Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-88-AC1, 2020

Solid Earth
Discussions

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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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 are marked with "A:" and are below the original comments.

- 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)

A: The maximum value for the density time derivative is two orders of magnitude smaller compared to the velocity divergence. Also, (Bercovici, Schubert , & Glatzmaier, 1992) concluded that compressibility effects on the spatial mantle structure are minor when the superadiabatic temperature drop is close to the adiabatic temperature of the mantle, which is the case for the Earth. We therefore assume here that the Boussinesq approximation is still valid and suggest that density changes due to volumetric deformation are negligible. We consider only small density changes affecting the buoyancy stresses. Not considering adiabatic heating in the energy conservation equation leads to a significant deviation of the thermal structure from the initially imposed adiabatic temperature gradient over large time scales (>100 Myrs). The resulting vertical temperature profile (if adiabatic heating is neglected) is constant throughout the upper mantle and the newly equilibrated vertically-constant temperature is equal to the imposed temperature at the bottom boundary. In consequence, the density structure taken from the phase diagram table according to pressure and temperature values is wrong. By using the extended Boussinesq approximation, i.e., the adiabatic heating term is included in the energy conservation equation but not in the continuity equation, the initially imposed adiabatic (or isentropic) gradient is maintained over long time scales. The resulting density structure agrees well with the PREM model as shown in this study. A more detailed comparison between different approximations of the continuity equation is beyond the scope of this study.

- 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.

A: Indeed, latent heat released or consumed by a phase transition can perturb the thermal field by up to 100 K and induce a buoyancy force aiding or inhibiting the motion of cold subducting slabs (van Hunen, van den Berg, & Vlaar, 2001) or hot rising plumes. However, when the lateral differences in temperature do not vary much, the deflection of the phase transition by an ascending plume or a subducting slab has a much bigger impact on the buoyancy stresses than the latent heat released or consumed by the phase transition (Christensen , 1995). Also, most of the studies on the impact of latent heat rely on the assumption that density is temperature dependent only. In the models presented here, density is a function of temperature and pressure, which makes it difficult to estimate the impact of temperature changes due to latent heat near a phase transition on the buoyancy term a priori. Because a detailed parametric investigation of the impact of latent heat on buoyancy stresses for temperature and pressure dependent density is beyond the scope of study, we neglect latent heat for simplicity.

- 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:

A: As mentioned correctly by the reviewer, material inflow/outflow velocity boundary conditions may lead to a shear zone forming at the transition point between inflow and outflow. To avoid such boundary condition effects close to the mechanical lithosphere, we set the transition point deeper than the initially imposed lithospheric thickness. We chose z=-330 km as the point of transition, because values for deviatoric stresses at this depth are significantly smaller compared to those at the base of the lithosphere. The mechanical thickness of the lithosphere may vary greatly over time mainly due to temperature changes and is therefore difficult to constrain a priori. By setting the transition point of the velocity inflow/outflow boundary condition deeper than the initially imposed mechanical thickness of the lithosphere, we allow for self-consistent adjustment of the mechanical thickness of the lithosphere during the evolution of the model. We have added velocity arrows to many of our revised figures. These figures show that the thickness of the lithosphere, with respect to consistent horizontal velocities, is not controlled by the inflow/outflow boundary away from the model boundaries.

- 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: One aim of this study is to quantify the impact of convection in the upper mantle on the long-term extensionâĂŤcoolingâĂŤconvergence cycle of the lithosphere. Convection is controlled by the Rayleigh-number, which is a function of the viscosity of the convecting layer. Since the viscosity of the mantle is poorly constrained and estimated values vary by two orders of magnitude, this parameter is the most interesting for us to investigate. A frequently used technique to parameterize the impact of convection is the effective conductivity approach. This way the temperature field can be stabilized without having to calculate an enhanced convective velocity field in the mantle (which can be numerically challenging). We therefore compare this approach to the explicitly modelled convection, which calculates an enhanced convective velocity field in the mantle. The initial extension rate has been chosen to model the formation of an approximately 400 km wide basin containing exhumed mantle in an ultra-slow to slow spreading environment. The timing of the extension, cooling and convergence phases are motivated by data from the Alpine orogeny, which is now clearer explained in the revised manuscript. Testing the impact of different extension and compression rates as well as different cooling durations is beyond the scope of this study.

- 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.

A: The manuscript and result presentation have been re-structured based on the constructive comments of both reviewers. However, we keep the results of the simulations with an “effective conductivity” in the main manuscript, because these results are part of our main geodynamics study.

- 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.

A: We include erosion and sedimentation to avoid too high or low topography. To achieve this, we decided on one particular erosion/sedimentation model. A parametric study on the impact of different sedimentation/erosion processes or sediment transport mechanisms on the deformation of the lithosphere is beyond the scope of this study.

- despite its central importance, the paper lacks a clear definition of "convection", that sometimes means "advection" or "drag" or "flow".

A: The word "convection" can be used to describe the motion of any fluid. Convection in a fluid can either develop freely, via thermal or compositional variations, or it can be induced by external forces (Ricard, 2007). In mantle convection simulations, free material motion in the mantle can be initiated by for example buoyancy contrasts due to variations in temperature or chemical composition. A geological example for induced mantle flow is a rigid plate that moves on top of the mantle. These statements have been incorporated to the introduction during the review process.

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. Tectono- physics. Volume 746, 30 October 2018, Pages 669-677

A: We have added some of the references to the revised version of the manuscript.

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

A: We have added the velocity arrows to the figures. This was a very constructive comment for better visualization – thank you.

- 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

A: This has been done during the review process.

- 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

A: The figure order has been changed in the review process.

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

A: We have decided to shift the parameter table 2 to the appendix rather than describing all equations in the main text.

- 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.

A: We have tried to add the time bar, but the figures become too busy. We have changed the order of the figures and implemented much of the constructive comments.

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

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

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

A: This is explained in the introduction line 70 ff. and also in the methods section.

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

A: This has been done in the review process.

- 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

A: The timing of the deformation periods is chosen to allow for comparison with orogenies such as, f.e. the Alps. By choosing inflow/outflow we simulate the movement of the plates, decoupled from the rest of the domain, which we consider here more realistic than extending or compressing the entire side walls of the model domain with a height of 660 km.

- 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

A: . . . Even that old oceanic crust did not undergo spontaneous subduction yet, which was the point to mention it as an example here.

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

A: Indeed, this was our mistake: we chose the wrong time step to show the topography at the end of the cooling period. This has been updated during the revision. In general, it is also true that the system cannot be in isostatic equilibrium by definition, since there are deviatoric stresses holding the topography of the passive margins. However, the difference between the height of margins and the depth of the basin is ca. 5 km, which is the calculated topographic difference for an idealised block of 30 km thick crust floating on top of the mantle (Turcotte & Schubert, 2014). We therefore argue that the topography across the passive margin system produced by our model is close to isostatic equilibrium at the end of the cooling period.

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

A: The down-welling of two convecting cells meet directly below the margin at which subduction is going to be initiated later. The enhanced downward motion of upper-mantle material in this region likely exerts a suction force that assists in initiating and stabilising only one single-slab subduction.

- 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 height of your simulation domain to be restricted to the upper mantle.

A: We want to highlight here that we model a convective pattern that is observed in nature, namely convection in the upper mantle, that is below the lithosphere and above 660 km. Other people might argue that such “upper-mantle convection” does not exist and only “one-layered convection” of the entire mantle, down to ca. 2900 km, exists in nature. We confined our model domain using the justification that in the Alps the convection seems to be two-layered. Besides that, all models are confined to the upper mantle, but the reach of the convection cells extends to different depth, depending on the Rayleigh-number (compare M1 to M3). This is now more obvious when visualising the flow pattern with velocity vectors.

- 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...

A: As mentioned, we calibrated our initial model configurations in such a way that they match data from the PREM model and from GIA estimates. Starting from this point, we let the model freely evolve and do not change material parameters or geometries anymore. During rifting we generate margins of realistic first-order geometry, during cooling, the basin subsides to realistic depths and convection has realistic Rayleigh numbers, and finally subduction is initiated self-consistently (i.e. without imposing any

major triangular weak zone cutting through the entire lithosphere at the OCT) via thermal softening. Therefore, we argue that model M1 captures the first order physics of the extension-cooling-subduction cycle correctly. M6 shows a scenario in which the initial viscosity profile is not backed-up by natural data. This model becomes unrealistic and is not applicable to the present-day Earth after the rifting phase. Hence, this model does not capture the first-order physics correctly, because Ra-numbers are too high and the lithospheric thickness becomes too thin. We therefore show end-member models that either capture the first order physics of the present-day mantle correctly, or not.

- 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).

A: We want to say that the vigor of convection is significantly reduced, for example, absolute magnitudes of convection velocities are significantly reduced. We modified the text accordingly.

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

A: We did not run models without shear heating. (Jaquet & Schmalholz, 2018) and (Kiss, Candioti, Duretz, & Schmalholz, 2020) investigated the importance of shear heating for shear zone formation and subduction initiation. They showed that in absence of any other active weakening mechanism (f.e. brittle-plastic strain weakening) the deactivation of thermal softening results in large scale folding of the crust without localisation of a major shear zone. The geometry at the onset of convergence in the models presented here is similar to the initial geometry of (Kiss, Candioti, Duretz, & Schmalholz, 2020). We, therefore, expect similar behaviour for our model. Since we do not employ any other strain weakening mechanism in the models presented here, we rely on thermal softening for localisation of shear zones.

- 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

A: These features result from the convection cells that are already active.

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

A: The second invariant of the deviatoric stress tensor is integrated vertically over the entire domain. To estimate the plate driving forces we first average the vertically integrated second invariant of the deviatoric stress tensor horizontally over the left most and right most 100 km. Second, the estimated value for the plate driving force is computed as the average of the two average values obtained before. We have clarified this in the revised version of the manuscript.

- 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

A: We wanted to say: “when no far-field inflow/outflow deformation is applied”; we clarified the text. Values for tau_II are not equal to 0, because there are always deviatoric stresses, f.e. to sustain the margin geometry or shear forces induced by mantle flow at the bottom of the more-or-less rigid plates. However, there is no significant deviation from these “background” stresses when no far-field deformation is applied to the system. Thus, the values for tau_II remain relatively constant during the cooling period.

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

A: This paragraph has been rephrased for clarity during the review process.

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

A: In our study, we calculate a local Rayleigh-Number on each grid point. The average Rayleigh number is calculated as the arithmetic average of local Rayleigh numbers >1000.

- 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?

A: The goal of this exercise is to match a realistic Nusselt number for the Earth’s mantle by assuming a conductive heat flow through the upper mantle. This means that q_LAB is parameterized via an enhanced, conductive heat flux. To match the Nusselt number of the Earth’s mantle, this artificial heat flow has to be 13x larger than the realistic conductive heat flow of the mantle. We therefore enhance the thermal conductivity in the upper mantle by a factor 13.

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

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

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

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

- the initial velocity condition is not given

A: This been corrected in Fig. 1 in the revised version of the manuscript

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

A: They are given line 52.

- 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)

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

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

A: To indicate the initial depth of the LAB. We have explained it in more detail during the review process.

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

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

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

A: This has been changed in the figure legends.

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

A: At low temperatures, the strain rate is a nonlinear function of the stress, which suggests that the active deformation mechanism of the crust is likely dominated by dislocation creep. Diffusion creep is usually active at high temperatures and low deviatoric stresses and is therefore more important in the upper mantle.

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

A: The strength of crustal rocks depends on temperature, pressure and deformation rates. In the models presented here, the strength of the weak and strong layers should be regarded relative to each other. They represent a more heterogeneous crust, which is more realistic than a unified homogenous material for the entire crust. The weak layers represent for example silica-rich metasediments and the strong units represent for example mafic material (see also (Petri, et al., 2019) for more details).

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

A: Thickness reduction from original thickness to <10 km following (Sutra & Manatschal, 2012).

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

A: Yes, we clarified this in the revised version of the manuscript.

- why do you take the 10ËĘ21 Pa.s contour as the base of the lithosphere? Why not take th 1350âŮęC isotherm?

A: The viscosity contour remains horizontally straight, whereas the 1350 °C isotherm is deflected by the convection cells indicating mantle material flow rather than a rigid plate boundary. Above the viscosity contour the length of the velocity vectors is essentially zero, but below this contour line the convection cells are active. We have clarified this in the revised version of the manuscript.

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

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

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

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

- line 207: define GPE

A: A definition of GPE is given in line 197. A more detailed explanation is given in the appendix.

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

A: Yes, this has been changed in the revised version of the manuscript.

- 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

A: We argue that in our models, subduction is initiated “self-consistently”, because we do not ad-hoc prescribe any major weak zone cutting through the entire lithosphere at the passive margin. Also, in our initial model configuration, we do not impose any major weak zones, or seeds, to force mantle exhumation and separation of the continental crust. The heterogeneities at the passive margin have been modelled self-consistently within the same continuous numerical simulation. Also, the layered heterogeneities are only present in the crust and no major heterogeneities are imposed in the mantle lithosphere. We modified the text to clarify our statement.

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

A: Horizontally far away from the subduction zone, which is where we calculate the force, this assumption is valid.

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

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

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

A: Indeed, this is not clear enough in the model configuration: depth is -660 km, surface level is 0 km and +20 km are left free to allow for topography. This has been addressed during the review process.

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

A: This has also been clarified during the review process.

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

A: I do not see the vertical grey line. The comment is not clear enough.

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

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

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

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

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

A: Figure 10 shows the conductive heat flow of the entire domain. Whenever convection is modelled properly, the conductive heat flow in the upper mantle must be close to 0. In models M4, M5 the conductive heat flow is still high due to the enhanced thermal conductivity. This illustrates, that this approach mimics the convective thermal structure but does not capture the physical process of convection in the upper mantle correctly. We have combined figure 10 with figure 4 in the review process.

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