.

Line 68: Why only upper mantle? This is discussed late in discussion (section 4.4, paragraph 395)

A: This has been moved to the introduction section in the revised version of the manuscript.

Line 69: delete 'of applying'

A: This has been changed in the revised version of the manuscript.

Line 74: revise sentence - its meaning is not clear to someone who hasn't read the

methods/results section.

A: We have restructured this part of the introduction in the revised version of the manuscript.

Line 88: reference to the code how it was benchmarked? (Info in appendix A, but should be in the main text too)

A: The benchmarks have been moved to the main text in the revised version of the manuscript.

Line 89: rephrase

A: We have rephrased this sentence in the revised version of the manuscript.

Line 96: repeats with Line 92, also Duretz et al 2016/2016a?

A: This has been changed in the revised version of the manuscript.

Line 105: based on the sentence the crust should be: 3*5+4*5 = 35 km thick. But a sentence earlier it is 33km

A: This mistake has been corrected and the description of the initial configuration has been clarified in the revised version of the manuscript.

Line 103: what is the mathematical expression for the perturbation? in case the model needs to be reproduced?

A: We have added the mathematical expression for the marker field perturbation to the algorithm description in the appendix of the revised version of the manuscript.

Line 111: more details on the rheology? Indicate appendix A for reference

A: We refer to the appendix in the revised version of the manuscript.

Line 113: reference to "corresponding laboratory flow law estimates"?

A: References are given in the footnote of table A1.

Line 113-114: rephrase. i.e. The mantle lithosphere is rheologically stronger than the mantle asthenosphere due to the temperature gradient.

A: With this sentence, we want to emphasize that we used the same material parameters for both the lithospheric and the upper mantle. We therefore keep the phrase as it is.

Line 120: what is the motivation for alternating between calcites and pelites for sedimentation algorithm?

A: To account for changes in sediment strength due to changing sedimentary environments. A detailed investigation on the impact of different implementations of surface processes is beyond the scope of this study. We have clarified this in the revised version of the manuscript.

Line 130 - give reference to Table 1.

A: This has been adapted in the revised version of the manuscript.

Line 133: viscosity cutoff for M1 is not provided to understand the difference.

A: The cut-off value is given in table 1.

Line 134: realistic value? Are the other values not realistic?

A: A thermal conductivity value of 36 is not realistic for a peridotite at upper mantle temperature and pressure conditions.

Line 148: Figure 2 -> can define a variable F = 2xtau_II

A: We have introduced a variable for the plate driving forces in the revised version of the manuscript.

Line 160: introduce the horizontal driving force per unit length, but what is it proxy for?

A: It is a proxy for the strength of the lithosphere and it indicates how much force is needed to localise deformation. Has been clarified during the review process.

Line 168: you can't see to a depth of 660 km as indicated

A: Figures have been adapted accordingly.

Line 195: values

A: This sentence has been rephrased during the review process.

Line 197: what is the delta GPE showing? (Info given in appendix)

A: The gravitational potential energy (GPE) has been explained in the appendix. Delta means that it is the difference of the GPE compared to a reference value, commonly the value close to one of the boundaries.

Line 224: reference to fig 9a, yet that figure is for extension stage. Paragraph 220-225: confusing.

A: This has been addressed during the review process.

Line 228: what is mechanical heterogeneity? increases the strength of the weak layers

A: We have clarified this in the revised version of the manuscript.

Line 233: breaks later than the continental. after gives the impression of location.

A: This suggestion has been implemented into the revised version of the manuscript.

Line 234: what do you mean ' Mantle convection does not establish as early as rifting and crustal separation.'?

A: This sentence has been clarified in the revised version of the manuscript.

Lines 243, 245: use of realistic. close to the Ra estimated for the Earth. Line 250: which modulates mantle velocities.

A: We have implemented this suggestion in the revised version of the manuscript.

Line 250-254: why the discussion on time step size (a numerical feature) here? Lines

255-257: which simulation results are the authors referring here?

A: This has been addressed in the revised version of the manuscript

Paragraph 258: reference figure 5e,j in this paragraph. Also, maybe plot density averages in passive margins/exhumed mantle separately?

A: We have tried to modify this figure, but the figure becomes too busy.

Line 272: you jump from density differences to values of tectonic forces. An additional sentence needs to connect them (i.e. estimate the buoyancy force due to modelled density differences). How much is needed to initiate subduction? (a similar calculation is done in line 315)

A: This depends on the strength of the lithosphere, which is still subject to debate.

Paragraph 280: this should come before the Cloos 1993 paragraph

A: We have restructured the discussion accordingly during the review process.

Line 286: yes, but under convergence

A: When a major weak zone is imposed, one also has to push from the sides to initiated subduction. Only when you skip the process of subduction initiation and already assume the presence of an inclined slab, subduction continues freely (given that the initial slab is long enough and that boundary conditions have been chosen correctly).

Line 294-295: total convergence is double sided, while in M1 is single-sided (asymmetric). Not clear why subduction initiation is stable only in M1. Convection cell size important? how about thickness of lithosphere at the point? M2-M5 are quite symmetric and they all have Ra_avg âĹij1e5, while M1 has Ra_avgâĹij1e6. That should have an effect.

A: Likely the distribution of cells is important: the more asymmetric the mantle flow the more the model tends to produce single-slab subduction rather than double-slab

subduction. Asymmetry decreases with decreasing Ra. This has been explained in more detail during the review process.

Line 312-315: - suction force induced by down-welling in the convection cell in M1. What is the similar force in the other simulations?

A: It is likely similar in M2-3 compared to M1, because of similar density distributions and vertical velocities, but it has not been computed here. Due to higher temperatures induced by the effective conductivity, values for densities are lower in M4-5 compared to M1-3, which explains the reduced absolute speed of material in the convecting cells. Most important seems to be the asymmetric distribution of the cells. We have clarified this during the review process.

Line 330: not sure what the reference is for. The double-subduction term was not coined by those workers.

A: We cited this reference, because they also modelled double-sided subduction.

Line 332: sentence not clear. Which simulation are you referring? would say M2-5 are more or less symmetric double subduction

A: Has been reformulated during the review process.

Line 343: onset of convergence - unclear when this happens?

A: This happens at 100 Myrs. We have clarified this in the revised version of the manuscript.

Line 357-361: use of 'realistic'

A: This has been rephrased in the revised version of the manuscript.

Paragraph 395: this paragraph should be in the methods, as it motivates/explains your model domain until 660 km. The sentence 'The convective patterns simulated in our study are in agreement with these observations.' is irrelevant because you don't model the lower mantle.

A: This paragraph has been moved to the introduction in the revised version of the manuscript.

Paragraph 410: this should come earlier - I had questions about it earlier. on previous work on subduction initiation.

A: This paragraph has been moved to the introduction in the revised version of the manuscript.

Line 418: most definitely will have an impact

A: We have implemented this suggestion in the revised version of the manuscript.

Paragraph 430: and melting

A: We have accounted for this suggestion in the revised version of the manuscript.

Line 444-446: rephrase/simplify.

A: We have rephrased the conclusion section in the revised version of the manuscript.

Figures and Tables: Table 1: thermal conductivity should be 'k' without the 'th' subscript. The authors can also provide the formula for the Ra number in the main text. How was the Ra_avg calculated?

A: This suggestion has been implemented in the revised version of the manuscript.

Table 2: there should be a column 'Description' to describe the meaning of each parameter i.e. 'rho0' - reference density. Use k instead of k_th for thermal conductivity. What is dry/wet mantle? I assume wet mantle applies only to M6? Plastic and elastic parameters are also listed. Not very clear in the main text.

A: Description of the parameters is given in the appendix section. We also moved the table to the appendix for better comprehension. Dry and wet refer to the rheological parameters for dry and wet olivine. In the references given below the (now) appendix

table 1 it is explained in which model we use either wet or dry rheologies.

Figure 4: why plot the vertical velocity field separate from the horizontal? should plot arrow/streamlines field to see the convection cells.

A: The figure has been adapted accordingly in the revised version of the manuscript.

Figure 10: should be merged with Figure 4. One column velocity, one column temperature.

A: These two figures have been merged in the revised version of the manuscript.

Figure 5: What if you plot the profiles at the rift axis (within a distance) vs off-axis on either flanks of the rifts? Caption: g-j show enlarged areas.

A: We have tested this version of the figure, but it becomes too busy, unfortunately.

Figure 6: legend: temp contours are red.

A: We have corrected this mistake in the revised version of the manuscript.

Figure 8: the line plots are not entirely clear. Maybe use a dotted line instead of dashed line? and same thickness.

A: We have changed line style and colour for the figures in the revised version of the manuscript.

Appendix A Line 481: that's a strange notation of i,j indices (Einstein notation). Eq A2: if written in Einstein notation, then vectors are written in terms of scalar components (a_i should not be bold). Same in Line 482 a=[0,g]. -> revise this appendix for completeness of sentences, and explanation of all parameters. For example, what is Ap, tauP etc. Gamma value? in eq A10

A: We have clarified these points in the revised version of the manuscript.

Appendix B Paragraph 531: rephrase

A: This suggestion has been implemented in the revised version of the manuscript.

Line 560: not clear

A: This has been clarified in the revised version of the manuscript.

Appendix C Line 595: gamma_T=1? Line 601: g=10ËĘ4?

A: To perform this benchmark one has to apply a local Rayleigh number of 100 at the top and using the Frank-Kamenetskii approximation a local Rayleigh number of 10ˆ7 at the bottom. Choosing the dimensionless values as it is done here matches those numbers and reproduces the desired pattern with diagnostic quantities that are in the range of values reproduced by other algorithms as tested in (Tosi, et al., 2015).

Bibliography Gülcher, A. J., Beaussier, S. J., & Gerya, T. V. (2019). On the formation of oceanic detachment faults and their influence on intra-oceanic subduction initiation: 3D thermomechanical modeling. Earth and Planetary Science Letters, 506, 195-208. Tosi, N., Stein, C., Noack, L., Hüttig, C., Maierova, P., Samuel, H., . . . Glerum, A. (2015). A community benchmark for viscoplastic thermal convection in a 2âĂŘD square box. Geochemistry, Geophysics, Geosystems, 16(7), 2175-2196.

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-88, 2020.