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 1

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 CO_2 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).

We agree with the reviewer that the effect of temperature-dependent thermal parameters is not a first-order effect on the thermal structure of subduction zones and indeed takes a backseat compared to the choice of rheology or plate age. Nevertheless, the changes incurred by using temperature-dependent thermal parameters are significant in certain applications, such as comparison with earthquake hypocenters or when considering the exact depth of phase changes. Other applications, focussing for example more on the large-scale subduction dynamics of a certain region would indeed likely find that the effect of temperature-dependent thermal parameters is irrelevant. We have rewritten parts of the manuscript (see below) to make this distinction more clear and indeed emphasise that temperature-dependent thermal parameters are of less importance than first-order modelling ingredients such as rheology and plate age.

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).

We would like to emphasise that we do not at any point claim that we are modelling a generic subduction zone, because our model setup can indeed not be compared to any realistic subduction zone setting, as noted by the reviewer. Rather, we employ the 'generic modelling philosophy', as defined in Van Zelst et al., (2022), where we aim to better understand the subduction zone system's general behaviour and physics (i.e., rather than aiming to reproduce the specific state of any one subduction zone on Earth). We believe that throughout the paper we point out to the reader sufficiently that our model setup is highly simplified and the results and conclusions should be considered in light of our simplifications and assumptions (see, for instance, our title, abstract, and lengthy discussion on model limitations).

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.).

This is unfortunately outside of the scope of this manuscript. We trust that our extensive discussion as well as the mentions throughout the manuscript and abstract clarify under which assumptions our conclusions are valid.

Comments

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.

We have removed "in steady state", for clarity.

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.

The correct capitalisation of Dutch surnames is unfortunately something that (English) journals do not take into account, as the capitalisation of the word 'van' depends on whether or not there is something (a first name or initial) in front of it. Most journals therefore use the (incorrect) spelling of, e.g., 'van Dinther' in a citation, where it should be 'Van Dinther'. I have now chosen to use the (incorrect) non-capitalised versions for Van Dinther and Van Keken citations, because that aligns with the journal's preferred referencing format and I don't want to make assumptions on how they would prefer to have their name spelled. However, for myself, I use Van Zelst (i.e., with a capital V), as I find it important that my name is spelled correctly.

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.

We initially included the gravity term for completeness, but since it is equal to zero we have now removed it from the equation. We solve the same equations as the original benchmark paper by Van Keken et al (2008).

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).

The code is indeed inefficient as it has been adapted from the educational code FieldStone. The way the code is written is therefore easily readable and understandable to non-experts, although this comes at the cost of efficiency. Since we will share the code upon publication, we value high readability in our code. In terms of computational resources, the code could also have been run on a simple desktop or laptop, but due to the pandemic I did not (and still do not) have access to a work desktop or laptop. Therefore, we used the available local clusters of the University of Leeds and DLR.

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).

We indeed solve the Stokes equation in the crust, as we solve the Stokes equation in the entire domain. However, we also impose a no slip boundary condition at the bottom of the overriding plate (line 154-155). Together with the other boundary conditions, this results in the solution of the Stokes equation to be automatically 0 in the overriding plate and us recovering the solution of the benchmark. We follow the same procedure as in van Keken et al (2008).

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.

A value of half that of the mantle is appropriate at lower temperature conditions (suitable in the crustal layer, based on both mineral physics calculations (e.g., Grose and Afonso, 2013), and ocean core sample observations (e.g., Kelemen et al., 2004), which typically find values of 2 – 2.5 W/m/K, in comparison with values for olivine at such temperature of 4 – 5 W/m/K). We have added these references in the text. We also note that some convergence at higher temperatures is likely, but during the initial stages of subduction, a 50% reduction from the values for a pure-olivine mantle seems a reasonable approximation for an oceanic crustal aggregate.

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.

Indeed, it is not surprising that we get distinct differences due to the different rheology. However, we believe it is important to highlight these differences nonetheless, in case anyone wonders why they are there.

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’.

We reformulated this.

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.

As mentioned above, we never mention that we are modelling a generic subduction zone. Instead, we say we model a highly simplified and unrealistic subduction zone setup that follows the generic modelling philosophy of numerical modelling (Van Zelst et al., 2022). These models can be very insightful when determining the general, physical behaviour of a system and are therefore well-suited for 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.

We have rephrased this paragraph (line 464-471). The reviewer is correct that there are more important driving factors of subduction zone thermal structure; we hope we make this clear in the manuscript now. However, the changes induced by temperature-dependent thermal parameters are still large enough to be important when aiming for as-accurate-as-possible thermal models of subduction zones when comparing, for instance, to observed seismicity.

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.

We have reformulated this (line 516-517).

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.

See our response above; the changes are not first-order but still significant enough that they should be taken into account.

L572ff. This is not an original finding is it? I believe it is even in the benchmark paper.
This is correct and this is why we refer to the van Keken et al (2008) benchmark paper when making this statement.

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.

In this paragraph, we put our assumption into context so readers know how (un)realistic our assumption is, such that they can view our results and conclusions through the correct lens. As mentioned before, a different model setup is unfortunately outside of the scope of this study.

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.

This is not cherry-picked at all. Our preferred case is indeed the model where all thermal parameters are temperature-dependent and we present our conclusions on this model in a later paragraph (line 684-690). Indeed, the preferred model is also the model we mention in the abstract. However, for the completeness of the conclusions, we briefly discuss the individual effect of having temperature-dependent thermal conductivity, heat capacity, and density. When we include a temperature-dependent thermal conductivity, the effect on the 600 C isotherm is as big as stated here. In the rest of the conclusions, we also present the effect of the temperature-dependent heat capacity, and density, as well as our preferred model and other model batches that we ran (i.e., the effect of slab age and the effect of a crustal parameterisation). We therefore do not cherry-pick at all, but instead clearly state all our findings in a succinct summary. To clarify this in the text, we have slightly rephrased the conclusion.

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

This is unfortunately outside the scope of this study.

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.

We rephrased this sentence. However, we still believe that - based on Figure 7 and 9, for instance - we show that temperature-dependent thermal parameters are an important modelling ingredient. We clarify in the manuscript that temperature-dependent thermal parameters do not affect the thermal structure of subduction zones to first order, but the changes of a few tens of degrees that we observe and therefore a few tens of kilometers of the expected isotherm depth are indeed important, especially for seismological applications or interpretations of these models.

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.

We are happy to make the data available to reviewers in advance of publication by sending a .zip file or something similar. Unfortunately, we cannot upload this to the Solid Earth environment and Zenodo doesn't allow changes after publication, so in order to avoid uploading it twice, we made the data availability statement that we did.

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).

We went through the bibliography again and cleaned it up.

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.

We added this reference.

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.