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Solid Earth
Discussions

Interactive comment on "Comparing a thermo-mechanical Weichselian ice sheet reconstruction to GIA driven reconstructions: aspects of earth response and ice configuration" *by* P. Schmidt et al.

P. Schmidt et al.

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We thank the Anonymous referee #2 for a thorough and constructive review that have helped to improve the quality of the manuscript. Here we reply to the remarks raised by the referee in the order they were listed.

1 Replies to "General Remarks"

1.
The manuscript have been substantially revised.

2.
Interestingly, Eaton et al. (2009) (as referenced by the reviewer) defines in section 2.9 the "elastic lithosphere" as "The mechanically strong outer shell of the Earth that can support applied loads elastically and without permanent deformation." and further concludes that "the effective thickness of the elastic lithosphere depends on the residence time of the load." In other words the effective thickness of the elastic lithosphere seen by different processes need not be the same. Further we note that the notation T_e is used in Watts (2001) (also referenced by the reviewer) when discussing the elastic thickness of the lithosphere as derived from late glacial rebound (e.g. in section 4.2.1 and 4.2.2 as well as in table 6.2). That said we will replace the notation T_e by LT as this is more commonly used the GIA literature. We will further skip the term "elastic thickness of the lithosphere" and simply use "thickness of the lithosphere" or "lithospheric thickness".

3.
We have added additional references in the introduction to previous studies where more than one ice sheet reconstruction is used. We further more clearly point out the difference between these studies and our. However we chose to only briefly compare the preferred Earth models of other studies to the best-fit models in this study as the optimal Earth model parameters is not the target of this study.

4.
The intention of the model suite with uniform mantle viscosity was to scan the misfit to the GPS data as a function of lithospheric thickness. However, the reviewer is correct in that these models are special cases of models with a 2-layered viscosity structure in the mantle. As the result of the uniform models is not of significant importance to this

study we have removed them from the study.

5.

We are now using the Lidberg et al. (2010) solution to the Bifrost data and have added a description of the work of Zhao et al. (2012) to the intro as well as a brief comparison between our best-fit models and their results. We do not use the horizontal component it has been found that the methodology we use (i.e. the Wu (2004) implementation of the flat earth approximation) may result in too large horizontal displacements (Schotman et al., 2008), although inclusion of material compressibility (as also done in our study) was found to improve the quality of the predictions. Since we have not yet tested our implementation in this respect we can only assume that the results found by Schotman et al. (2008) also applies to our implementation and therefore we will not use the horizontal displacements. In the revised version of the manuscript we explicitly express this when describing our model implementation. We have further removed references to the horizontal component throughout the manuscript, except when discussing potential improvements to our model.

6.

Our current implementation does not include the core-mantle boundary at 2900 km depth. We expand the sub-surface of our model in all directions to a radius of about 41,000 km where we apply so-called semi-infinite elements, so in principle the mantle is modeled with an infinite depth. We have run a test models with more material layers in the mantle, including the core-mantle boundary. The difference between such a model and model with the layering given in table 1. are very small however, indicating that the resolving power of the GIA process in Fennoscandia is limited. This is further confirmed by several studies, e.g. both Paulson et al. (2007) and Zhao et al. (2012) concludes that only three layers can be resolved beneath Fennoscandia: the lithosphere, the upper mantle and the lower mantle. In contrast to the recommendation in Wu (2004) we do not use foundations but spring-elements to implement the pre-stress advection. The reason for this, as shown in Schmidt et al. (2012), is that

foundations will not yield the proper forces as soon as the material interfaces are not perpendicular to the gravitational acceleration. This said, the use of springs instead of foundations is of minor importance to this study as we do not consider laterally varying thickness of material layers. We therefore do not spend to much time on describing this technicality but give a reference to Schmidt et al. (2012) where the implementation is fully described.

In the revised manuscript we have expanded the description of our model setup to include a description of the boundary conditions used.

7.

The bifurcation seen in the misfits of UMISM and ICE-5G is present at all the stations well within the formerly glaciated region while for stations outside the formerly glaciated region a saddle-point alike pattern can be observed in the misfits in the upper/lower mantle viscosity parameter space. Stations located around or close to the LGM-margins of the ice sheet display a transitional pattern. Figure 1 displays the characteristics as a function of upper/lower mantle viscosity of model misfit to the BIFROST data (Lidberg et al., 2010) at individual stations. Stations where a clear bifurcation in the optimal viscosity is seen in the viscosity in both the upper and lower mantle are marked by red triangles, stations where the misfit displays a saddle-point alike pattern in parameter-space are marked by yellow triangles and stations displaying a transitional pattern are marked by blue triangles. Examples of the observed patterns for all three ice reconstructions are displayed in the nine panels to the left in Figure 1, with station for which the misfit applies indicated above each column and on the map. As the uplift rates (both observed and predicted) are larger in the formerly glaciated region the variation of the misfit at a single station, as a function of the material parameters, will also be greater than at a station outside the formerly glaciated region. Hence the misfits will be dominated by the misfit at the stations where the bifurcation arises as explained in section 4.3.

8.

As we do not solve the sea-level equation nor compute the geoid heights in our model a comparison to RSL data is strictly not feasible. In light of this issue being raised by the reviewer P. Whitehouse we have chosen to remove the comparison to RSL data from the manuscript. We do however still compare uplift curves predicated by the different reconstructions and have in addition to the uplift curve along the Ångerman river added predicted uplift curves at Tromsö, Norway, and Blekinge, Sweden, from 10 kyr BP until present day.

9.
The reviewer is correct in that our suggested areas of improvement may be flawed by the neglectance of the ocean-load in our model. We have now added this information to our discussion. We further note that Wu et al. (2010) (as suggested by the reviewer) finds that the uplift rate is sensitive to the ice thickness in all of the regions where we discuss modifications to the ice reconstructions. However, it should be noted that the ice reconstructions used by Wu et al. (2010) are the predecessors to the ANU and ICE-5G reconstructions used here, further Wu et al. (2010) assumes uncertainties almost twice the formal uncertainties of the Bifrost data as given in Lidberg et al. (2010) and used in our study.

2 Replies to "Small remarks"

1.
We have modified the title of the manuscript slightly to avoid the use of "GIA-driven"

2.
We have rewritten the abstract with emphasis on making it more concise.

3.
We have reworked the introductory section substatially including adding references where appropriate.

As pointed out by the reviewer, observational data on ice sheet thickness may be found in mountanious regions. We have therefore adjusted our statement about the availability of observational data on ice sheet thickness.

4.
Appropriate references have been added.

5.
The development of ice sheet volumes for all three reconstructions is displayed in the top panel of Figure 2 in the manuscript. The numbers on ice volumes, as expressed in the figure, multiplied by a factor of approximatelly 2.78 corresponds to sea-level equivalents in meters.

6.
We have changed the annotation ICE-n to ICE-x to avoid confusion.

7.
Our model now uses springs instead foundations to simulate the pre-stress advection term in the governing equation. This is all described in Schmidt et al. (2012) as referenced in the manuscript. This allows us to properly model layers with laterally varying thickness. However, as the present study only covers models with uniform layer thickness (1D models) this model update has no direct consequences for our results. We still mention the modification to give the reader the possibility to reproduce our model but leave the details to be found in the reference.

8.
The expansion of the sub-surface of the model is done to avoid boundary-effects. Test models have shown that expanding to a radius about 10 times the dimensions of the problem will be enough to ensure that the applied boundary conditions do not affect the solution in the region of interest.

9.

The reviewer is correct in that the 160 km lithospheric thickness by Steffen and Wu (2011) is related to the resolution of GRACE, in fact this is briefly mentioned in the conclusions in Steffen and Wu (2011). We have now removed the direct comparison to this value but note that similar lithospheric thicknesses have been estimated by others as well using both RSL and tide gauge data (see Table 3 in Steffen and Wu, 2011).

10.
Section 5.1 have been reworked and part of the section have been moved into the discussion

11.
Section 5.1.1 have now been moved into the discussion section

12.
We do not agree that the entire section 5.2 should be a subsection of the discussion. However we have reworked the section to more clearly separate the results from the discussion.

13.
The indicated case-errors have been corrected

14.
We have added a comment to our discussion pointing out that our suggestions may be flawed by not including the ocean load.

15.
We agree with the reviewer that solving the sea-level equation is important, for one thing this is neccesary if comparison to RSL data is performed.

16.
We have replaced "post glacial" with "post-glacial" throughout the manuscript

17.
The addition of a line indicating the average VM2 viscosity in Figure 1 would only clutter

the image, so since this value is not really of major importance for this study we chose not to represent it by a line in Figure 1.

18.
The colors used in all figures in the manuscript have been edited to ensure that different colors can be mor easily distinguished.

19.
We have removed the uniform mantle models entirely which includes the left panel of Figure 6. 120 km is a commonly published estimate of the lithospheric thickness in Fennoscandia as seen by the GIA process and estimates as high as 160 km have been suggested (see e.g. Steffen and Wu, 2011, for a summary of previous studies).

Although it would be very interesting to add misfits for additional lithospheric thicknesses we will not do so by two reasons. First of all the objective of this study is not to find the optimal Earth model parameters for the three ice reconstructions, but to compare the reconstructions to each other. As we compare the GIA-predictions to observations it is however un-avoidable that we arrive at some estimates of the earth model parameters and therefore also that these be compared to estimates in other studies, but again this is not the objective of this study. Secondly a practical issue, these models do not run in an instance. In fact the total runtime of the models in a single panel in Figure 6 takes about 3/4 of a month to run and post-process on our system, hence to add misfits for two more lithospheric thicknesses would take about 4 months which is significantly longer the time period we have to revise the manuscript.

20.
We have removed the lower part of Figure 7. but chose not to add figures of the modeled uplift rates as we do not find that these will contribute significantly to the manuscript.

21.
We consider Figure 8 to be important for the discussion of general trends in the residual

velocities predicted by the UMISM and ICE-5G reconstructions and will therefore not remove this figure.

22.
Figure 9 and Figure 10 have been edited and merged into a single figure.

23.
As we do not use gravity data in this study it would be very hard to motivate the inclusion of such a figure as suggested by the reviewer.

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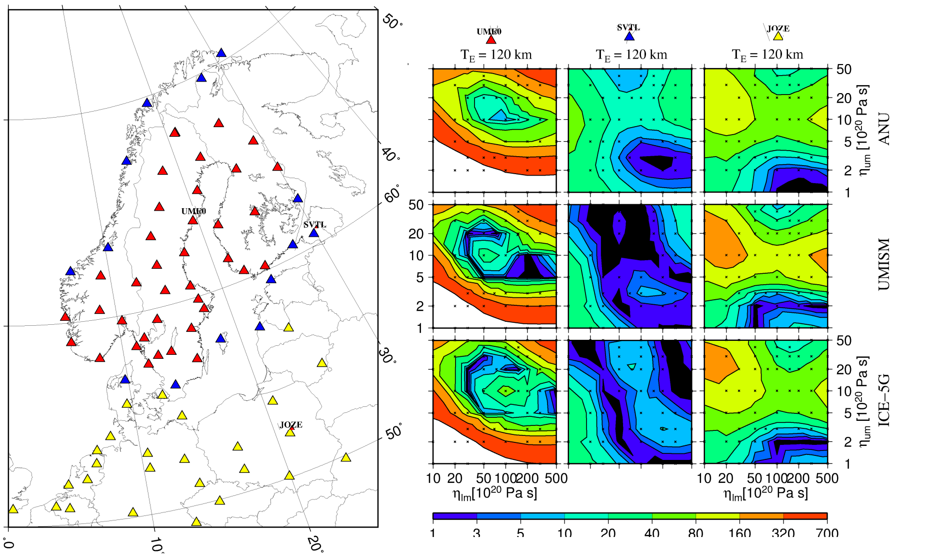

Fig. 1. Characteristics as a function of upper/lower mantle viscosity of model misfit to BIFROST data at individual stations.