EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-768-RC2, 2022

Comment on egusphere-2022-768

Anonymous Referee #2

Referee comment on "The effect of temperature-dependent material properties on simple thermal models of subduction zones" by Iris van Zelst et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-768-RC2, 2022

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.

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 dependentcein the thermal coefficients (\rho, C_p, k).
- 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.

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.

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.

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

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".

L112 You relate the deviatoric stress to the deviatoric strain-rate. You aren't relating the "stress to deformation" at all.

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.

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".

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).

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.

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.

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.

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

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

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

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.

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.

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.

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.

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.

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).