Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-134-RC2, 2020


Solid Earth
Discussions



# *Interactive comment on* "Thermo-mechanical numerical modelling of the South American subduction zone: a multi-parametric investigation" *by* Vincent Strak and Wouter P. Schellart

**Anonymous Referee #2**

Received and published: 16 September 2020

The authors present a parametric modeling study that aims to determine the physical subduction parameters associated with South American subduction. Overall, the figures are text are clear, the descriptions of the model behaviors are very well done, and the topic is interesting. However, I think there are some major issues with the study relating to i), the thermal weakening rheology adopted in a set of models, and ii), the overall study design. For the study to achieve its goals, I therefore think significant work is needed to re-think important aspects of the paper.

Specific comments



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My first concern relates to the exploration of a thermal weakening rheology. This is important as these are the most successful suite of models. Ultimately, I am not sure your rheological implementation corresponds to what you intend. In the rheology section, you first describe a standard T-dependent Arhenius flow law (Eq. 12) and mention that the lithospheric layers have variable viscosities controlled by composition (lines ∼185). In a compositional model, you would typically neglect the temperature dependence of the viscosity in the lithosphere (with the view that the compositional strengthening is mimicking this). Because you have both lithospheric composition and cold temperature, I am not sure how you dealt with this? (But I presume you did sufficiently as the viscosity field does look reasonable in the first non-thermal weakening figures). More importantly, "thermal weakening" is then introduced as another T-dependent viscosity (Eq. 14). This expression is just a linearized version of Eq. 12 and so I am not sure: why it is referred to as thermal weakening, how it is combined with the other two viscosities (compositional and Arrhenius), and what process it describes. Looking at the figures, it has the non-intuitive effect of lowering viscosities in the cold temperature lithosphere, particularly in the lower mantle (which doesn't agree with Eq. 14). What does this correspond to physically? (The studies that you cite – Ratcliff and Schubert (1996) and Zhong et al. (2000) - just use this flow law to approximate a regular temperature dependent viscosity which, in disagreement with your models, produces strengthening in cold regions).

My other concern relates to the design of the study. The goal is the determination of geodynamic parameters for future 3-D modeling from the current 2-D models. However, the parameters chosen for exploration are not well justified. Why do you focus on these three parameters? If you are just trying to nail down a geodynamic reference setup, there are many other parameters that could significantly influence subduction and are just as uncertain: e.g. slab strength and rheology, lower mantle strength, oceanic plate density (e.g. plateaus), upper plate rheology. I think you need to either explore a larger range of parameters or provide more robust justification for your choices. Second, trying to find a 2-D reference model for a very 3-D subduction system that exhibits strong along-strike variation (e.g. flat slab vs. no flat slab) seems challenging. I acknowledge you need a starting point for future 3-D models, and so it's probably worthwhile, but I think this produces extra concerns that should be addressed. For instance, one of the fit criteria is flat slab subduction. What about the regions that don't have flat slab subduction? Is a SAmerica reference model that produces flat subduction appropriate? (Especially given proposed links to buoyant oceanic plateaus in this region.) Also, at what latitude are the plate velocities extracted (Fig. 3) for comparison with models? Are they representative of the whole margin? For dips, you consider the along-strike range which seems very sensible. Perhaps a similar approach is also needed for the plate velocities?

Other comments

75-84: Relates to my first main comment but many studies consider temperature dependent slab viscosities (e.g. Garel et al., 2014, G-cubed) but, importantly, not in combination with a compositional slab viscosity.

100-105: If you are quoting a run times then you should also state the size of the model (e.g. total number of elements, degrees of freedom).

Eq. 5: Is compositional buoyancy just applied to the upper plate and not the subducting plate? I could not figure this out (also Lines 165-170).

∼240-250: You ignore the OP shortening component of vt. Fair enough, but could this be why you get a progressive decrease in vt (Fig. 3b)? If so, is it appropriate to match this vt trend with models that don't have significant OP deformation?

503: Factor 100 lower mantle viscosity increase is probably reasonable, but on the high end. Did you test a reduced value?

532: According to what reasoning are these crustal yield stress values reasonable? Refs?

644: Ra # increased by reducing mantle viscosity? Or increasing slab/plate density?







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673: What is this Gilbert paper? Not in reference list. If similar to the Cerpa work, they do not solve for the viscous mantle but parameterize it using edge forces on the slab. So not really fair to call these infinite slab-mantle viscosity ratio models.

Overall, the discussion is long winded and, in places, repetitive. Further effort to consolidate it around the main points would really improve readability!

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