Solid Earth Discuss., 4, C466–C470, 2012
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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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Received and published: 21 September 2012

We also thank Reviewer 2 for his/her comments, and respond to them as follows:

Response to comment 1:
As we are not sure which (if any) of our improvements are truly significant in a statistical sense, we suggest to remove "significantly" in the abstract, and merely state that "correlation is improved", and also remove "significant" in the discussion and merely state that "our models provide an improvement compared to earlier models". We note, though that it is fairly easy to formally test for statistical significance using standard methods, but that might not be that meaningful. One of the problems is to get the true number of degrees of freedom. Assuming we compute

correlations for $L = 20$, then theoretically the number of degrees of freedom (DOF) = $(L + 1)^2 - 2 = 439$. With that, 90% bounds on a correlation of, say, 0.2 are 0.091 ... 0.30. Put differently, a change from 0.2 to 0.3 correlation has a 90% of being real. Since $r_{20} = 0.21$ for the model of Steinberger (2000), the improvement would be not significant for st12den-1 ($r_{20} = 0.25$) but it would be significant at the 90% level for st12den-7 ($r_{20} = 0.34$), which is most similar to Steinberger (2000) in that it is a “slabs only” model with no lower thermal boundary layer and hence no active upwellings but, in this way, as the reviewer points out, geodynamically less meaningful. The true number of DOF, though, is perhaps quite a bit smaller than the number of parameters in the expansion, on which the formula above is based. We would prefer to not include such a discussion in the paper and hence just not use "significant".

Response to comment 2:
The insufficient model description was also criticized by reviewer 1 in her comment 1. We are going to provide a more detailed description. See also our response to reviewer 1 comment 1.

Response to comment 3:
In Figure 1, we attach a suggested revision to Figure 5, where we have also plotted MIT-P08 and which therefore has six columns. Given the page setup, we should be able to plot this without reducing the size of the panels. Accordingly, we suggest to extend the description of cross sections in section 3.2.2. to also account for these cross sections.

Response to comment 4:
Yes, this mechanism can surely also help to slow down slab sinking, and we would like to follow the suggestion to add this to our discussion.

Small comment a:

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Our treatment of phase boundaries is indeed quite simplified. We had explained this on page 855, lines 13-22:

"In model st12den-2 the Clapeyron slope of phase boundaries is considered by adding mass anomalies at the depth of the phase boundary. SpeciïňĄcally, we add here a mass per area at depth 660 km that is equal and opposite to the density anomaly just below 660 km (here at depth 700 km) multiplied with a 77 km thickness. This thickness corresponds to a product of Clapeyron slope and density difference of 300 (MPa/K)(kg/m^3) as suggested by Akaogi and Ito (1999), and a thermal expansivity of $\alpha = 2.1 \cdot 10^{-5}$ K (Steinberger and Calderwood, 2006). In the same way, we use a thickness 105 km (and equal, not opposite, density anomaly) for modeling the effect of the "410", however this plays almost no role, as density anomalies are inserted into the model below that depth."

We have also compared models with phase boundary (st12den-4) and without (st12den-5) and found they look very similar, i.e. phase boundaries have no strong effect on our model. We plan to mention this in the paper. We suggest to also mention that complexities of slab ponding at 660 may well be to blame for the poor fit, and that this could be addressed with future modeling, along the lines of what the reviewer says.

Small comment b:
Does conductivity vary with depth as well?
We use a constant thermal diffusivity. In our model, thermal diffusion is mostly important in the base of the mantle (as we explicitly insert slabs and do not consider the top thermal boundary layer such as not to include the same effect twice). We wrote in Steinberger and Torsvik (2012):

"We use here a diffusivity of $0.95 \cdot 10^{-6}$m^2/s. For c_p = 1250 J/kg/K [Schubert et al., 2001] and ρ = 5500 kg/m^3 this corresponds to a thermal conductivity of 6.5 W/m/K,

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somewhat above the lower end of the range of thermal conductivity estimates for the lowermost mantle, which vary by a factor of ~ 5 [e.g., de Koker, 2010; Goncharov et al., 2009, 2010; Hofmeister, 2008; Stackhouse et al., 2010]."

We can write something along these lines in the course of our more detailed model description (comment 2)

How big an effect is neglecting lateral heat diffusion?
The diffusivity can also be written as κ = (50 km)^2/100 Myr. Assuming a diffusion of $2\sqrt{\kappa t}$ in time t, this corresponds to a diffusion of about 100 km (less than 2 degrees of arc just above the CMB) in 100 Myr, the approximate sinking time. Compared with the width of the anomalies in the figures, we expect this to be a rather small effect and the assumption is justified. We can write something along these lines in our revision.

How large is basal heat flow and how does it affect CMB temperature? We would like to add a reference to Biggin et al. (2012), Nature Geoscience, where we have discussed this issue in more detail. CMB temperature is assumed constant

Small comment c:
To address this issue, we did a simple test where we correlated st12den-1 with smean, smean-, and smean+ where the latter two were derived by scaling negative and positive anomalies with a factor of 2.5, respectively, while leaving the other anomalies at unity scaling. The correlation plots are attached in Figures 2 and 3 and results can be qualitatively understood: The correlations are overall similar for all three models, but smean- has somewhat worse performance because the slow anomalies aren't predicted that well by mainly slab-driven models. Smean+, on the other hand, is almost identical to smean correlations with st12den-1, consistent with the suggestion that we're matching fast anomalies best, and increasing the amplitude of those doesn't make much of a difference. We suggest that it is not necessary to add these extra

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figures, but sufficient to add 1-2 sentences to that end to the text.

Small comments d and e:
We are happy to implement these suggested changes as well. Our suggested table (attached as Figure 4) does in fact include all our models st12den-1 to st12den-7. The model in panel 6b is st12den-6 and has correlation values r_8 = 0.23 and r_{20} = 0.14.

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