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 detailed feedback. We incorporated most of their suggestions in the new manuscript, which we believe has resulted in a clearer and more nuanced paper.

The two reviewers had different views on the direction this manuscript should take to ready it for publication. Reviewer 1 requested that we use a more complex, realistic subduction zone geometry in our model setup. In contrast, reviewer 2 suggested that we could cast this paper and the current results as a companion paper to Van Keken et al. (2008) and/or present our work as a negative test. We have decided to largely follow the suggestions by reviewer 2 and emphasize our closeness to the original Van Keken et al. (2008) paper. Although we cannot fully cast our work as a negative test, because our models show that temperature-dependent thermal parameters do have an effect on the resulting thermal structure of the subducting slab, we now better explain for which purposes these second-order effects are likely relevant. We also explicitly mention parameters with first-order effects on the thermal structure of a subduction zone, such as rheology or plate age, to avoid any confusion as to the relative significance of our findings.

By following the suggestion from reviewer 2, we cannot simultaneously follow the suggestion of reviewer 1 of a more complex, realistic subduction zone geometry. This is unfortunately beyond the scope of this paper, which we explain in more detail in the rebuttal letter below. However, we have addressed their other comments and thereby believe we have improved the paper sufficiently for consideration for publication.

We also note that reviewer 2 commented that the paper was quite lengthy and should perhaps be shortened and more limited in scope. We refrained from doing this, as a lot of the added material that increased the length of the initial manuscript stems from previous rounds of revisions at JGR: Solid Earth. Since we also want to honour the valuable input from those two reviewers, we have not shortened or limited the scope of our paper. We believe that the extensive discussion is useful to provide the context in which our results are meaningful, both in terms of discipline (i.e., a seismological application) and our assumptions and model limitations.

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

Reviewer 2

The objective of this paper is to understand the variability of the thermal structure along the subduction interface when using a specific heat capacity, conductivity and density are temperature dependent. The temperature dependence in these parameters has not been considered in previous (steady-state) subduction zone simulations (to my knowledge). The authors conduct their analysis using a model inspired by the reference model defined in a community benchmark paper (van Keken et al. (2008)).

I do not consider this paper appropriate to publish in its current form for the reason that the conclusions and numerous statements made in the paper are not supported by the results shown. Worse over, the authors actually appear to contradict their own findings throughout the paper on several occasions.

We have reformulated sentences to ensure that there is no ambiguity and perceived contradictions in our writings. By clarifying our writings, we now also ensure that everything is clearly supported by the results shown. See below for specific changes made in response to comments by the reviewer.

The closing sentence is one example: "For optimal comparison to data and to avoid misinterpretations, we therefore suggest that temperature-dependent thermal parameters are an important modelling ingredient and that they should be taken into account when using thermal(-mechanical) models of subduction zones."

- Your own results actually show the assumption of steady-state (+ age) has a much larger influence on the temperature than including temperature dependence in the thermal coefficients (\rho, C_p, k).

This is correct. However, we hope to convey that temperature-dependent thermal parameters are also an important modelling ingredient to take into account when one wants to accurately model the thermal structure of a subduction zone. Especially for applications where modellers compare with observed seismicity, changes in the temperature field of tens of degrees or tens of kilometers are significant. To clarify that our findings are not first-order controls on the thermal structure of subduction zones, we have added a sentence in the conclusions (line 700 - 702).

- You neglect shear heating. Including that shear heating alone has been reported to increase the temperature by > 200 deg C, see for example Peacock, Geol. Soc. Am. Bull., (1993); England and Molnar, Tectonics, (1993); Burg and Gerya, Geology, (2005).Your results appear to indicate that the temperature dependence results in +/- 20 deg C variations in the thermal structure along the subduction interface. Given that the two points above, the temperature dependence you've introduced seems to be rather a secondary effect and thus the claim that T-dependence should be taken into account for reasons of accuracy, realism and to avoid misintrpretations in data / obsertvations is unjustified and unsupported. As written in its current form I found this contribution unclear, ambiguous and often disingenuous.

As mentioned above, we would like to convey that temperature-dependent thermal parameters are also an important ingredient to take into account when modelling the thermal structure of subduction zones. We do not claim that they are the most important ingredient - indeed, as the reviewer mentions, there are many other more important first-order factors, including the rheology, plate age, and the addition of important thermal processes such as shear heating. However, we show that on a smaller scale, temperature-dependent thermal parameters are indeed something to take into account, especially when one wants to use the models for comparisons with seismicity: then these second-order effects to thermal structure become important.
To make it more clear that we are talking about a second-order effect on thermal structure throughout the manuscript, we have reformulated the text throughout the manuscript (also see comments below).
We already mention the effect of shear heating on the thermal structure of subduction zones and the fact that we neglect it in our model in line 624. We have added the additional references that the reviewer mentions.
With our extensive discussion on model limitations and other model ingredients that could affect the thermal structure of a subduction zone, we believe we convey to the reader the nuance and caveats of our results concerning the important, though secondary, effect of temperature-dependent thermal parameters in thermal models of subduction zones.

From your results, the inclusion of the T-dependence does not appear to greatly influce the temperature along the interface. Hence, instead of exaggerating or extrapolating the results you have, it would be better to just report / document what you find. That is, I suggest you refactor the submission such that it is more like a companion paper to van Keken (2008) which only quantifies the thermal variability — within the scope of the idealised subduction model you consider — due to introducing temperature dependence. Focus on reporting the facts which are supported by your results, and place them within the context of all other modelling assumptions which are made in your idealised subduction model. In my opinion, confining the scope of the study in this way would make it a better contribution.

With the changes to the text we made based on the recommendations of the reviewer, we now believe that we are unambiguously reporting the findings of our study. We indeed view our paper as a companion paper to Van Keken et al (2008) to an extent, but we believe that the extensive discussion with potential implications and model limitations is beneficial for readers who would like a more in-depth view of the applicability of our results. In addition, many parts of the paper were added on the request of previous reviewers at JGR: Solid Earth and we are reluctant to remove those additions, since they also provided valuable comments that improved the paper. Hence, we have mostly focused on reformulating parts of the discussion and our conclusions to align with the current reviewer's wish of confining the scope of the study.

Comments

L77: “well-constrained” - In what sense is the community benchmark model well-constrained? I agree its simplified and well-defined. But it’s not well-constrained.

We changed it to well-defined (also at other instances in the text).

Eq (2) Why bother to introduce \vec g and then promptly state its value will always be zero?

We removed the second term in this equation and now state that we solve the conservation of mass and momentum with the assumption of zero gravitational acceleration, i.e., without introducing $\boldsymbol{g}$ in the text.

L111 The statement “purely viscous rheology and hence neglect any elastic and plastic contributions to the deformation.“ seems redundant. You can just say you consider a “purely viscous rheology”.

We changed this.

L112 You relate the deviatoric stress to the deviatoric strain-rate. You aren’t relating the “stress to deformation” at all.

We reformulated this.

L121 “assume zero activation volume“. The importance of making this assumption, i.e. what effect / influence this has on the thermal structure is not at all cosidered or discussed. That seems like an oversight. Just beacuse the assuumption was made in the benchmark paper doesn't mean it's an appropriate choice for subduction modelling in general.

We reformulated our initial statement in the methods where we said ‘assume zero activation volume’ to clarify why we make this assumption. In addition, we have added a few sentences to the discussion to clarify what the effect of a non-zero activation volume would be on our model results (line 598 - 601).

L127 You defined something as “square root of the dev strain-rate tensor” then re-defined it is the “effective deviatoric strain rate“. Just provide one definition and remove “i.e., effective deviatoric strain rate”.

We removed the explanation between brackets on the effective deviatoric strain rate.

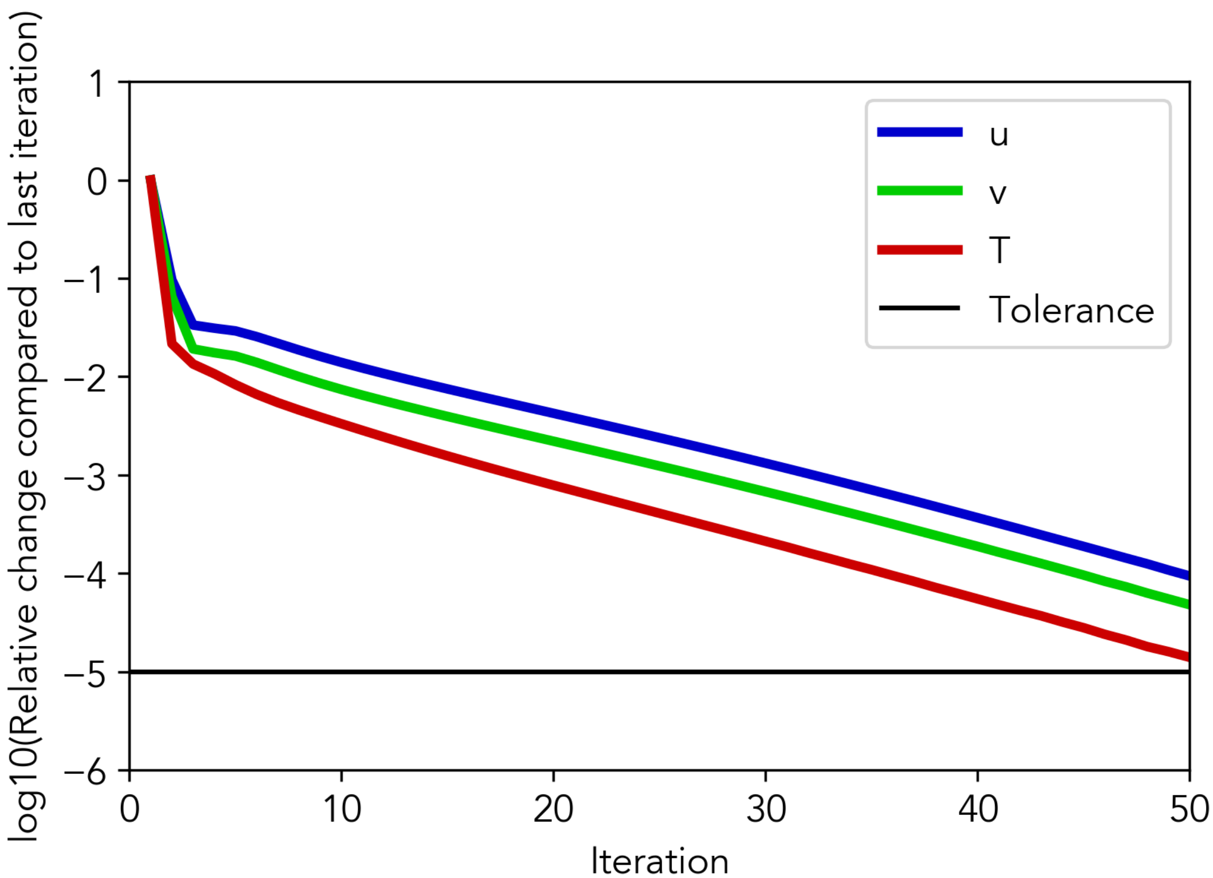
L137 In what sense is the benchmark well-constrained? Such a term would be interpreted to mean that the definition of the model is somehow in agreement with a natural subduction zone (which it is not).

We changed it to well-defined (see above).

L164 “temperature compared to the previous iteration change less than a given tolerance “ this stopping condition will will return a false positive if no progress is made in solving the non-linear problem.

This is true, but we observe monotonic convergence for the velocity (vx and vy) and temperature in the models. Therefore, the scenario where there is a false positive of convergence due to no progress in the solving of the system, is not one that occurs with our model setup.

As an example of what we observe concerning the convergence of the model, here is the convergence plot of the model case2c_all:



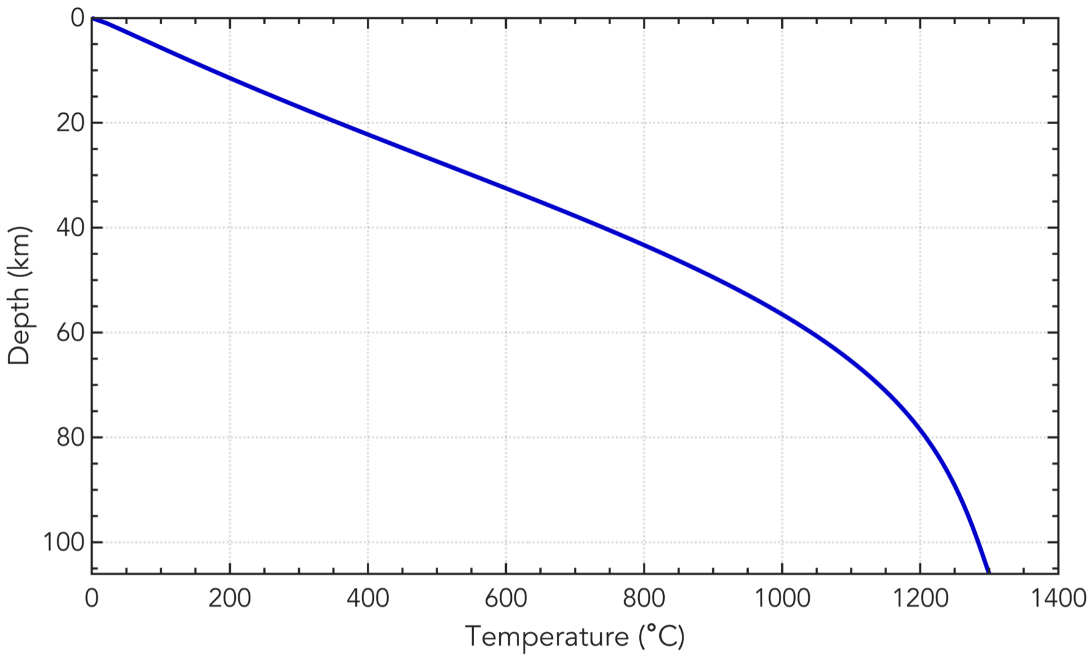
The chosen tolerance (1e-5) in essence means that we solve the system until the average value of the temperature field change between two consecutive iterations does not change more than 0.01K, which is sufficient for this type of model setup. As mentioned in the paper, additional tests show that employing a lower tolerance of 1e-3 (which is always reached before 50 iterations) changes the model diagnostics from the results section by less than 1°C and has no effect on the reported isotherm depths.

L171 This statement “results in robust convergence“ is completely false and should be removed. Your stopping condition (as mentioned above) doesn’t monitor the convergence of the solution to the nonlinear problem F(v, T) = 0. Hence you cannot infer convergence is “robust”. Using your stopping condition, when the non-linear solver residuals stagnate, (meaning no progress is made) you will incorrectly interpret this as converged.

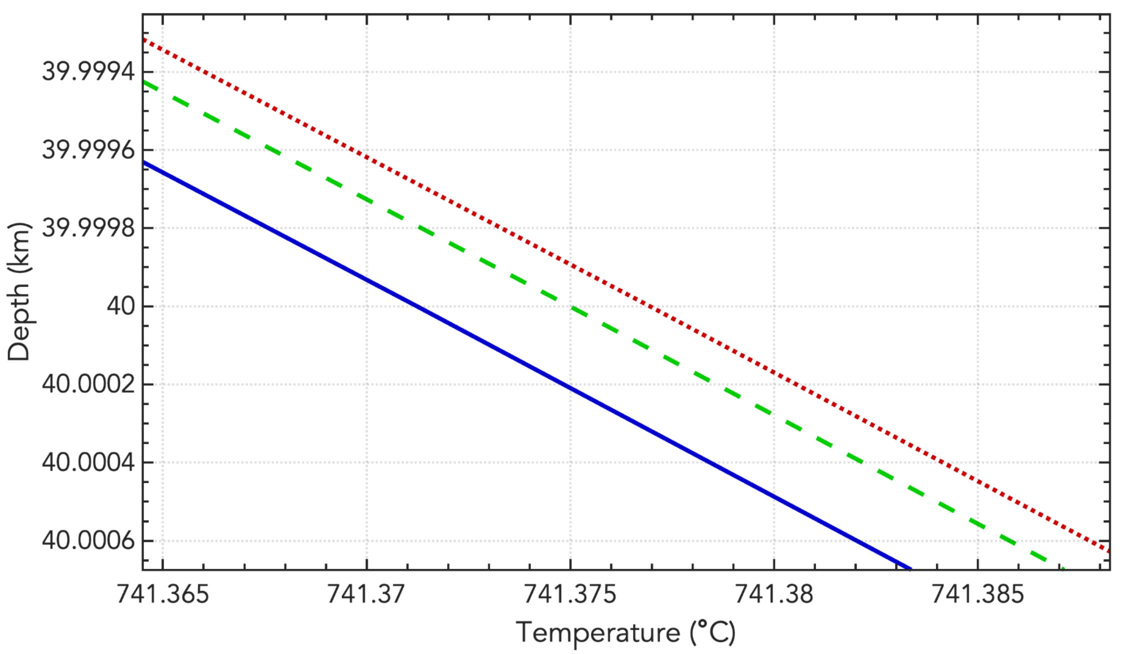
We changed it to “which prevents numerical oscillations in the solution towards convergence”.

Eqns 13-16 define the solution procedure for a linear problem (i.e. when \rho, C_p and k are not functions of T). You stated earlier you incorporate the nonlinear parameters into this 1D model and use them as boundary conditions. Please correct the description of the method used to obtain the 1D temperature profile for the non-linear case.

The current description accurately describes what we do in the code. For this procedure we followed Richards et al. (2018). As the reviewer points out, it is true that we do not perform any additional non-linear iterations beyond the described predictor-corrector step, similar to Richards et al. (2018). We also note that McKenzie et al. (2005) used time steps on the order of ~7000 years and only performed 2-4 non-linear iterations.
Following the reviewer’s question we investigated the matter by looking in detail at the influence of the CFL condition to see if we make any errors by neglecting additional non-linear iterations. We find that due to our low time step (delta t = 1000 years), we are well below the CFL condition. We find that our choice of low time step catches the changes that non-linear iterations would have provided. To illustrate the small changes induced by the use of different time step values, and hence by proxy the addition of non-linear iterations, we show the temperature profile (with depth and zoomed in) of the case2c_all model at 50 Myrs calculated with different time steps: dT = 500 years (red + dotted), dT = 1000 years (green + dashed), and dT = 2000 years (blue + solid). The changes in temperature for a given depth are on the order of 0.005 C and therefore negligible.
Hence, we keep the description of our method as is, as it accurately reflects what we did. Due to the small time step, we did not need to perform any additional non-linear iterations, which is why they are not mentioned in the text.



0
20
40
60
80
100
Depth (km)
0
200
400
600
800
1000
1200
1400
Temperature (°C)



39.9994
39.9996
39.9998
40
40.0002
40.0004
40.0006
Depth (km)
741.365
741.37
741.375
741.38
741.385
Temperature (°C)

Eq (20) Don't use \cdot to denote multiplication. You have used the same notation to denote a dot product (e.g. Eq (1)).cc

Since we use \cdot in many of the equations to denote multiplication and make the equations more readable, we keep \cdot to denote multiplication. However, to distinguish it from the dot product in equations 1-3, we now use a thicker dot (\bullet) for those equations.

L275 Why is this statement even made? Your point was made clearly when you wrote down Eq (3) without using \kappa.

We removed this statement.

L315: The L_2 norm (big L2) defines an integral. l_2 (little L2) is used to define a sum. Please correct this.

Thanks for pointing this out. We corrected this.

Figure caption 4. You state the velocity contours go up to 5 m/s - I think you mean 5 cm/yr. Actually throughout this caption you speak about velocities measured in m/s which is incorrect. Same comment for figure captions 5 and S1.

You are indeed correct. We fixed it.

Figure 7: The plot style is inappropriate. When you connect dots together with a line you imply there is a relationship between the two data points. However, the x-axis in this plot are different models - hence there is no relationship between the data (e.g. between all the yellow squares for example). Remove the lines connecting the data points.

This is an excellent point. We have removed the connecting lines and slightly changed the colours of the figure to make the entire figure more readable. We have updated figures 7, 9, and S29 in the supplementary material, as they all shared this problem.

L394 Here you say "extreme effect in the overriding plate indirectly affects the thermal structure of the slab." whilst at L 452 you say "… the nature of the overriding plate, and indeed the inclusion of an overriding plate at all, does not significantly affect the temperature field." You supported this statement with figure 7.

The difference between these two statements is that in the first statement, we discuss that the temperature in the overriding plate is affected the most by the inclusion of temperature-dependent thermal parameters, which indirectly affects the thermal structure in the subducting slab. In the second statement, we talk about the effect of changing the nature of the overriding plate (continental plate, oceanic plate, etc) on the thermal structure of the subducting slab. Hence, there is no contradiction here, as we were talking about two different things. This was worded confusingly, so we have rewritten both statements to make this distinction more clear (line 399 - 401 and 459 - 461).

L460-465 You state “Our models with different plate ages show that the implications generalise to ALL subduction zones regardless of plate age but still lack realism…” Your results do not support your implications generalise to ALL subduction zones - at best it generalises to those which have a constant dip of 45 degrees and are at steady state.

We reformulated this to “Our models with different plate ages show that our conclusions are valid regardless of the slab age”

L503-504 You write “Neglecting temperature-dependent thermal parameters could result in significant errors of up to hundreds of kilometres in the estimated“ but none of the results presented in the paper support this statement. Take figure 7 and compare the two extreme models case2c_PvK_cp and case2c_all which you regard as the least inconsistent and the most self-consistent / accurate / complex. The 600 degC isotherm is shift by ~25 deg C (squares) and ~ 50 deg C (circles). It’s not changing by 100’s. of deg C. The differences are even smaller for the 350 and 450 deg C isotherms.

We changed this sentence to “tens of kilometers” and we added some extra sentences to provide more nuance to this statement by discussing the difference between considering all 3 thermal parameters to be temperature-dependent and including only 1 thermal parameter that is temperature-dependent.

L588-589 Regarding “However, based on our results, we predict that changes in the model with regards to the overriding plate will not significantly affect the temperature field of the slab.” - yes I agree, assuming the over-riding plate parameterization did not include any radiogenic heat production.

This is a good point. We added a sentence to explicitly mention our lack of radiogenic heat production in the overriding plate, so our prediction is now written with that caveat beforehand.

L645-650 Here you state you have 87.5 km variation (it looks more like 50), however for the cooler isotherms (where the variation is actually less!) you previously reported 100’s of km of variation (L503-504).

We changed the previous statement; the value reported in the conclusions of 87.5 km is correct.

References

Grose, C. J., & Afonso, J. C. (2013). Comprehensive plate models for the thermal evolution of oceanic lithosphere. Geochemistry, Geophysics, Geosystems, 14(9), 3751-3778.

Kelemen, P. B., Kikawa, E., Miller, D. J., Abe, N., Bach, W., Carlson, R. L., ... & Takazawa, E. (2004). Proceedings of the Ocean Drilling Program, Initial Reports, Volume 209, Drilling mantle peridotite along the Mid-Atlantic Ridge from 14 degrees to 16 degrees N, Sites 1269-1275.

Richards, F. D., Hoggard, M. J., Cowton, L. R., & White, N. J. (2018). Reassessing the thermal structure of oceanic lithosphere with revised global inventories of basement depths and heat flow measurements. Journal of Geophysical Research: Solid Earth, 123(10), 9136-9161.

van Keken, P. E., Currie, C., King, S. D., Behn, M. D., Cagnioncle, A., He, J., ... & Wang, K. (2008). A community benchmark for subduction zone modeling. Physics of the Earth and Planetary Interiors, 171(1-4), 187-197.

Van Zelst, I., Crameri, F., Pusok, A. E., Glerum, A., Dannberg, J., & Thieulot, C. (2022). 101 geodynamic modelling: how to design, interpret, and communicate numerical studies of the solid Earth. Solid Earth, 13(3), 583-637.