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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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This paper reports the results of a thermal modeling effort to test the hypothesis of dehydration embrittlement being responsible for earthquakes in the subducting crust of the Japan Trench subduction zone. The idea that the distribution of the shallowest earthquakes in the slab follows the blueschist dehydration boundary was originally proposed by Kita et al. (2006) based on the thermal model of Hacker et al. (2003). An issue with the Hokkaido corner and the idea of downdip transport of continental material were discussed by Kita et al. (2010). In this new work, the authors used more advanced modeling method and developed 2D models for many trench-normal profiles

along the Tohoku-Hokkaido margin in order to address these issues in greater detail. The conclusion is that the idea of blueschist-out boundary controls seismicity of the subducting crust generally holds except for the Hokkaido corner. The model cannot explain why the idea does not work for the Hokkaido corner, and the conclusion is that some 3D mantle wedge dynamics might be responsible. This is a useful exercise.

I have read the review by Dr. Yamasaki, and I agree with most of his comments regarding the clarification of a number of technical aspects of the thermal modeling. I will therefore not repeat those points.

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 seismicity 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 profiles 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.

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

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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.

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.

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.

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

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