Response to comments from Anonymous Referee #1 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 work by Strak and Schellart uses numerical models to study the South America subduction zone. They compare their models results with data from South America using a ranking system to find which values of the studied parameters work best to fit the natural case. The manuscript is well written and the figures are clear, however I have troubles understanding the logic behind the study because I believe there is an important flaw at the base of it (see first comment below).
Response
We thank Anonymous Referee #1 for his review work on our manuscript and for providing a critical analysis. The main criticism raised by Anonymous Referee #1 (and also raised by Anonymous Referee #2) 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. As discussed in our detailed responses below, this is not a major issue since the two-dimensional approach is, as a matter of fact, appropriate to study the dynamics at the centre of wide subduction zones such as for South America using a vertical section at the centre of the subduction zone. So we are really trying to model only the centre of the South American subduction zone (Bolivian orocline region), and only compare our model results with this central segment in nature, and not the segments to the north and south. This point was mentioned in the methods section of the original manuscript (L98-101). In our revised manuscript we will state this more clearly by adding/revising text in the abstract, introduction, methods and captions of Fig. 2 and 3 (see detailed responses below).

Comment
Major points
In this study, the South America subduction zone is treated as if its characteristics (those used to rank the models) are the same everywhere along trench. This is a huge assumption that I believe really affects the conclusions.
Response
The reviewer comments that our study assumes that a number of South American subduction zone characteristics (e.g. subducting plate velocity, trench velocity, subduction partitioning, slab dip) are constant along the trench. This is not the case. We think this comment stems from an oversight by the reviewer 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 of 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."

Comment
Fig. 3 for example shows changes in trench and plate velocity with single lines, are these averages along the whole length of the trench, or are these taken along a specific section? If the latter is true, then which section is it? This is extremely important because, as the authors show in Fig. 2, there are large variations along trench. For instance, the slab is not flat everywhere, but there are only 2 regions where this is the case. Importantly, along section BB' the slab is not flat and this is where the positive anomaly in the lower mantle is higher (meaning that the slab pile is more clear, following the reasoning of the authors). In fact, the other two sections, where the slab is flat, do not show the same large anomaly in the lower mantle. How does this reconcile with the main conclusions and, more generally, with the philosophy of the study?
Response
What may have brought additional confusion in the original manuscript is that in Fig. 2 we also plotted profiles A-A' and C-C', which are not located in the centre, and that we did not state in the figure caption of Fig. 3 that the velocities are calculated for the central segment of the subduction zone. We now add a statement to the caption of Fig. 3 that "the velocities were calculated for the central segment of the subduction zone", and we keep profiles A-A' and C-C' in Fig. 2 in order to demonstrate that the South American subduction zone is quite symmetrical with respect to its centre regarding the upper mantle slab geometry. What should be added, and may also have brought confusion, is that the values of slab dip angle

close to the surface that we used as a basis for the model-nature comparison are between 0 and 25 degrees because the slab dip has evolved with time. Therefore, the present-day values range between 8–25 degrees (profile B-B' on Fig. 2) but we extended the range down to 0 degrees since flat slab is known to have occurred for the central segment as well [e.g. Ramos and Folguera, 2009]. For this last part we will include text in the revised manuscript explaining why the range is from 0 to 25 degrees (in the text and in the caption of Fig. 8).

Comment

In other words, this study tries to find the best 2D model that fits as many criteria as possible with the natural case, but in South America the criteria themselves are not all present in one single 2D section of the subduction zone. So, what is the point of finding a model that fits everything, when even in nature this is not the case? I used the slab dip and the slab pile in the lower mantle as examples, but also the other criteria (like the subducting plate velocity and the trench velocity) are not the same all along the trench.

Response

As discussed in our responses above, we do not intend to model or fit all parameter values along the subduction zone, but rather only the values measured and calculated for the central segment. We hope that this aspect is now clarified.

Comment

Another important point is that the conclusions of this study are based on the comparison between models and data according to 9 criteria. However, at the moment, these criteria are mostly qualitative. How is the progressive reduction of trench velocity computed? What is the 'acceptable' range/error of flat slab portion length to have a rank of 1 or 2? The same goes for all criteria: what the authors define 'somewhat comparable' and 'very comparable' is subjective. I suggest to add a table in which the values of these criteria are clearly stated both for the models and for the data the models are compared with. And then again, some models might better fit one section of the subduction zone, but others that do poorly in that section might be better at fitting another section. I am not sure what we can learn from this though.

Response

We have now defined a set of quantitative rules in order to determine the fitting score for all models. For example, based on the subducting plate velocity, a model gets 2 points if the average value after slab penetration into the lower mantle is within the natural range and the maximum and minimum values are within 10 percent of the natural maximum and minimum. The model gets 1 point if the average value after slab penetration into the lower mantle is within the natural range and the maximum and minimum values are not within 10 percent of the natural maximum and minimum. Otherwise the model does not get a point. We performed a similar analysis on all parameters and the main results remain similar as in the original manuscript. We clearly explain all the quantitative rules that we have used for our ranking in the revised manuscript.

Comment

About the flat slab. The authors state that there is no need to add external forces or have a buoyant body to have flat slab and that slab flatting can happen dynamically as a consequence of a progressive decrease of the slab dip (lines 438-443). Then one might wonder why are there only two portions of flat slab along the South America subduction zone. Why is the slab not flat along section BB' (Fig. 2)? Moreover, these models do not take into the presence of buoyant bodies in the subducting plate (which it cannot be denied it is the case in the Nazca plate with the ridges entering the subduction zone), so the question is how would the results and conclusions change if a more realistic subducting plate would be modelled?

Response

As we noted above, a flat slab also occurred at the central segment ("Altiplano flat slab") in the geological past (40-32 Ma and 27-18 Ma) [Ramos and Folguera, 2009]. Some of our most recent (unpublished) models and models presented in Schellart [2020] indicate that the slab dip angle close to the surface strongly evolves with time and may display an episodic behaviour. The aim of this paper is not to model natural complexity by adding all the properties of the subducting plate (aseismic ridges). We rather propose to first scale parameters using a relatively simple geometric and rheological setup in order to bring and study complexity stepwise. The next step indeed will be to investigate the effect of aseismic ridges with a 3D setup.

Comment

40-44: I find the objectives of the paper very vague, they could fit with any paper that presents a parametric study. I suggest to be more specific about which features of the South America subduction zone the authors are trying to explain/understand with this study. For example, in the first sentence of the introduction it is stated that this subduction zone is enigmatic because of the orogeny formed with an oceanic subduction, but this study is not really addressing this issue. Instead, paragraph 2.5 is describing the features that are then compared with the results. To me, it makes more sense to move this paragraph in the introduction, but the authors should also add an explanation on what it is about these specific features that is not yet understood and what are the main research questions related to these features they are trying to answer.

Response

As we explain above, we are trying to explain and understand the central segment of the South American subduction zone. Our view may moreover differ from the reviewer's opinion about parametric studies and their usefulness. We think that a parametric study can represent a goal on its own. Our study, in particular, represents an attempt to 1) scale independent variables using a model-nature comparison in which the model evolves self-consistently and many natural dependent variables are compared with the model outcomes. This is an important goal since it allows geodynamic modellers to re-use the scaled parameters in future modelling studies. 2) In our study, we moreover investigate the effect of the tested parameters on subduction dynamics, kinematics, slab geometry and overriding plate deformation. This is already interesting on its own since it could provide a better understanding of the rheological effects (upper mantle rheology, subduction interface strength, slab thermal weakening) on subduction dynamics. One particular point that is difficult to reproduce with geodynamic models is the fast subducting plate velocity observed for the Nazca-Farallon plate, which therefore justifies our choice of investigated parameters since they all can affect this velocity. Our conclusions moreover show that it is important to include thermal weakening of the slab to reach a fast subducting plate motion, which therefore has implications for future modelling studies. We agree with the reviewer that our study is not really addressing the topic of orogenesis at a subduction zone, so in our revised manuscript the first sentence of the introduction has been changed.

Comment

243: from Fig. 3a it seems more that the gentle decrease in vT (between 2.5 and 2 cm/yr) is only until about 12 Ma, then there is a clear step to 1.5 cm/yr and the trench velocity remains more or less constant. Given that this is one of the thing that decides the final ranking of the models, how does this affect the results? Again, having a table with quantitative values for each criterion would help.

Response

The trench retreat velocity was estimated using motion of the South American plate. Thus, the landward-directed deformation of the trench due to shortening in the Andes is not included in our calculation. However, this would result in further decrease in trench retreat rate, notably in the last ~40 Myrs (e.g. Fig. 3 in Faccenna, 2017). We have clarified this point in the text of section 2.5, as follows:

"Because our calculation does not consider landward-directed deformation of the trench due to shortening in the Andes, the progressive decrease in vT could be more significant (Faccenna et al., 2017)."

Comment

The amplitude of oscillation in vSP is also something that the authors look at in the models, however this is not described in paragraph 2.5 at all, but it starts to be mentioned only in the results and discussion. Describe the natural range.

Response

We do describe this in paragraph 2.5, namely on L241: "it fluctuates between ~6 and ~10 cm/yr in the past ~45 Myr". To emphasize this better in the revised manuscript, the text "it fluctuates" is replaced with "the amplitude of oscillation in Vsp fluctuates".

Comment

The viscosity jump between upper and lower mantle has a major control on the slab bending, piling and flattening. And slab piling and flattening is the main focus of this study. However the viscosity jump is not a parameter that is investigated. 100 is a commonly used value, but it is also an end-member. Often, other numerical studies use 30 as a viscosity jump. How do the authors think a lower viscosity jump would affect their results? I would like to see a model with a lower viscosity jump and see the effect on slab piling and flattening.

Response
We have also conducted models with different lower mantle viscosities and densities, but have decided to present the results of these models in another paper focusing on lower mantle properties. 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.

Comment
Other minor points 128: Does the assumption of a neutrally buoyant OP affect the overriding plate deformation?
Response
We do not know but we expect that the effect is moderate based on a comparison with published models that include a positively buoyant overriding plate [Schellart, 2020]. In any case, this would not change the conclusions of the paper since all models have been run with the same overriding plate.

Comment
The overriding plate is 60 km thick for about 1000 km from trench. How does this compare to South America?
Response
It actually fits quite well the present-day thickness estimated geophysically [e.g. Heit et al., 2007]. It moreover considers the forearc and backarc thickness determined by Curie and Hyndman [2006], which suggests that these forearc and backarc are/were prevalent features in ocean-continent subduction zones of the Pacific domain (L162-164 of original manuscript).

Comment
196-198: is it only the viscosity of the slab that is affected by the thermal weakening? Or is it also the viscosity of the mantle? One of the reasons for this question is also because in Fig. 12 there are 'blue', thus very weak, regions around the slab in the lower mantle. It seems to be a consequence of numerical instability, is it?
Response
Yes, only the viscosity of the slab is affected by thermal weakening (L195-198 of original manuscript). In Fig. 12, the blue regions of the lower mantle are actually zones of the slab that are stretched because of the reduced viscosity. Thus, they are not a consequence of numerical instability.

Comment
291-292: the subducting plate does not seem to be entirely consumed in Fig. 6f, why does the model stop?
Response
The model does not stop but continues for several Myrs. We did not show these late subduction stages on the figures because we thought that there are already enough figure panels in this paper and the late subduction stages display characteristics that are similar to the last figure panel in the manuscript.

Comment
Fig. 6 only show two of the models with different A, I suggest to show the dynamics of the other two models (or at least a final snapshot) in the supplementary material.
Response
Yes, Fig. 6 shows the models with the two extremes of the A value. However, Fig. 5 already shows the reference model with an intermediate A value. We can add the model with the other A value in the supplementary material if needed.

Comment
374: "the higher E', the quicker the flat slab is attained". Or is it simply because subduction is faster, but the amount of convergence is the same?
Response
Yes, in those models, the higher slab thermal weakening, the faster subduction and thus the quicker the flat slab is attained.

Comment
729: then why is the natural range in figures (blue area) only going between 6 and 10 cm/yr?
Response
The natural range for the subducting plate velocity will be updated to 3–10 cm/yr. This will not affect the results and conclusions of our paper.

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