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Solid Earth Discussions

Interactive comment on "Subduction to the lower mantle – a comparison between geodynamic and tomographic models" *by* B. Steinberger et al.

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We thank Hana Cizkova for her comments, and respond to them as follows:

Comment:
1.The model description. In general, I would prefer at least a short model description in the text (or supplementary material). Here the reader is refered to a recent paper of Steinberger and Torsvik (2012) for the model details. It may however make it easier for the reader, to give here at least the basic information – governing equations, model resolution and possibly also the radial proïňĄles of viscosity and thermal expansivity.

Response:

We can give such a more extended description in the revised version. This will mean some repetition of material from Steinberger and Torsvik (2012) and earlier work, but we agree that this would make the manuscript more easily readable, as the reader will not be required to look up the previous work.

Comment:
I would also suggest to explain clearly the difference between the models st12den-1, st12den-2 and st12den-7 in the model (methods) section.

Response:
Again, we can do that, and agree this would make the paper more easily readable. In fact, following the comments of reviewer 2, we suggest to include a table summarizing features of models st12den-1 to 7.

Comment:
Furthermore, I don't understand completely, how the density model of the upper mantle is constructed. The authors state, that "Between their time of initial subduction and 14 Myr later, slabs are not included in the density anomalies that drive the mantle flow." (page 855, par.5). Does it mean that the upper mantle density anomalies are kept zero and the slabs in the lower mantle are in fact "detached"?

Response:
That is correct; in model st12den-2 slabs are kept zero in the flow calculations (but not in the correlation calculations), and in this way, slabs in the lower mantle are in fact "detached". We can explicitly write that. In our extended description, we will also write that slabs in model st12den-1 are added at a shallower level – smeared out between depths 150-450 km. Nevertheless, results for both cases are quite similar (see e.g. Fig. 7) indicating that the depth at which slabs are inserted into the model does not fundamentally alter results at the (rather low) level of detail we are considering here.

Comment:
If this is the case, why do they need to "extrapolate alpha to the surface" (page 854, par. 20)?

Response:
Again, this question should go away, with a more detailed description of model st12den-1, where slabs are inserted in the upper mantle. We suggest we can replace "extrapolate alpha to the surface" by "extrapolate alpha to the upper mantle". We also note that - in particular in cases st12den-1 and st12den-2, due to the presence of upwellings - density anomalies do in fact occur right up to the surface.

Response to comment 2: We fully agree that our statement that "higher viscosities would ... be difficult to reconcile with geoid constraints" was wrong, as the geoid is not sensitive to the absolute viscosity level. The paper of Steinberger and Calderwood, however, also considers global heat flow and the "Haskell" viscosity average to constrain absolute viscosity levels. We suggest to replace "reconcile with geoid constraints" by "reconcile with constraints for fitting geoid, global heat flux and postglacial rebound". Shifting the viscosity profile to higher viscosities does indeed lead to somewhat reduced sinking speeds. We have run a test case where viscosity has been increased by 50 % everywhere – which is probably already above the upper limit of what is compatible with the "Haskell" postglacial rebound constraint, and found that this reduces sinking speeds, inferred from a depth-vs-time plot as in Fig. 3, by only about 10 %. We suggest we can mention this result in the paper. However, as we have already pointed out in the paper, we think it might rather be an effect of lateral viscosity variations (regionally higher viscosities in those regions where subduction has been going on for a long time, rather than viscosities higher on global average) and the effect that in regions where subduction has been going on for a long time, the density contrast between slab and surrounding has been reduced, further reducing

slab sinking speeds. We suggest that such effects should be further considered in future work.

Response to minor comment: We hope to hear from the editors which figure size is planned in the final version. If it is the same size (which would be o.k. with us) we can certainly produce figures with larger labels. In the updated Figure 5 (attached to response to reviewer 2) we have already increased label sizes.