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

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GENERAL COMMENTS

The manuscrit "Impact of upper mantle convection on lithosphere hyper-extension and subsequent convergence-induced subduction" by Candioti et al. presents 2-D thermo-mechanical simulations of successive geodynamic processes (continental extension, cooling with small-scale convection, convergence leading to single or double subduction initiation), with variations between the 6 models of either thermal conductivity parameterisation near the lithosphere-asthenosphere boundary, lower bound for viscosity, or rheological parameterization (wet vs. dry) of diffusion creep and dislocation creep in olivine.

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Discussion paper

Scientific significance: good

A motivation of the study is to model part of the alpine orogenic cycle, with continental rifting leading to a short-lived ocean that then subducts before collision. In my opinion, the most substantial contribution of this manuscript to the field of solid Earth dynamics is the novel model design that allows a self-consistent formation of structural inheritance at passive margins, which proves crucial for later subduction initiation geometry. As argued in section 4.5, this is a clear progress relative to the implementation of ad-hoc weak zones that do not capture the structural complexity related to the extension history. The paper also provides estimates of the force (per unit width) required to initiate subduction.

Scientific quality: fair

If the overall model design and parameterization seem valid, I have several issues with the model set-up:

- the fluid material is described as incompressible in section 2.1, but the thermal gradient is adiabatic, mantle densities vary greatly along the domain high due to phase transitions (Fig. A1) and the authors use the extended Boussinesq approximation for compressible fluid to solve the conservation equations (Appendix B line 579)

- phase transitions are implemented through the variable density (Fig. A1), but latent heat associated to phase change is missing from equation (A14), and this approximation should be justified.

- the side velocity boundary conditions during the extension or convergence phase (Fig. 1a,d) are likely to induce a sheared weak zone near the side boundaries at the transition depth between lateral inflow and outflow (340 km depth). Also, for the extension set-up, the suction created by the divergence in uppermost mantle is likely to generate a bulk ascending mantle channel in the middle of the domain (X=0 km).

- more generally, the flow pattern over the entire model domain is never shown when in/out flows are imposed at the sides, and this is an issue when discussing application to Earth (section 4.4): what is the geological justification that the divergence or convergence was not only active at the plates' surface but also across 300 km in the mantle below the plates? The justification of the side velocity boundary conditions should be developed, and the global flow pattern (velocity glyphs/arrow) should be shown during extension and convergence phases.

I also have other issues with the methods and results analysis:

- the choice of which input parameter are varied is not explained: why choose to vary the minimum viscosity between models M1, M3 and M5, rather than for example the initial extension rate, the duration of the thermal relaxation phase or the inflow/outflow side velocity profiles? The discussion does not well explain why there is a single subduction in M1, but double subductions in M3/M5.

- a methodology study on the comparison of "explicitly modelled convection" and "effective conductivity mimicking a convective heat flow" is inserted in the middle of the main geodynamics study. This hinders the continuous read of the paper, and I suggest all analysis and related figures of models M2/M4 are moved to Appendix B, along with the heat flow profiles of Figure 10.

- the simulations have numerous features that do not seem relevant for the scientific question (erosion, sedimentations of alternating calcites and pelites), but add yet another set of free parameters that make the interpretation of the simulations more complex.

- despite its central importance, the paper lacks a clear definition of "convection", that sometimes means "advection" or "drag" or "flow". The manuscript could also maybe reference the following papers dealing with the plate-asthenosphere interactions or subduction initiation or various scales of mantle convection:

L. Husson. The dynamics of plate boundaries over a convecting mantle. Physics of the

Earth and Planetary Interiors, Elsevier, 2012, 212-213, pp.32-43.

V. S. Solomatov. Initiation of subduction by small-scale convection. Journal of Geophysical Research: Solid Earth, 2004.

F. Lévy, C. Jaupart. The initiation of subduction by crustal extension at a continental margin. Geophysical Journal International, Volume 188, Issue 3, March 2012, Pages 779–797.

N. Coltice et al. Interactions of scales of convection in the Earth's mantle. Tectonophysics. Volume 746, 30 October 2018, Pages 669-677

Presentation quality: poor

This is a major flaw of the manuscript, which scientific contributions are hard to unearth because of confusing text and figure organization.

For example :

- showing vertical and horizontal velocity background colors (Fig. 4, Fig. 9) makes it difficult for the reader to visualize the flow pattern > could the authors show velocity glyphs or arrows, to better reveal e.g. the wavelength of small-scale convection in Fig. 4

- Figure 5 should be referenced in the methods section since achieving realistic temperature, density and viscosity model output is rather a constrain on the input parameters than a surprising result

- Figure 8 and 10 are referred to very early in the text, whereas they belong in the discussion (or appendix?) rather than in the results section same for text on lines 187-199,156-164

- Appendix A belongs to the main text, otherwise the parameters of Table 2 are not defined

- a time-bar could be included in Fig. 1 showing to scale the 3 stages of boundary conditions with colours corresponding to the velocity profiles shown in Figure 1a,d (also please add a null-velocity profile fo the thermal relaxation). The same time bar could then be put on other figure to know at a glance which stage the figures belong to.

SPECIFIC COMMENTS

- in the introduction, the authors should define what they mean by "convection" and discuss the different scales

- the exhumation of hot mantle (Fig. 2) is expected to lead to melting, please comment

- you need to support some statements with results/data, i.e. "Alternating activity of the subduction zones is observed." (line 219)

- if you mention the importance of apply a force rather than a velocity BC, then you should also mention the importance of setting the lateral flow in/out of the domain

- I am not sure you can compare your model to the Atlantic (line 272) since old oceanic lithosphere there is much older than in your models

- I disagree with the statement "the models are in a state of isostatic equilibrium at the onset of subduction initiation." (lnie 278): the convergence velocity and the topograpic low above the new trench (Fig. 8) suggest a dynamicl topography

- line 294-307: the explanation of the control of signe-sided subduction is not clear

- you cannot claim that "mantle convection seems active and largely confined to the upper region of the upper mantle. The convective patterns simulated in our study are in agreement with these observations." (line 399) since you impose the hight of your simulation domain to be restricted to the upper mantle.

- you should not boast that "the model has captured correctly the first order physics of the investigated processes." since model M6 shows the immense importance of rheology parameterisation - that is far from being constrained...

- what do you mean by "If convection in the mantle is suppressed by high effective thermal conductivities or high, lower viscosity limits" (line 453)? During the convergence phases, the mantle still flows in the domain (which is why you should show the velocity glyphs).

- did you try to run models without shear heating to estimate the relative role of structural vs. thermal softening for localization? (lines460-462)

- Figure 9 and Figure 2a,b: comment on the "slab-like" features between 100 and 200 km depth below the extended margins in M1 and M2

- Fig.8d: what is the X-locations and the depth range for integration of the second invariant of deviatoric stress tensor?

- explain in caption of Fig. 8 "values for tau_II remain constant when no deformation is applied to the system" whereas Fig. 4 shows large convection cells un the mantle that may deform the plates above

- appendix B: it is not clear why D should be thickness of the whole upper mantle whereas Fig. 4a,f shows small convection cells

- equation B1: how is effective viscosity average over the domain?

- the explanation on lines 554-561 is not convincing: what take a constant Rayleigh number that on D and on k and then claim that D and k can be adjusted?

- the isentrop is Fig. A1 does not match the temperature profile in Fig. 1

TECHNICAL CORRECTIONS

- "cooling" is more appropriate than "thermal relaxation" for stage 2

- the initial velocity condition is not given

- do you have more references for the "common approach" to indirectly include the effects of thermal convection ? (line 48)

- line 97-98 : what does "free slip with constant material inflow/outflow velocities" mean?

- 7 units of 5 km each make a thickness of 35 km (not 33)(line 104)

- line 104 : why describe a 87-km thick mantle lithosphere if all parameters are the same (line 114)

- Table 1 : please higlight (bold ?) which parameters differ from model M1 for all models.

- Table 2 : how are the column of the 2 sediments different? link with pelites/calcites or with sediments 1/2 of Figure 6?

- Table 2 : why no diffusion creep in the crust ?

- Table 2 : which rock are analogue for strong and weak crust?

- section 3.1.1: how do you define the length of the margin (threshold in crust thickness?)

- line 160: is the second invariant tensor of the deviatoric stress calculated for the whole lithosphere including the crust?

- why do you take the 10ˆ21 Pa.s contour as the base of the lithosphere? Why not take th 1350°C isotherm?

- line 176: give X-location of special flow fiel at 120 km depth

- line 184: why doyou claim that the lithosphere is delaminating whereas the iso-viscous contour is almost flat?

- line 207: define GPE

- line 223: Figure 8d rather than 8b?

- rephrase "In our models, subduction is initiated self-consistently, without prescribing any major weak zone or an already existing slab." (line 286) since they are weak heterogeneities in the passive margin

- line 319 "if shear stresses are negligible" = is that really the case at suduction onset?

- equation A3: define alpha and beta (which is different from the beta in Eq. B2 I guess...)

- Fig. 1a,d: the depth looks smaller than 680 km

- Fig. 1c: initial random perturbations look denser between -20 and + 20 km, is that the case?

- Fig. 3d: issue with the bottom of the plit nera -200 km (vertical grey line?)

- Fig. 5: dashed lines for M4 and M5 are barely visible, I suggest you use thick lines with other colours

- Fig. 8: it would be helpful to have label on the topopagray such as "trench", and to mark the subduction initiation in the timeline of Figure 8d

- Fig. 10: what is the new information brought by this figure compared to Fig. 4?

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