

Interactive comment on “The effect of obliquity on temperature in subduction zones: insights from 3D numerical modeling” by Alexis Plunder et al.

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Answers to the interactive review of the paper: “The effect of obliquity on temperature in subduction zones: insights from 3D numerical modeling” by Alexis Plunder et al.

Dear Editor, dear reviewers,

First of all, we wish to thanks the reviewer for their time considering our work. Below you can find *answers* the questions asked by the referees.

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Anonymous Referee #2, Received and published: 26 March 2018.

Review:

The study presented here investigates possible temperature variations at the subduction interface due to the subduction obliquity. The motivation comes from geological data (Western and Central Turkey) and present-day configuration of the global subduction system, that large slab segments are subducting at an angle relative to their upper plate. The authors perform 3-D thermo-kinematic numerical models, in which they vary: a) the curvature (convex, concave, sinusoidal), b) amplitude of curvature ($\sin A$), c) and parameters beta and gamma, that control the shape of the curvature. Two additional simulations were performed on a subset (reference model, convex geometry) to investigate the effect of the subduction rate. The focus of the study is thus calculating the flow in the mantle wedge and the temperature variations at the subduction interface (how temperature profiles vary laterally). Results show that the effect of the trench curvature (obliquity) on the geotherm is considerable. Variations in obliquity can lead to temperature variations as large as 200degC along strike. The results are then discussed in relation to geological data from the Western and Central Turkey, and could potentially be applied to other present-day/paleo-oblique subduction zones. The manuscript at this point has a well defined structure, with clear and well documented results and conclusions. Some exceptions include insufficient figure captions, labels that need to be improved, and few paragraphs that need rephrasing/more details. I recommend the manuscript to be published in Solid Earth with major modifications, and I identify below 5 major points to be addressed, followed by other minor points. My comments primarily aim at clarifying some aspects of the model and results, and thus making the manuscript a more complete piece of work. Before answering in details, we note that the revised version of the manuscript has been carefully checked for

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possible remaining typos and mistakes. The figures have been revised considering the comments of referee #2, and captions have been re-written in more detail.

Major points: 1. Model details. a) Time stepping and temperature advection. Temperature advection (i.e. Page 8, Line 5) was suggested in a couple of locations as an important mechanism. However, it is not explained what temperature advection is (for the general audience), or how you solve for it (from Eq. 3).

Temperature advection, of more generally advection is the mechanism of transporting a quantity (vectorial or scalar). It is explained by the equation (#3) itself.

$$\mathbf{v} \cdot \nabla = v_x \frac{d}{dx} + v_y \frac{d}{dy} + v_z \frac{d}{dz}$$

where \mathbf{v} is the velocity field.

Then the temperature (scalar) advection becomes the following vector:

$$(\mathbf{v} \cdot \nabla) T = [v_x \frac{dT}{dx} + v_y \frac{dT}{dy} + v_z \frac{dT}{dz}]$$

$$v_x \frac{dT}{dx} + v_y \frac{dT}{dy} + v_z \frac{dT}{dz}$$

$$v_x \frac{dT}{dx} + v_y \frac{dT}{dy} + v_z \frac{dT}{dz}]$$

We are not sure whether advection really needs to be explained in a research paper. It has been explained in many textbooks and is rather a simple notion. In any case, all methods are explained in the appendix of the paper by co-author Thieulot (Thieulot 2011, PEPI)

A few questions to help here: When was steady-state (Abstract, Line 8) reached in simulations? How long did the models run? Did you solve just once for Stokes and T equation (1 time step)? How large was the time step? This is not clear. What about transient evolution of temperature and feedback to the system (i.e. flow of hot material that facilitates subduction)?

The Stokes equation is solved once (because we use a linear fluid). Concerning the steady state, we use a similar approach as mention in Currie et al. 2004, Wada & Wang 2009, of Kneller & van Keken 2008. This is now better stated in the manuscript. The time step is changing to respect a CFL condition (order of 5000 yr). The steady state is reached after 15-20 My, depending on the initial geometry. This is now stated in the manuscript. Because the fluid is linear viscous there is no feedback in the system.

The simulations are run until the temperature pattern in the slab is not mainly driven by the advection term of the energy equation (eq 3). We run the calculations until steady state is reached (ca. 15-20 Ma depending on the simulation with a time step of about 5000 years) on a Desktop machine using a single processor. Each model took about one to two hours to compute.

b) Model dimensions. What are the physical dimensions of the model? What is the physical resolution of the domain? Box dimensions are indicated in Fig 2a, but please include more information in the main text.

The physical dimensions of the models are provided on Fig. 2. They are now included in the manuscript. The physical resolution is 2.30-3-2.30 km and is now included in the model setup description.

c) Inflow/outflow boundary conditions (i.e. Page 6, Line 10). Could you explain better the in/out flow condition at 100 km depth? Did you choose this particular BC to allow for corner flow in the mantle wedge? If so, please indicate in text.

This is a “classical” approach for such models. As in other studies (van Keken et al.,

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2002; Curie et al. 2004; Wada et Wang 2009, or Wada et al., 2015) we prescribe an in/out flow boundary condition to allow for corner flow. This is now stated in the manuscript.

2. Subduction curvature and obliquity. Confusing interchange of “curvature” and “obliquity”. For example, Page 1, Line 10: One sentence uses “trench curvature”, and the next “obliquity”. Authors should make it clearer how obliquity and curvature are linked to each other.

True. We now clarified: “Real subduction zones, however, tend to be curved, i.e. trench strike varies laterally and the angle between the absolute plate motion at the trench and trench strike – the subduction obliquity – thus change along-strike”.

Global subduction zone. Page 2, Line 32, Figure 1: Measured subduction curvature depends on the trench length considered (i.e. Schellart et al 2007). When you make the statement “majority of subduction zones have concave, . . . or convex”, do you consider the length? What is the maximum curvature/obliquity (i.e. θ_{\max}) for the present-day natural system?

Thanks for raising that question. We just start from an easy observation. Trenches of subduction zones have shapes, and the majority of these shapes are concave or convex. The θ_{\max} today can be $> 45^\circ$ (In the Marianas for example). There are even extreme cases such as the Northern part of the Sunda-Sumatra system or the Aleutian trench, where the plate boundary becomes a transform fault.

The additional text reads: Some trenches contain as much as 90° curvature such that along the same trench, subduction may gradually (Aleutians, Sunda-Burma) or abruptly (southern Marianas, northern Lesser Antilles) change from near-orthogonal

subduction to near-transform motion.

3. Systematic study. First, all simulations (convex, concave, s-shaped) should be clearly listed as in Table 2, with corresponding varied parameters. Then, comparing the model results as in Table 2 across the entire simulation spectrum (v, v_y max, dT max) could provide more information on the general behaviour of the system. For example, that the largest dT are obtained for s-shaped simulations, what are the max/min bounds for dT for each geometry, or how subduction rate affects dT. I consider valuable information could be derived from an extended Table 2.

Table 2 was replaced by table 2 and 3 and was completed. We also added / completed figure 4 with depth-temperature paths of other experiments. Table 2 shown the variation of the reference model with respect to: subduction angle, age of the downgoing plate, velocity of the downgoing plate, and variation of geometry of the ref setup. Table 3 completes the former table 2 with all the settings.

4. The study needs more link to former studies on the topic. For example, Page 2, Line 16, Lines 24-25: Ji and Yoshioka (2015), Yoshioka and Murakami (2007) also investigated the relationship between slab geometry (convex, concave) and obliquity, and the thermal regime of the plate interface in 3D models. It deserves more explanations (what did they find, what is different in your model etc.) than just a mention. A comparison between their and your results should also be included in the manuscript.

A more systematic discussion with respect to previous models is now provided. The reference to Ji and Yoshika (2015) was better made. Concerning the other reference, we thank the reviewer as we have missed it. It is now added in the manuscript and discussed: These results agree well with previous numerical modeling work showing

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differences of temperature of ca. 100-200 deg. at 90 km depth (Bengtson2012, Morishige2014, Wada2015), ca. 120-350 depending the depth (Ji2015) or about 50 deg. C at the base of the seismogenic zone (Yoshioka2007).

5. The limitations of the model need to be discussed into more details (i.e. Page 10, Lines 24-27). What are the factors that could modify your results (max dT 200C)? i.e. revision of field data (different P-depth interpretation), model improvements (non-linear rheology for mantle), geometry, subduction parameters (age, velocity, subduction angle) etc. What about dynamic models (steady-state vs transient state of temperature)?

The fieldwork data for the Turkish case are pretty rock solid (different study by different groups over the last decades). So we are not sure the model (as simplified as they are) can really question them. The non-linear rheology would definitely be a nice addition to our modeling setup. Having a better geometry (a real subduction zone geometry) would also be a plus. Dynamic models would also probably lead to along-strike temperature differences. This could be a next step for our study building on the work of many authors by adding temperature in their dynamic models of subduction zone.

That said: the aim of our study is a test of physical plausibility, or, in other words, an attempt to falsify the hypothesis that subduction obliquity caused lateral temperature variation. We did not falsify that hypothesis, pending the assumptions we made. Future work may succeed in falsifying the hypothesis if more details are taken into account, but for now, our hypothesis stands.

Minor points: Page 1.

Line 1: The geotherm in subduction zones. *Changed*

Line 4: proposed/observed instead of supposed. *Replaced with “proposed”*

Line 7: Please revise the sentence: some commas missing and remove “only”. *done*

Line 8: the results in terms of: (i) mantle flow. . . , and (ii) temperature. . . *Comma added*

Line 12: heat that is advected by velocity causes such temperature variations (linked to the magnitude of the trench parallel component of velocity). *Sentence changed*

Line 17: are primarily. Lines 19-23: Sentence too long, please rephrase. Line 24: “with” instead of “whereby”. *The sentence has been shorten and revised*

Page 2. Line 2: trench perpendicular flow (poloidal). *Changed*

Line 11 (paragraph): Explain what is tem- perature advection and why it is important/of interest here? *We have added a sentence*

Line 13: trench curvature vs obliquity - should be explained what they are/stay consistent. *See earlier comment: we have explained this. In our view, the difference is pretty obvious: curvature is the along-strike change in trench strike, obliquity is the angle between absolute plate motion of the downgoing plate and the trench.*

Line 22: setup. *Corrected*

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Page 3. Line 6: with increasing. *Corrected*

Line 10: proxies to record them [lateral variations in temperature]. *Corrected*

Line 14: Melt-inclusion data suggest that temperature variations occur along strike and vary through <time>. This invites for some discussion about time-dependent (dynamic) variations in slab geometry. *This is now stated in our discussion*

Line 16-18: Please explain what's the difference between eclogite and garnet-amphibolite facies (i.e. high/low P,T) for the general audience. *A brief notice is now provided.*

Line 18: Yamato and Brun (2016) have shown that peak pressures recorded in subducted rocks might not reflect their maximum burial depths. This suggests that the assumption of transforming pressure into depth might not be the best practice. Could you comment on this aspect? How would that change the temperature variation estimated in line 19 (i.e. >300C) and how would that relate to your modeling results?

The point raised by Yamato and Brun (2016) is really interesting. In the Turkish case, there is a pressure and temperature difference (eclogite vs. garnet amphibolite). If we consider the pressure drop effect, we still need to explain the temperature difference: if the 25 kbar of the eclogite is not true and correspond only to 10 kbar (taking the fig. 1 of Yamato and Brun, 2016), the temperature estimate is generally solid (500C). Considering the garnet-amphibolite case (8-10 kbar), things are more difficult. The paper does not provide data below 10 kbar. If we consider that the behavior is purely linear, this implies a pressure drop of 5 kbar. The temperature is 800C (generally solid). We end up with a DT of 300C, that at a similar pressure will still exists and

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even increase (if we consider a linear geothermal gradient that is ok at first order). So despite the fact that lithostatic pressure might not be the best practice, the temperature variation is still there.

Because our paper specifically addresses lateral temperature differences, we do not include the discussion on the conversion of pressures to depth to our paper, since it doesn't change the point we're making.

Line 27: Please explain in a few words what is supra-subduction, as compared to subduction for the general audience. Supra subduction here is the ophiolite type. A sentence was added to explain what supra-subduction ophiolite are.

Page 4. Lines 3-7: Please rephrase this sentence. It is too long. The sentence was divided in two parts

Line 10: Nice transition/motivation to the next section. Thanks

Line 15: measurements. Corrected

Paragraph 22-33: This paragraph provides some background on previous studies investigating the effect of geometry (obliquity) on subduction dynamics. However, more should be included on studies that look at development of trench curvature (i.e. Schellart et al - convex/concave due to slab width, or sinusoidal when there is both trench advance and retreat), because these studies are more relevant to the present investigation.

A ref and a sentence about the paper by Schellart et al., 2007 is now provided. Thanks

for raising that point that we simply have forgotten in the amount of work present in the literature.

Page 5. Line 8: Why no analytical solution in 3D? Perhaps because of its too complex nature (i.e. take into account poloidal and toroidal components and other complex features)?

Generally speaking, the Stokes problem has an analytical solution in 2 or 3 D only with very specific boundary conditions (for example the SolCx, SolKz or SolVi benchmarks – passed by our code). The corner flow problem itself has an analytical solution in 2D (Batchelor, 1967, England et al., 2004), but we are not aware of any in 3D. Also, a lot of work was restricted to 2D in previous decades because of computer limitations at that time. In principle, 2D is a first order approximation, which yielded some important results, but with some limitations.

The authors could explain why the transition from 2D to 3D studies (i.e. trench obliquity is an inherent 3D feature). *The sentence was rephrased accordingly*

Line 9: setup. *Corrected*

Line 23: parameter values . . . Table 1. *Added*

Line 24: Decoupled energy equation: what about dislocation+diffusion creep, with P,T dependence for mantle viscosity? How would that affect temperature advection on the interface? *The model is isoviscous. Predicting the effect of disl. + diff. creep in such a 3D model might not be trivial, but following the 2D work of van Keken (2002) that would probably increase the temperature at the plate interface.*

Line 32 (throughout manuscript): Need to be consistent with units, especially for the time unit (yr): Ma, My, Myr, cm/yr etc. The occurrence of Myr was corrected to My. *Velocities are now only expressed as mm/yr and the occurrences of cm/yr were changed. My denotes the age of something without a reference (the age of the plate in the model). Ma relates to an absolute age.*

Model Setup: Should indicate before line 30, that that the model setup and boundary conditions are tuned for the Anatolian case study explained in Section 2. *This part was rephrased*

Page 6. Line 7: than. *Corrected*

Line 16: use. *Done*

Line 15-19: Any computational libraries that need to be cited here? *Indeed. The refs were added.*

Line 20: How deformed are the Q1Q1 elements to conform with the geometry? Is that affecting the accuracy of the solution? *The deformation of the elements is not big, therefore we expect no problems with the accuracy of the solution.*

Page 7. Line 9: multiple typos. *Corrected*

Why use these particular boundary conditions? Is mass con- served? *These BC sounds more realistic with respect to previous studies. And of course mass is conserved.*

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The inflow/outflow bc are not clearly explained (i.e. flow comes in horizontally from the top right boundary and flows out at the bottom boundary, conserving the mass. . .
This is now better explained in the manuscript

Line 24: sentence is not clear. *Rephrased*

Line 28, Figure 3: Why does the magnitude of v_y max decrease with depth? Is it a consequence of the model setup?

â€”

Page 8. Line 4 (end): Reference to Figure 4 is incorrect, as that observation was derived from Figure 3. *Corrected*

Line 6: becomes. *Ok*

Figure 4: insufficiently explained in the text/caption. What are the pink/light blue lines in figure 4a? *The figure caption is now properly written*

It also seems in Figure 4b, that path 3 is the warmest (compared to 1,2,4) - which corresponds to location theta max? *You are right. Going back to the data, the difference is 0.5C between path 3 and 2 or 4. We think that this difference even if it exist is not really relevant. (There is an extrapolation error introduced when we calculate the Temperature at a given position; i.e. not on the node of the model).*

The sentence was modified accordingly: Fig. 4 illustrates that the path in the centre of the model is the coldest and that paths where theta tend to a maximum are the

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warmest (paths 1 to 4 being within 1 degree at a similar depth; see zoom on Fig. 4

Line 14: What was the subduction rate for the reference model (sin20 1) compared to these values? *The velocity of this ref. model is 40 mm/yr (cf. fig. 2). This is now stated in the text and was added in the caption of figure 4.*

Line 15-16: Why an increase in subduction velocity produces such a polarity change in the temperature variation? This is not explained why that happens/results are not shown. The paragraph is not clear that it refers to the pink/light blue lines in Fig 4. Also, please clarify the differences between the reference model and these additional experiments. *The paragraph was completed together with the caption of figure 4*

Line 20: also called inflection points. *This was rephrased*

Page 9. Line 3: 100 or 110C (as in the Fig5c)? *We wrote "of more than 100C". It is now written of about 110C.*

Line 6: centre (purple contours), flow brings colder material from the surface to the centre of the slab. Concave models: I feel their model description is incomplete. The warmest part of the slab is in the center. How warm relative to the edges? What about v_{y_max} ?

We do not really understand this comment. The concave model is the inverse symmetry of the concave model. Nothing is changing except the shape of the trench. For Model -SIN20_1, the warmest part of the model is in the center, with a temperature difference of about 50C at 80 km, as in the reference model (SIN20_1). The horizontal velocity shows similar variation than in the ref model (see v_y velocity maps on Fig. 5)

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Line 16: max theta is in the inflection point. Yes

Line 20: sentence not clear. *The sentence was rephrased: The trench-parallel velocity reach a maximum of 8.0 mm/yr in model ATAN40_05, of 1.8 mm/yr in model ATAN05_20 and of 3.5 mm/yr in model ATAN10_20*

Lines 21-22: typos. *corrected*

Line 23: Why is the temperature field asymmetric in Figure 6b, slice at 75 km depth, as compared to a,c? *Looking at the data, this comes from the interpolation with the visualization software paraview (the 380C isotherm does not show this asymmetry anymore and the 370 is not present everywhere). The figure was changed with the 380 isotherm that is not confusing.*

Line 26, Figure 6: T variation for ATAN05_20 (fig 6b) is indicated 75C, not 200C as suggested in the text./ If you mean ATAN10_20, the T variation in fig 6c is indicated 110C, even if it looks around 200C. Please revise figure 6 and paragraph. *The figure and paragraph were revised*

Page 10. Line 9: In which way the results in this paper agree well with previous work? *The differences of temperature along strike in other work is now given to emphasis that our calculation agree well with other works.*

Line 16: Theta remains constant in the simulations here. Should make it clear and potentially discuss implications for variable Theta (age, velocity, subduction angle) *Theta is not constant here: it varies from 0 to theta max in each simulations*

Lines 24-27: The limitations should be extended a bit more. For example, how would power-law T, p dependent rheology of the mantle expect to influence the T variations? Are the T variations calculated here lower bound estimates?

This is partly discussed: “third, the isoviscous rheology we use is known to underestimates the temperature predicted in the mantle wedge (van Keken et al., 2002)”. In their paper, van Keken et al (2002) discussed the effect of isoviscous vs. non-linear (or power law) rheologies. It is difficult to predict if our T variations are lower bound, but considering the increase of velocity variation with non-linear rheologies (Jadamec and Billen 2010), they might.

Line 25: typo. *Corrected*

Page 11. Lines 11-12: What are the limitations/improvements of the numerical model that could produce a larger T variation as observed in Anatolia? *Amongst other: our model is ocean/ ocean, the Anatolian case study is continent going under ocean: that would probably change the downgoing plate velocity. There is ophiolite spreading/magmatism in the upper plate of the Anatolian case, so partial melting in response of fluid release could help. The rheologies are probably non linear in the reality.*

Figures and Tables.

Figure 1: Please add some labels/names of the major subduction zones used to illustrate in the text (i.e. Aleutians, remnants of W-C Turkey). b) The name abbreviations in the figure are not clear. Please either explain in caption or in figure. *The map was completed with names of the subduction zones. We also added the position of the fossil Anatolian subduction zone.*

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Figure 3: Labels for Fig 3g,h,i are missing (as indicated in the text on Page 8, line 2) *The labels g,h,i were part of an early version of the figure. They were removed from the text.*

Figure 4: Incomplete figure caption/figure legend. Labels are missing (a,b). What do the paths,vbc,"middle/edge" represent? Some sentences in the caption could help explain the figure better. *Figure caption was completed*

Figure 5: Please indicate simulation labels (i.e. sin20_1) in Fig 5a-b. c). *Done*

The colours for the maximum temperature paths are misleading. If they are all taken at the inflection point (yellow in the sketch), they should all be yellow (like the purple curves). Similarly for the concave model.

Yes, but they are not all taken at the inflection point. (see the 3d view with the 450C isotherm). The colour of the paths indicates their respective location (as on the inset).

Figure 6: Vy direction arrows as in Figure 5a,b would be useful. *Arrows are already present on the Fig. 6 (3 d views). We made them bigger.*

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