EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-768-RC1, 2022

Comment on egusphere-2022-768

Anonymous Referee #1

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-RC1, 2022

I reviewed an earlier version of this manuscript that was submitted to another journal. I was unenthusiastic recommending publication because 1) the presentation in parts was very sloppy; 2) claims were made that the effect of including T-dependent properties was large whereas it was demonstrated in the paper that the effects were secondary compared to other governing parameters such as plate age or convergence velocity; and 3) that the (benchmark) model geometry and description used was unsuited to make inferences about thermal structure of subduction zones (even if it might be a useful geometry to test geodynamical codes).

The presentation has improved (but not completely, see below) and some of the most dramatic statements in the previous manuscript that suggested great importance of the T-dependence of the parameters in the heat equation have been removed, at least from the first parts of the paper. There are still quite a few (albeit repetitive) statements that I think are a mischaracterization of your findings (see below). You demonstrate that the thermal effects that you study are anything but secondary, if not tertiary, even when looking at the possible location of the BDT, compared to variations in the main driving parameters (slab age, speed, and dip). I will expand on my remaining concerns below.

As such I cannot recommend publication in present form. I realize a lot of work (and computer time and CO2 production) has gone into this paper. I could possibly be convinced that a revised version could be acceptable if a) the authors would phrase their modeling as a negative test of the hypothesis (because they demonstrate that T-dependence of k, c_p, and rho are minimal compared to the reference case of constant parameters; see below); and b) either a more realistic subduction geometry were to be used (see below) or that the heat equation would be solved as a time-dependent one with an evolution to 40 Myr or so – that should be enough to mitigate the pronounced negative effects of the benchmark model assumptions. As for b) I would prefer the former as then you can also include (more) realistic radiogenic heating and a more realistic wedge boundary condition for temperature.

I'll provide more details on my main two criticisms of this paper followed by a chronological list of issues that I think require attention below.

It is clearly demonstrated in the figures that the importance of T-dependent k, c_p, and rho, their effects are secondary at best. The cause for this is shown in Figure 2: the variations in the thermal range of interest (i.e., 400 C and above) are limited to 10-20%. The largest differences are near 0 C but this is not a temperature of great interest to subduction zone thermal modeling (except perhaps in the top boundary condition). The effect on the thermal structure of the incoming lithosphere is modest – the maximum difference at any given depth is a little hard to guess because of the graphics but it looks like 30 C or so. Rather minor compared to what you get when you change the age of the incoming lithosphere.

While there are some cases in Figure 7 that, side by side, suggest relatively large shifts in the depth of contours (e.g., 'case2c_k1' vs. 'case2c_cp3') there appears to be a minimal shift between the reference model ('case2c_PvK') and the model incorporating the T-dependence in all parameters of interest ('case2c_all'). The same is illustrated in Figure 9, where the maximum change is perhaps 40 km. That is minimal compared to the shift in isocontour depth that occurs when changing the slab age (as is shown nicely in this Figure). Clearly, the T-dependent variations in k, c_p, and rho are secondary (if not tertiary) to other subduction zone parameters such as slab age (shown here) and convergence speed (easily predicted by way of the thermal parameter).

I do not understand why the authors use this 'highly simplified' geometry with 'simplicity' (L135). I would say the model geometry and parameter assumptions are overly simplified and very far away from a 'generic' subduction zone (L140). There is no subduction zone on Earth that dips under a 45 degree angle to 600 km depth or that has no radiogenic heating in the overriding crust. Most geophysical observations exclude coupling at 50 km depth (e.g., Wada and Wang, Gcubed, 2009). The model geometry may be useful for benchmarking, but there is a huge artefact that occurs with temperature-dependent viscosity which is the formation of a very large and unrealistic 'viscous belly' (e.g., Figure 4c). This is a consequence of the assumption of steady-state which causes progressive cooling of the overriding lithosphere that effectively takes place over hundreds of millions of years and its thickness is enhanced by the lack of radiogenic heating in the overriding crust (see discussion in Hall, PEPI, 2012). Most subduction zones don't exist for that long and heat flow observations or observations of seismic attenuation clearly show that such a viscous belly does not exist (where we have such observations).

As such the variations in various figures in the lithosphere look much larger (see e.g., Figure 6a) than they will be in any (more) realistic subduction zone geometry. I predict that the temperature variations in the overriding plate will be restricted to the shallowest and coldest portions of the crust if more realistic subduction zone model parameters (as in, e.g., Wada and Wang, 2009; other papers cited in the ms.) were used. I do not know what the consequences for the thermal distribution in the slab will be, but they won't be completely insignificant. I think it is essential that the authors demonstrate that their conclusions still stand with a more realistic set of assumptions of the base model (including geometry, coupling point, wedge viscosity, radiogenic heating in the overriding crust, etc.).

L51ff. Many of the papers cited do not study the 'thermal evolution of a subduction zone in steady state'. Many of these use time-dependent modeling. Please fix.

L54, 60, other places. Please pick an upper case or lower case for references to 'van Zelst', 'van Dinther' or 'Van Keken' and stick to it.

Eq. (2). Why include the gravity term if you set it to zero? I don't think this equation follows the benchmark paper because of this reason.

L159. This seems like a large waste of computational resources. Why not solve the Stokes equation in the mantle wedge. Your code appears to be highly inefficient (you should be able to solve the benchmark cases in minutes on a single core of a laptop using existing codes but you appear to need to use Arc4 and German supercomputing resources) and making it even more inefficient by first solving the Stokes equation and then overwriting it with a kinematic condition just doesn't make any sense (at least, not to me).

L159. Do you really solve the Stokes equations in the crust? I am amazed you are getting a decent comparison to the actual benchmark, which imposes a zero slip boundary condition at 50 km depth (away from the slab).

L286, other places. Why do you use -half- the value of the computed conductivity? That seems excessive. What paper suggests that this is reasonable? The conductivity in the crust should be lower but typical values are generally only 20% lower than that of the mantle.

L343. I do not find it surprising at all that you get 'distinct differences' (even if they are 'outside the main focus [region?] of your study') because you use a different wedge rheology. This whole paragraph seems unnecessary.

L395. "the extreme effect in the overriding plate" Maybe I'm misunderstanding you here but I can't see how a temperature difference of 20 C (Figure 6a, others) is 'extreme'.

L412. I totally agree that the results are 'unrealistic' because of the 'artificial boundary effects'. That should give it away that this is not a generic subduction zone but much simplified model set up to allow for a simple benchmark comparison. This model should not be used for any other research purposes.

First paragraph of discussion: I cannot see how you can call 20 C or a change in depth of a contour by a few 10s of kilometers 'significant' or 'great'. There is a change, yes, but it is secondary compared to changes in more important driving factors of subduction zone thermal structure.

L502. “Neglecting temperature-dependent thermal parameters could result in significant errors of up to hundreds of kilometers in the estimated depth ….” You really do not show this anywhere in the paper. A person reading just the abstract and this part of the discussion (because perhaps this person is only interested in seismicity) would walk away with a thoroughly misled impression of your paper.

L512ff. More of the same. Just stating that things are significant doesn’t make them change from being (relatively) insignificant. You have not demonstrated this at all. Sorry to be repetitive, but I find it is necessary to call out repetitive mischaracterizations of your own work essential.

L572ff. This is not an original finding is it? I believe it is even in the benchmark paper.

L590. You seem to be repeating statements from an earlier paragraph. Irrespective, I wholeheartedly agree that you should not be using a subduction geometry that has a continuous dip of 45 degrees.

L649. This is completely cherry-picked. You choose a complete outlier that is based on a very selective comparison of extreme end-members of models. You clearly show that the variations between the reference case and your preferred case are minimal.

L670ff. I totally agree. I think you should explore this.

L673ff. Aside from the slightly awkward styling of the sentence, you do -not- show that temperature-dependent thermal parameters are an important modelling ingredient. See Figure 9.

Data availability statement. I do not know why one can submit a paper without making the “data” (or in this case models) available. Making them available after publication doesn’t allow for an evaluation of said “data” or models, at least not until after the fact.

References. I appreciate you cleaned up some of the most egregious mistakes in the previous ms. that I saw but a bunch of remaining ones are easily spotted particularly in capitalization, lack of correct typography, and spurious / missing information (‘Geophysical research letters’; ‘https://doi.org/xxx’; ‘H2O’; L886-887), article numbers that are confused with page numbers (L774, L799, others), or incomplete (L790) and nearly completely incomplete citations (L740).

I'm surprised that the authors do not seem to be aware of Chemia, Dolejs, and Steinle-Neumann, JGR, 2015. Seems like a highly relevant reference here.