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Comment on se-2020-217

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Referee comment on "Buoyancy versus shear forces in building orogenic wedges" by Lorenzo G. Candioti et al., Solid Earth Discuss., https://doi.org/10.5194/se-2020-217-RC2, 2021

Review of the manuscript "Buoyancy versus shear forces in building orogenic wedges" submitted to Solid Earth - Discussions by Candioti and co-authors.
This manuscript investigates the effect of upper crustal rheology, serpentinite strength, and different density evolution models on the dynamic growth of orogenic wedges during continental collision. To do so, they use numerical models that include phases of extension, tectonic quiesence (cooling), and subsequent convergence leading to subduction and ultimately to continental collision. The pper is well written and the figures very nicely support the text. I have no major concerns that have to be addressed before acceptance for publication. However, I mention some minor suggestion below that might help improving the manuscript. Furthermore, I have some general concerns about the model rheologies that have to be more clearly explaind in the main text and the appendix (see below).
With this said, I recommend the manuscript to be accepted for publication in Solid Earth after consideration of minor revisions.
I hope my comments are helpful and constructive. Authors may feel free to contact me directly in case of unclarities related to my comment.
Best wishes
Jonas Ruh

General comments:

1) The model involves the mantle and therefore claims to have an advantage to crustal wedge models. That is a valid argument. But one point I want to address is the mantle rheology. Obviously, the mantle rheology should strongly affect the outcome of the models, and therefore, its implementation should be clearly introduced. The mantle rheology depends on various types of flow laws, including dislocation and diffusion creep. I spotted in the appendix table annotations that all mantle grain size is constant at 1 mm. This has a huge affect on mantle flow, as it results in close to 100% diffusion creep and much lower related viscosities than for pure dislocation creep. Grain sizes of 1 cm would already result in dislocation creep and actual grain sizes in the mantle are thought to be even larger based on a wealth of tomography papers and exhumed xenoliths. Now, I wonder why such a small grain size was applied, because it immensely affects the entire

model dynamics. What happens if diffusion creep is switched off?
A great example of the effect of diffusion creep is Fig. 5, where it can be observed that the lithosphere is 50 km thick everywhere and not restricted to less than 1300°C, which may be the temperature to enhance dislocation creep. One can also observe lithospheric dropping-type delamination or thermal erosion of the continental lithosphere, mainly driven by very low viscosities due to (unnatural) diffusion creep forced by very small grain sizes.
I don't say the authors should redo all models, but just explain better what they implemented and what the effects are of the implemented grain size, maybe refering to Jiao et al. (2017; JGR).

2) Also related to diffusion creep: Table A1 shows weird values. In the first part of the table it is stated that rheologies are taken from H+K2003, but the values are not correct (wet diffusion for const. OH: Q = 335, not 375 for example). Also, all r = 0.0 should be changed to r = -. Because r = 0 would change the A according to your equation for A(Pa^-n-r.....)

3) A general comment on using frictional / brittle / plastic. I also often write brittle-plastic, but I like it less every time I use this term. Of course, theoretically brittle processes are described by a viscous process in the numerical model and some authors use "plastic" for everyting that has a yield strength. However, brittle and plastic is still something different, where the latter describes a process acting at the molecular framework of crystals. I know that there is no exact definition on how to use those terms and I keep having arguments with many people from different fields, but anyway. Maybe my comment serves as the initiation of an interesting discussion. I'd write that the code is mimicking brittle deformation with a viscous implementation by reducing the viscosity based on a yield strength, or so.

Minor suggestions:

- The abstract is very long and very wordy. I had difficulties to follow it. You can easily delete some unnecessary information to better attract the reader to go on and read the entire manuscript.

- L34: I'd delete "-plastic"

- L39 and 41: Twice: "crustal wedge models have also been" in a row, sounds weird

- L44: May cite Jammes et al., 2012 after ... actual collision

- L47/48: indicate that the overriding lithosphere is meant. Of course, without this information is much stronger taking into consideration the second part-sentence. Of course, also your wedges have a plane décollement, but with the unclear sentence the reader might understand that lower plate lithosphere is detached and involved in the wedge. As is, the overridig lithosphere just forms a backstop...

- L50: likely significantly - rephrase

- L55: maybe mention "body forces" in contrast to "surface forces"

- L60/61: That is not true, of course they do, they have gravity and density. But it is not very important as in mantle-scale models

- L62/63: I don't understand. Tey don't consider shear forces at the interface? And what happens along the décollement? Shear strength along the interfaces defines these models, very similar as in the presented models.

- L63: what is lithosphere-upper mantle? lithospheric mantle? or lithosphere and non-lithosphere mantle? I'd use crust in constrast to mantle, and asthenosphere in contrast to lithosphere.

- L65ff: maybe refer to Fig. 1 in this paragraph

- L95: Brittle deformation

- L96: simplify instead of repeating. We do not apply any frictional nor viscous strain weakening

- L102: I couldn't find mica and calcite in the text, although they are introduced in the figures. Maybe also explain that mica is a weak inclusion at shallow levels and a strong inclusion at deeper levels (in contrast to quartz for example).

- L143: how did you come up with 18? I always found it very weird just to multiply the pre-exponent, but ok. You may just call it a stronger rheology. I don't know how much it still has to do with serpentine. But why 18? Any explanation in the text would be appreciated.

- Table 1: I would write in the annotations what 1 and 2 are, not only refer to another table in the appendix

- L165: 120-130 km??

- L166: the width of the left margin

- L183: sheared off

- L303: refer to Figure after "diagram"

- L338: The natural examples need a better introduction. I for example would be interested in the average shortening. Is there less exhumed material in the Pyrenees because there is less shortening? How does it compare to the model results?

- L346: refer to figure after "convergence"

- L353: delete "model", as in L339

- L356: maybe worth citing Cristina Malatesta's paper from 2012 in Lithos that investigates subduction of serpentinized oceanic mantle

- L373/374: because the lower crust is stronger? Then explain that it is because there is no decoupling at the Moho but a weak lower upper crust.

- L393/394: that is weird. The lithosphere just acts as a backstop could be argued. It does not involve the entire lithosphere in wedging (there are also crustal models with elastic beams depending on bouyancy: e.g., Stockmal et al., 2007; Fillon et al., 2012; Ruh, 2020).

- L395: the model by Platt is rather crustal. He appied it and compared it mantle-scale orogens

- L398-401: I see that the entire downgoing lithospheric mantle remains undeformed (except bending) beneath the crustal wedge. Hence, there is no strong variation in plate-parallel velocity. This is similar to apply a lower boundary condition to a crustal model...

- L411/412: as written, it sounds like delamination is also called roll-back..

- L522: introduce before that particles are applied. And state how many per cell etc.

- Table A1: Kohlstedf should be Kohlstedt