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Solid Earth Discussions

Interactive comment on "Thermal structure and intermediate-depth seismicity in the Tohoku-Hokkaido subduction zones" *by* P. E. van Keken et al.

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Dear Yamasaki-san:

Thank you for your comments and your positive and constructive assessment of our manuscript. We address your specific comments below and have modified the manuscript following your suggestions. Three figures (R1 through R3) that hopefully aid in the answer to one of your questions are provided in the supplement.

(2-1)Page 1072, lines 18-19: Please explain what physical parameter value is interpolated by means of a method described in Appendix A. This method is for the interpolation of positions (such as earthquake locations) that are precisely located in 3D

with respect to the top of the slab. In order to make sure the correct relative position is preserved in the 2D cross sections we need to do this over quite small intervals, particularly in the case of significant changes in slab shape, such as that occurring at the transition from Tohoku to Hokkaido.

(2-2)Page 1072, lines 19-20: Why "a simplified Moho" is used in this study, not using the Moho structure that is well imaged in receiver function and tomographic studies (see lines 4-7 on page 1071)? We used the cited papers to construct the finite element model but did not use the full details because this level of detail is unnecessary for the goals of this paper. The Moho is primarily important for the surface heatflow due to the distribution of radiogenic elements in the continental crust, which allows us to compare the models to the observed heatflow. The thermal models are not sensitive to the precise Moho location since the Moho sits well above the lithosphere-asthenosphere boundary.

(2-3)Page 1072, lines 23-25: "a combined diffusion-dislocation creep rheology" is adopted for the mantle wedge. It is essential to show rheological parameter values of dislocation and diffusion creeps used in this study. Please also explain a grain-size distribution in the numerical model and whether or not the grain-size has a time-dependent evolution (because grain-size is the principal parameter controlling diffusion creep). We used the parameters provided by Karato and Wu (1993) with constant grain-size. Since the equations are provided in Syracuse et al. (2010) and van Keken et al. (2008) we prefer not to repeat them here.

(2-4)It is also essential to show thermal parameter values used in this study. We have added the thermal parameter (as defined by speed times age) in Table 1.

(2-5)Please explain the mechanical and thermal boundary conditions on all the four boundary surfaces. We have added the boundary conditions (which are more fully described in Syracuse et al., 2010) in Figure 3.

(2-6)Please explain whether or not steady-state solution is obtained for the thermal

structures demonstrated in this study. The models are not in full steady-state. We evolve the models for 20 Myr. With the high speed of the slab this is sufficient to set up a steady-state thermal structure in the slab. As we documented in the benchmark paper (van Keken et al., 2008) and in Syracuse et al., 2010 the thermal evolution of these models is slow beyond this age. Changes are primarily in the growth of a 'viscous belly' below the lithosphere which changes the wedge flow (in what we consider unrealistic) but does not influence the thermal structure of the slab (much). For the purposes of this paper we can consider the slab thermal structure to be in quasi-steady-state.

(2-7)Page 1074, lines 19-23: Suggest explaining where in the model space "strong thermal gradients" are obtained. Only the statement "below this depth" is not enough. We have added "at the top of the slab" to clarify this statement.

(2-8)Page 1075, line 4: Please explain how P in Eq. T = 617 - 52P is evaluated, showing density values used in this study. I do not know how much ambiguity can possibly be present for the assumed boundary and how the ambiguity (if it is present) helps us to fix the inconsistency between the phase boundary and the seismicity beneath the junction between the northern Japan and Kurile arcs. For density we use 2.7 for the crust and 3.3 for the mantle; we take the variable Moho depth into account. The pressure in the slab below the forearc sliver will be lower (the maximum thickness of the sliver is about 20 km, so the pressure shift is equivalent to (3.3-2.7)/3.3*20 = 4 km deeper. Since the slab below the forearc sliver is still quite cold, the blueschist-out boundary is not present in the slab; it only occurs when the slab warms up down-dip from the forearc sliver. We added the geometry of the fore-arc sliver to figure 7c to help clarify this.

(2-9) The slab geometry may possibly be deïňĄned so as to obtain the upper plane seismicity just below the slab surface, which would explain the inconsistency found beneath the junction. We have added explanation of the estimation of the plate surface (in the beginning of the "Modeling approach") and explanation of the seismicity above the slab (seismicity in the forearc wedge, in the section 3.3). When we estimate the

plate geometry, we have used both of the upper envelope of the seismicity and events on the plate boundary events (repeating earthquakes of Uchida et al. [2003, GRL] and low-angle thrust fault events of Kita et al. [2010, EPSL]. These events are shown in Figure 3 of Kita et al. [2010, EPSL]), as we have added sentences in the beginning of the section 2. Cross-sections used in estimating plate surface are shown in Figures R1-R3. In these figures, it is clear that repeating earthquakes and low-angle thrust fault events are located in the plate boundary. From Figures R1-R3, we also easily confirmed that our plate geometry from Kita et al. (2010) can divide the events in the forearc wedge from events of the slab. Therefore, we can extract events of the slab from all events beneath the Hokkaido junction if we use the location of the plate geometry.

(2-10)Fig. 7a: The predicted surface heat flow is much lower than that observed in the junction between the northern Japan and Kurile arcs. Several possible factors can be considered, including (i) much more fore-arc crust has been subducted than expected from seismic tomography, (ii) thermal conductivity of the sliver may be lower, or (iii) 130 Ma age may be appropriate for the subducting Pacific plate (e.g., see Fig. 7b: lower seismic plane, where thermal influence from the wedge mantle seems to be minor, almost exactly corresponds to the âĹij 600 degree isotherm), but 30 Ma age may be too young for the other side boundary...... These are interesting suggestions that need to be explored. We agree that the similarity of thermal structure in the lower plane of seismicity strongly suggests that the dynamics and constitution of the mantle wedge, cold corner and geometry of the fore-arc sliver control the thermal structure and not the thermal structure of the incoming plate.

(2-11)Fig. 7b: Please explain the condition under which the thermal structure is calculated: what the full coupling depth is and whether or not the subducted fore-arc crust is present? The thermal structure in 7b is computed with a 90 km full coupling depth and inclusion of the fore-arc sliver, indicated by the solid black line (which have now labeled). We use a partially coupled zone (5

(2-12)Fig. 7c: Suggest showing the model result obtained under a condition that the fore-arc crust is subducted (so-called modified cross-section in the manuscript) and the full coupling is assumed below 80 km depth, with which the effect of the subducted fore-arc crust can be understood in a systematic way, and deleting the model with "full coupling at 90 km". Since the fore-arc sliver is imaged at a depth larger than 80 km we consider the 'standard' 80 km decoupling inconsistent in this case, as the transition to full coupling likely occurs below the crustal sliver. The reader could construct, without much difficulty, this hypothetical scenario by extrapolation of the currently displayed blueschist-out boundaries for 90 and 100 km coupling depth.

(2-13)Page 1077, lines 9-11: Suggest providing physical interpretation on deepening the blueschist-out boundary with deepening the full coupling, explaining why "the match with seismicity does not improve significantly". The blueschist-out boundary is deepened only in a direction parallel to the slab surface, though the seismicity deepens in a direction perpendicular to the slab surface. We currently cannot provide a full physical explanation of the location of the seismicity in this region. We had hypothesized that the fore-arc sliver would explain the deepening of the seismic belt (as suggested in Kita et al., 2010a) but the models presented in figure 7 demonstrate that in detail this hypothesis fails.

(2-14)Page 1078, lines 11-14: Does the following statement simply come from the model result that the predicted phase boundary does not fit with the seismicity distribution?: "....the suggestion that the presence of ïňĆuids (presumably released by the blueschist-eclogite phase change and at least partially transported back up the slab) is more important than dehydration embrittlement itself." This statement reflects that if dehydration embrittlement would be the prime mechanism than only at the loci of dehydration (predicted to be near the blue line) would show seismicity. Instead, seismicity occurs everywhere in the crust shallower than the blue line, which we interpret to be seismicity that is facilitated by the free fluids liberated at depth and traveling back up the slab.

(2-15)Page 1078, lines 14-16: What effects "a negative Clapeyron slope" has for resolving the inconsistency? Please explain. The negative Clapeyron slope is associated with processes where fluid generation outpaces porosity generation and that therefore leads to overpressure and seismicity. This has been suggested as an alternative to dehydration embrittlement in the two papers cited here.

(2-16)"4 Discussion": Suggest improving the discussion so as to clearly explain the relevance of 3-D subduction dynamics to the inconsistency particularly found beneath the junction between the northern Japan and Kurile arcs. What is necessary for resolving the inconsistency may be to avoid heating the subducting slab (a cold source is only the subducting slab itself). However, the arc-parallel deformation predicted from 3-D geodynamic modelling may bring about only additional heating of the slab as it is well known that a hotter material has more mobility. In addition, the authors should know that the arc-parallel deformation may also disturb that good correlation between the phase boundary and the seismicity beneath the other regions. We agree that there are many issues to be resolved here. We hope to be able to do some modeling with Dr. Manabu Morishige (who is visiting Michigan for the year) to help address the relative importance of these processes. The models that Morishige and Honda have shown (with revised manuscript submitted to EPSL) suggest the main impact is right at the junction at that away from the junction the flow behaves in a nearly 2D sense – which would suggest the 3D models would not deteriorate the correlation between phase boundary and seismicity away from the junction.

3.TECHNICAL CORRECTIONS

(3-1)Reference "Yamasaki and Seno (2003)" has not been cited in the text. Suggest deleting it from the reference list. Since this paper is relevant in the discussion of the generation of intermediate-depth seismicity we have cited it in the appropriate section.

(3-2)Reference "Kita et al. 2010" in Fig. 7 is not included in the reference list. Suggest supplying the publication detail or amending the year to be "2010a", "2010b", "2006",

or "2012". We intended to cite Kita et al., 2010a.

(3-3) Fig. 3: Suggest drawing the modified crustal structure (overriding crustal structure with the subducted fore-arc crust), as well as the unmodified crustal structure (overriding crustal structure without the subducted fore-arc crust). We have added a grey outline indicating the modified crustal structure used in Figure 9.

(3-4)Figs 4-7: Suggest drawing lines (may be green-coloured) for indicating Moho in the subducting slab in Fig. 4b, Fig. 5, Fig. 6, and Fig. 7c. We originally had indicated the Moho but decided that the images without the Moho were clearer and provided less clutter, given the density of the hypocenters in all these images.

(3-5)Fig. 4: Clarify the meaning of "the arc" (volcanic arc?). Check the scaling of heat ïňĆow (ticks are not uniformly distributed) in Fig. 4a. Please provide the interpretation on a solid line in Fig. 4b that obviously is not temperature contour. We have improved the image with these suggestions.

(3-6)Figs 5 and 6: Green-coloured line is suggested for indicating the top of the slab, keeping along with Figs 4b and 7b. That is a good idea.

(3-7)Fig. 7: Check the scaling of heat flow (ticks are not uniformly distributed) in Fig. 7a. Please provide the interpretation on a solid line in Fig. 7b that is clearly not temperature contour. Green-coloured line is suggested for indicating the top of the slab in Fig. 7c. We have fixed this.

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/4/C513/2012/sed-4-C513-2012-supplement.pdf

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Interactive comment on Solid Earth Discuss., 4, 1069, 2012.