Dear Editor, dear Reviewers,

Please find enclosed our revised manuscript (Preprint egusphere-2022-768) on ‘The effect of temperature-dependent material properties on simple thermal models of subduction zones’ by Van Zelst et al.. We thank the two reviewers and the editor for their feedback. We addressed the - mostly textual - comments by the reviewers in our revised version (see below). Responses to the suggestions by the reviewers are indicated in green. Line numbers refer to the line numbers of the provided tracked changes file.

Thank you for considering this revised manuscript for publication.

Yours sincerely,
Iris van Zelst (corresponding author),
Cedric Thieulot, Timothy J. Craig

Letter from the Editor

Dear Authors,

Thank you for submitting the revised paper version, which has been assessed by two reviewers. As you will see, both reviewers are positive but raised some comments, which need to be addressed. Please answer the comments, revise the paper respectively and make sure that the final version you upload includes the missing data mentioned in the "Data availability" section: "The models were run with the open source code xFieldstone (which will be made available on GitHub upon paper acceptance; the exact version of the code will be stored in a Zenodo repository). Upon paper acceptance, we will also make a Zenodo repository with the data used to reproduce the van Keken et al. (2008) benchmark and the Matlab scripts used for the postprocessing of the results and generation of the figures"

With my best wishes.

Taras Gerya

We have addressed the comments from Reviewer 3 and made the code and the data and scripts available on Zenodo with appropriate updated references in the text.

Referee #2

Dear authors,

Thank you for addressing the comments raised. The revised manuscript is much improved and is now a factual account of the results presented.

Please make sure that the final accepted version you upload includes the missing data mentioned in the "Data availability" section:

"The models were run with the open source code xFieldstone (which will be made available on GitHub upon paper acceptance; the exact version of the code will be stored in a Zenodo repository). Upon paper acceptance, we will also make a Zenodo repository with the data used to reproduce the van Keken et al. (2008) benchmark and the Matlab scripts used for the postprocessing of the results and generation of the figures"

Thanks! We have now made the code, the data, and the scripts available on Zenodo and added the references in the text.

Referee #3

The authors implement temperature-dependent thermal parameters in numerical models to test their effects on the thermal structure of subduction zones. I appreciate their efforts on pushing forward the community to use more sophisticated/realistic processes to simulate plate tectonics, and it's good to see the model changes affected by the temperature-dependent thermal parameters. However, I feel the strategy of this MS is a bit strange. On one hand, the authors claim that we should use more realistic/complex temperature-dependent thermal parameters in subduction simulation. However, on the other hand, the authors test the effect of the temperature-dependent thermal parameters with an extremely simplified model (not only the model setup, but also the governing equations). I do not believe that important physics could only be revealed by simplified models. I am fine with the current results presented in the MS, but would like to suggest the authors to use more sophisticated models for their future numerical studies.

My questions and comments are listed below.

- Temperature could affect brittle-ductile deformation, which in turn affects seismicity. Unfortunately, the authors do not provide any discussion. Maybe add some brief discussion in section 4.1.

  We have expanded the discussion on megathrust seismicity and the brittle-ductile ductile transition by adding three paragraphs to Section 4.1.3 (lines 533 - 552).

- Lines 11-12: strange conclusion: "......temperature-dependent thermal parameters...... has a secondary effect on ......compared to...... ". I fully agree with the previous reviewer who also questioned this conclusion. This kind of expression is indeed very confusing. In my opinion, the aim of this study is to clearly show readers the effect of the temperature-dependent thermal parameters on subduction evolution. I guess the authors do not need to emphasize the competition in terms of importance between the thermal parameters and other model parameters (e.g., plate rheology or age).

  We initially wrote this to comply with previous reviewers' comments who wanted to us to be clear that temperature-dependent parameters are not the main first-order parameters that could affect thermal structure. However, we agree that the way it is written here is stunted and unnecessary. The rest of the manuscript nuances the point of the other reviewers sufficiently. We have therefore rephrased this part of the abstract (lines 10 - 13).

- Line 105: the temperature equation. What are the "external heat sources"? Shear heating, for instance? If so, I really do not understand why, since you are dealing with temperature evolution. It's really unnecessary to exclude the heat sources (especially when we are able to implement).

  We now specify what kind of external heat sources we exclude in lines 108 - 109 and why we exclude them: to simplify the model. Mainly, the reasoning is that we stay as close to the model setup of Van Keken et al. (2008) as possible, who also excluded external heat sources, such that we can easily compare our model results and the effect of temperature-dependent thermal parameters in a well-defined, simple model setup. We briefly discuss the potential effect of external heat sources in the model in the discussion.

- Equation 2: Please explain explicitly with several lines why do you use zero gravitational acceleration, even for the vertical direction (and do not just refer to the van Keken paper without giving any necessary explanations). Besides, does "zero gravitational acceleration" mean there is no pressure? Is subduction purely driven by the velocity boundary condition?

  Indeed, the subduction is purely driven by the velocity boundary condition (i.e., kinematically-driven subduction). We added a few lines to explain why we use zero gravitational acceleration (lines 105 - 107):

  "In this formulation of the Stokes equations, we implicitly assume zero gravitational acceleration such that we have purely kinematically-driven subduction. This allows us to have the same (fixed) subduction geometry in all models."

- Line 141: Do not like this expression: "as we do not aim to ..."'

Since none of the other reviewers commented on this and the scientific content of the manuscript isn't affected by this phrasing, we have kept the expression as is.

- Line 147 Could different overriding plate structures (thickness and thermal structure) affect the slab thermal structure?

  We did not test this, but - to an extent - the overriding plate would probably indeed affect the slab thermal structure, especially when the thickness of the overriding plate changes and hence the mantle wedge corner is shifted to another location. We comment on this in the discussion and we added a sentence on the potential effect of the thickness of the overriding plate (line 632).

- Fig. 1b: Do the authors use constant temperature for the asthenospheric mantle underneath the overriding plate? In my opinion, this is over-simplified, since it is not complex to implement the adiabatic temperature gradient (and it is more realistic).

  We do not define the temperature in the mantle beneath the overriding plate. Instead, we prescribe that the temperature of incoming material is at 1300°C. Besides that, the temperature of the mantle wedge is then solved for according to the prescribed temperature boundary conditions (see Figure 1a).

- Line 180: Plate model also has the shortcoming to describe the thermal structure for young plate ages.

  This is correct. However, we do not consider the plate model in our paper and therefore do not mention it. Instead, as we are basing ourselves on Van Keken et al. (2008), we merely comment on the shortcomings of their use of the half-space cooling model. Using the half-space cooling model is fine for the benchmark they suggested, but is too simplistic when considering more realistic setups.

- Line 210, Figures S8-S17: Too many 2D figures. Please show the difference in a concise manner, e.g., plot difference with temperature profiles?

  The difference between the temperature profiles is already shown in Figure S18. We now also refer to this figure in the text for the reader's convenience.

- Figure 2 "The lighter colors indicate the crustal approximation for the thermal conductivity (i.e., multiplied by 0.5) and the density (i.e., multiplied by 0.79)." and Line 295. Why do you use the approximation rather than the realistic thermal parameters for the crustal domain?

  We added the set of models with a rough crustal approximation to determine the first-order effect a crust would have on the thermal structure of a subduction zone in our models. This was done after previous reviewers asked about the potential effect of a crust in our models. Since implementing the full, complete set of realistic thermal parameters for the crustal domain is beyond the scope of this paper, we opted for this simple approximation instead to give the reader (and ourselves) a sense of how (and if) different crustal parameters might change the results.

We now added a line where we introduce the simplified approximation of a crustal layer to direct the reader to the discussion where we discuss the implications of this simplification and alternatives and considerations (line 289).

- Line 245 "......first-order effect......" is confusing.

  We removed 'first-order'.

- Fig. 6: It's good to plot the difference between the two models. However, except the thermal parameters, the T-profile left boundary is different in these two models, i.e., the ref. model using the half-space cooling model while the preferred model using the plate model. This is weird. One should keep all other parameters (except the three thermal parameters) identical. Alternatively, one could compare the case2c_bc model with the case2c_all model.

  We showed the difference between these two models, because we wanted to compare the models with the biggest difference, i.e., model case2c_PvK and case2c_all (which are also the models shown in the main paper in Figure 4 and 5). We agree this is perhaps not the most straightforward comparison, but as the differences between case2c_PvK and case2c_bc are very minor (see Figure 7, Table 2, and Section 3.1) it does not really matter whether we use case2c_PvK or case2c_bc for this comparison figure. Since the other reviewers did not comment on this, we keep this figure as is.

- Lines 387-389, 392-393, Figs. 6a-b: The main reason of the colder slab and the overring plate in the preferred model (case2c_all) is larger thermal diffusivity (e.g., Fig. 3)? The lower part of the slab in the preferred model (case2c_all) is slightly warmer, why? One should explain and section 3.5 is the right place.

  We added the following explanation to this section (lines 406 - 409):

  "This difference in slab temperature between the two models is partly due to the difference in boundary condition, as the plate cooling model in case2c_all is cooler than the half-space cooling model of case2c_PvK at shallow depths due to increased conductivity at low temperatures, and warmer at larger depth due to the imposition of a lower thermal boundary condition in the plate model. This also depends on the age of the subducting slab (see Section 3.6)."

- Figs. 6c-d: It is quite difficult to understand the initial setup of the case2c_all_cp model. Since this is quite an important model, why not describe it in Table 1? Furthermore, this model behaves much different from the case2c_all model, and one should provide description in section 3.5.

Model case2c_all_cp is described in Table 1 in the note below the table and explained in the text in Section 2.6 (lines 301 - 303).

In section 3.5 we only discuss the model without the continental plate parameterisation, which is why we do not comment on model case2c_all_cp. The confusion likely resulted from the fact that we refer to the entirety of Figure 6, instead of just the first two panels a and b. We have now changed this and in Section 3.8 (where we discuss the results of the models with the parameterised overriding plate) we now refer to Figure 6 c and d.

- Supplement: Do readers really need to read through the 29 figures? Are these figures really essential for readers to fully understand the main text?

  We include all the figures in the supplementary material such that readers have access to all the results. Previous reviewers did not mind the amount of figures in the supplementary material and in fact many of the figures in the supplementary material were added because of suggestions by the reviewers, so we keep the supplementary material as is.