# Response to comments from Anonymous Referee #2 on "Thermo-mechanical numerical modelling of the South American subduction zone: a multi-parametric investigation" by Vincent Strak and Wouter P. Schellart

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**Comment**
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.

**Response**
We thank Anonymous Referee #2 for his review work on our manuscript and for providing a critical analysis that will allow to improve and clarify the manuscript. Two main criticisms are raised by the reviewer.

1) The first point raised concerns the rheological implementation of our temperature-dependent viscosity to model thermal slab weakening. This is not a major concern since the issue may partly stem from a lack of clarity regarding our description of the distribution of different viscous rheologies and rheological laws in our layered setup (compositional and temperature-dependent), as well as from an omission in Eq. 14. Thus, this is partly a matter of clarification that we will address in a revised version of the manuscript (see detailed response below). In addition, the reviewer's comment may partly originate from his/her own preference in the way the rheological setup is built. However, in our opinion, there is not just one correct way to build a model rheologically and we think that our approach, in addition to the one suggested by the reviewer, is also valid and it has led to consistent published subduction models by other groups (see detailed response below).

2) The second criticism relates to the fact that we use a two-dimensional model setup to study a three-dimensional subduction system of which the investigated parameters vary along the trench, thus in the third dimension that is not included in our models. This comment is similar to the first main comment of Anonymous Referee #1. As discussed in our detailed response to the comments raised by the first reviewer, using a 2D model setup is not a major issue because it is appropriate to study the dynamics at the centre of wide subduction zones such as for South America. So in the manuscript we now put extra emphasis on the fact that we aim to model only the centre of the South American subduction zone (Bolivian orocline region), and not the segments to the north and south. This point was already mentioned in the methods section of the original manuscript (L98-101). In the revised manuscript we will make it clearer by adding text in the abstract, introduction, methods and captions of Fig. 2 and 3 (see detailed responses below and also in the response to the comments from Anonymous Referee #1).

**Comment**
Specific comments

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.

**Response**

This issue may have been raised partly due to a lack of clarity in the original manuscript and an omission in Eq. 14. The T-dependent Arhenius flow law of Eq. 12 is used only for the upper mantle to approximate the dislocation creep deformation regime and not for the lithosphere layers, for which we used homogeneous viscosity values (and a visco-plastic rheology for the top layer of the subducting plate). The only exception is for those models that model thermal slab weakening, in which we indeed included a simplified version of Eq. 12, giving Eq. 14. This was clearly stated in the original manuscript (L175-180 and L195-197):

“Apart from the subducting plate top layer, all other plate layers and the lower mantle are Newtonian whereas for the upper mantle both a Newtonian and power-law rheology were tested (Table 2). In nature, mantle rocks deform by dislocation and diffusion creep following an Arrhenius flow law with temperature and pressure dependence (Karato and Wu, 1993; Hirth and Kohlstedt, 2003). For dislocation creep, we computed a dynamic effective viscosity following the Arrhenius flow law but neglecting the effect of pressure (van Keken et al., 2008)”.
“In some models, thermal weakening of the slab is simulated using a non-dimensional temperature-dependent viscosity that follows a linearized Arrhenius flow law (Ratcliff and Schubert, 1996; Zhong et al., 2000)”.

The linearized version of the Arrhenius flow law (Eq. 14) represents one way to use a dimensionless parameter (the dimensionless activation energy coefficient E’ that controls the magnitude of the thermal slab weakening) in order to constrain this parameter and re-use it in future modelling. However, what may have confused the reviewer is that we omitted to write the equation as a function of the initially prescribed compositional viscosity for the slab layers. Thus, we have added a parameter ($\eta_{C\_SP}$) in our methods to describe the compositional viscosity of the lithospheric layers, and we have added $\eta_{C\_SP}$ in Eq. 14, so that the right side of the equation becomes multiplied by $\eta_{C\_SP}$. Therefore, the thermal dependence is applied on the initially prescribed compositional viscosity of the subducting plate layers only. To further clarify this, we now state after Eq. 14:
“Using equation 14 allows us to model warming-induced viscosity reduction on the compositionally defined subducting plate layers”.

This issue may have also partly raised because of the reviewer’s preference to use a composite rheological law applied to a single material, thus without using a compositional layering, as in Garel et al. [2014]. However, as described above, we used an alternate approach in which both a compositional layering and rheological laws are used. This combined approach has been used and published by other groups and it has proven efficient to model subduction dynamics [e.g. Arredondo and Billen, 2016; Holt et al., 2015a]. So we think that using this combined approach is justified.

**Comment**

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).

**Response**

No, our thermal slab weakening actually decreases viscosity in warm regions of the slab. Thus, it is consistent with the equation used in Ratcliff and Schubert [1996] and Zhong et al. [2000]. The only difference with these earlier works is that our models do not show rheological strengthening since the slab does not go into cold regions.

**Comment**

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.

**Response**

One particular point that is difficult to reproduce with geodynamic models is the fast subducting plate velocity observed for the Nazca-Farallon plate. Thus, we have decided to focus on this set of parameters (upper mantle rheology, subduction interface strength and slab thermal weakening) because we believe that they can strongly affect the subducting plate velocity, and they have proven to do so as shown in our results. We will include text in the revised manuscript explaining more clearly why we chose this set of parameters. Note that slab strength and rheology are actually treated indirectly using the slab thermal weakening. We also conducted models varying lower mantle properties but they will be presented in another paper since the work contains as much material as for this paper. Indeed, we found that all the models with a lower viscosity for the lower mantle produce less slab folding and give a reduced fitting score, which is why we kept only models with a lower mantle viscosity of 100 in this paper. We have added a brief discussion to section 4.5 where we discuss the fitting scores of our models, to explain that using a reduced lower mantle viscosity will reduce the fitting score for all models. Then, we would like to add that we found a best-fitting model without testing the rheology of the overriding plate. This means that the best-fitting model can be used as such in order to include more complexity (third dimension, aseismic ridges) without changing the overriding plate properties because they already provide a good fit. However, we acknowledge that it would also be interesting to investigate how overriding plate properties affect subduction dynamics. We note that overriding plate properties have been studied considerably in subduction modelling studies [e.g. Holt et al., 2015a; Holt et al., 2015b] and we thus decided to focus on other parameters that we thought could affect more the subducting plate velocity.

**Comment**

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?

**Response**

This comment is similar to the first main comment raised by the first reviewer. So we would like to emphasize here as well that with our models and our model-nature comparison, we only focus on the central segment of the South American subduction zone (lines 98-101 in the Methods of our original manuscript). Indeed, our 2D model approach dictates that it is only (approximately) applicable to the central segment of a wide (and symmetrical) subduction zone [e.g. Schellart et al., 2007; Schellart, 2020], of which the South American subduction zone is the best present-day example on the Earth. In any case, we realize now that we could have stated this more explicitly in our manuscript. Thus, to avoid any potential confusion in the future, we now state more explicitly that our model-nature comparison is only applicable to the central segment (Bolivian orocline region) of the South American subduction zone.

To accommodate the comment from the reviewer we will revise the first sentence of the Methods section such that our revised manuscript reads:

"The regional models were designed to conduct a parametric investigation on the effect of upper mantle rheology (linearly or non-linearly viscous), subduction interface yield stress σy and slab thermal weakening on the subduction dynamics of the central segment (Bolivian orocline region) of the South American subduction zone over a long timescale (~60-200 Myr) and large spatial dimensions (11600 km laterally and 2900 km vertically)."

We have also modified the sentence on lines 98-101 in the original manuscript by adding several references that support our claim:

"The 2-D approach is a reasonable approximation considering that we simulate the subduction process at the centre of a very wide subduction system where toroidal mantle flow is minimal [Schellart et al., 2007; Schellart, 2017] and slab geometry and plate kinematics are very similar as in 3-D subduction models at the centre of the subduction system [Schellart, 2020]."

We have also added another sentence here for extra emphasis:

"We compare our model results only to the central segment of the South American subduction zone, not its northern and southern branches, because our 2D models only represent the central segment of a wide subduction zone like South America."

We also now state in the abstract of our revised manuscript that our modelling and our model-nature comparison focus on the centre of the South American subduction zone:

"A key to help solve those issues is through studying the subduction zone dynamics with 2D buoyancy-driven numerical modelling that uses constrained independent variables in order to best approximate the dynamics of the real subduction system in its centre."

We furthermore add clarification to the objectives statement on lines 40-44:

"The objectives of this paper are twofold: (1) calibrate independent variables for use in future 3-D modelling by comparing model outcomes with a range of geophysical and kinematic data of the central segment of the subduction zone, and (2) parametrically investigate the effect of the changed independent variables to get generic quantitative insights into how they affect subduction dynamics at the centre of the subduction zone."

We acknowledge that we did not state in the figure caption of Fig. 3 that the velocities were calculated for the central segment of the subduction zone. We thus now add a statement to the caption of Fig. 3 that "the velocities were calculated for the central segment of the subduction zone".

**Comment**
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.

**Response**

In our view using a combined approach including compositional layers and rheological laws is not an issue. Other groups have published models using this approach [e.g. Arredondo and Billen, 2016; Holt et al., 2015a] and our models produce consistent results likewise.

**Comment**
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).

**Response**
This is described at line 199 of the original manuscript. This information will also be indicated along with the run times in a revised version of the manuscript.

**Comment**
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).

**Response**
The compositional buoyancy equals to zero in the subducting plate, so that we consider only the buoyancy as controlled by temperature. We have added text to clarify this point in a revised version of our manuscript.

**Comment**
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?

**Response**
We indeed ignore overriding plate shortening and its associated effect on trench motion when estimating the trench retreat rate in nature using motion of the South American plate. However, the reduction in Vt would actually be more pronounced if we would include overriding plate shortening, as shown in Faccenna et al. [2017].

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

**Response**
Yes, we did test lower values but as we explained above these models all gave a worse fit (lower fitting score) and so we plan to present those models in another paper focusing on lower mantle properties.

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

**Response**
According to our results that suggest that values between ~14–21 MPa produce the most consistent model outcomes and also according to earlier studies as we discussed at lines 651-658 of our original manuscript (e.g. 14-16 MPa [Seno, 2009], ~10 MPa [Wang et al., 1995; Gutscher and Peacock, 2003], 15-30 MPa [Zhong and Gurnis, 1994], < 35 MPa [Duarte et al. 2015]). We have added text to clarify this in a revised version of our manuscript.

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

**Response**
Ra was increased by increasing the subducting plate density. We have added text to describe this in a revised version of the manuscript.

**Comment**
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.

**Response**
The reference is Gibert et al. [2012] and it is in the reference list. We have changed "infinite slab-mantle viscosity ratio models" to "high slab-mantle viscosity ratio models" since they used a viscosity ratio that is comparable to high end-members used in analogue models (6000–15000), which is thus ~10–55 times higher than the ratio we used.

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

**Response**
Further effort will be made to make the discussion more concise in a revised version of the manuscript.

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