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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 Kelin:

thank you for your comments and your concurrence with Dr. Yamasaki's assessment. We have modified the paper to address the points you raised along with our reply below. Please also note the figures we have provided as supplement to the reply to Dr. Yamasaki which hopefully aid in providing a quantitative basis to the answer to your first point.

1. The key aspect of the blueschist-out idea is that the seismicity band and the slab surface gradually diverge. They are together at the depth of 70-80 km, but the seis-

micity becomes deeper toward greater depth, and the separation of the two becomes about 10 km at 130 km depth or so. It seems that the relocation of the earthquakes and the location of the slab surface deeper than 70 km used different techniques each having its own assumptions and uncertainties. The logic of piecing the results together and the uncertainties involved should be clarified. If the earthquakes and slab surface are not simultaneously located using the same data and same procedure, how much confidence should we have in this key aspect? Kita et al. stated in their 2010 paper that the slab surface in the Tohoku region was determined by Zhao et al. (1997), and in the Hokkaido area was the upper envelope of the relocated hypocenters. If we use the upper envelope of seismicity to define the slab surface, the two will not diverge along proïňĄles T2, T18, T25 (Fig. 5). For other profiles, the definition of the "upper envelope" is not very clear, as we do see odd event above the slab (Figs. 5, 6). If these odd events reflect uncertainties defining the upper envelope, can the envelope be move up and down by a few km? These may not be hard questions to the seismological experts in the author team, but it is important to explain them to the readers. We have modified the section in which we describe the model geometry to answer these questions. Kita et al. (2010) estimated that the upper plate surface penetrates the locations of repeaters and is delimited by low-angle thrust faults. They also assumed the plate interface follows the top of the upper envelope of seismicity smoothly. The accuracy of the slab surface position is relatively low beneath the back arc (at depths of more than 80 km) in Hokkaido because the repeaters and thrust events disappear at a depth of 70 km. However, the same method, when applied to the Tohoku yields a geometry that is almost the same as that by converted waves (Zhao et al., 1997). Because of this consistency we estimate that this approach yields similarly reliable estimates for the plate geometry below Hokkaido.

2. It is not very relevant to describe a finite element model as "high resolution". First, the degree of details that a finite element mesh can resolve depends not only on the element size but also on the order of the shape function. With a high-order shape function, one can afford to use rather large elements. Would you call this kind of mesh *high-res or low-res? Second, it is difficult to measure what is "high". For example, for most of the model domain in the present problem, the element density used in this work is obviously an overkill. Where the field variables change slowly with space, very large elements can give very accurate results. On the other hand, a 1 km element size may still be too large where (the gradient of) the field variables change rapidly with space. I would remove "high resolution" from the list of merits of the new models and just give element size for critical areas.* We agree that a term "high-resolution" by itself isn't meaningful. Since the reviewer objects we've removed it and rephrased the sentence that discusses the methods by making it more specific. We had already stated the finest resolution and provided a picture of the mesh. We tend to do this in most of our papers. We have paid a fair bit of attention to developing finite element meshes that are optimal in terms of efficiency and accuracy and we are well aware of the points the reviewer raises. In fact, as is shown in the benchmark paper (van Keken et al., PEPI, 2008) we've even worked with Kelin and his group on this! There are various trade-offs in choices of elements – for the subduction zone work with variable slab geometry (i.e., slabs that are not straight lines) we've found the use of linear elements ideal and at the employed resolution quite accurate (since we always do convergence tests for the models we run and stated that explicitly in the submitted manuscript). With higher order elements it becomes harder to match slab shapes if they are determined, for example, by seismological methods and the use of optimal grid refinement becomes more difficult.

3. Frictional heating is ignored in these models. This does not affect the region of focus of this paper, but it does make predicted the heat flow near the trench too low. It is wise to advise the reader to ignore the shallow (near-trench) part of the model results. We have added a comment to this effect. Yes, we ignore shear heating along the seismogenic zone. For the cross-sections in this paper we find that the fore-arc heat flow is reasonably well satisfied; in Peacock and Wang (1999) the referee showed that modest amount of shear heating might be necessary and we agree that modest amounts of shear heating are permitted and that they would make the top of the slab in

the forearc a little warmer. This minor increase in temperature is completely overprinted by the heating of the slab when it gets into contact with the hot mantle wedge, and the assumption of no shear heating has indeed no effect on the thermal structure in the slab below 80 km depth.

4. The Appendix is not necessary. These operations do not need explanation. Also, compared to potential errors in the relative position of the earthquakes and slab surface (see comment 1 above), along-strike dip of the slab surface over a 10 km corridor seems to be an exceedingly minor issue. Our original assumption was that we could simply use the GMT project tool over a 20 km wide segment around each cross section. This fails however in the junction where the slab dip perpendicular to the cross-section is significant. We found errors in location that were in excess of 5 km here. Given the high relative precision of the seismicity with respect to the slab we found that we needed to adopt the non-standard procedure detailed in the appendix and we wish to retain this to make sure the results in this paper are reproducible. We have clarified the reason for the use of this procedure.

5. A few specific comments. 1070-26. high convergence rate of the old -> fast plate convergence and the old age of the 1071-16. Delete the word "fully" 1073-27. Please clarify "linear Taylor-Hood triangles". Is the velocity or pressure linear? For the convenience of most readers, I would just specify the interpolation orders for velocity and pressure without mentioning the term "Taylor-Hood". 1078-28. arc -> arcs 1079-8. south-east ward -> south-eastward We have modified the text following these suggestions, with the exception of the deletion of the term Taylor-Hood. While it's likely that most readers wouldn't care too much whether we used Taylor Hood or Crouzaix-Raviart elements, a few will. We have clarified it is linear for both pressure and velocity.

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