Solid Earth Discuss., 5, C913–C921, 2014
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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.

Anonymous Referee #2

Received and published: 21 January 2014

The authors compare three different ice models of the Weichselian glaciation in Fennoscandia and their performance in glacial isostatic adjustment (GIA) modeling as to predict present-day uplift rates and the relative sea-level (RSL) curve at Angerman River. The study is to my knowledge the first that compares in a more robust way the two different types of ice models used in GIA investigations. This is of broad interest as most (if not all) studies in the last two decades relied on either the ICE-xG series from the University of Toronto (Peltier et al.) or the ANU-ICE (or RSES) model from the Australian National University (Lambeck et al.), and both these models include earth model information which can bias earth model parameters in a dedicated analysis. The UMISM ice history is based on thermo-mechanical information and thus should provide interesting new information. A literature review shows that the model has been used by the Uppsala University group since several years, thus they have a long and solid expertise working with this model. The comparison is very welcomed and important for the GIA community. In this regard I would like to add that it is important for future investigations that the UMISM model is available for the interested researcher (and the updated ANU-ICE model as well). Otherwise, the information as presented here cannot be reproduced, confirmed and further evaluated.

The novelty and importance being on the positive side, the manuscript, however, needs major revision. There are a few issues regarding the structure, length and the usage of terms which should be improved. In addition, authors should improve their motivation of the paper. A comparison of ice models can be done by anyone who has access to all of them. Just some thoughts: What time and space resolution has been used in previous studies? Did they use time steps older than Last Glacial Maximum? Which modeling technique was used in these studies for earth model parameter determination? Why do you use a regional Finite Element model now and not the common normal-mode method? Because of the resolution? Why do you use vertical velocities only, when you know that bifurcation can most likely result? Did you do sufficient tests to avoid bifurcation? Last but not least, the authors state a few times that the motivation is not to estimate earth model parameters, but, frankly, half the paper deals with exactly that. Please revise accordingly.

General remarks:

1. The structure needs a complete revision. Please stick to a common frame like Introduction, Data, Methods, Results, Discussion, Conclusion. While sections 1-4 in your manuscript stick in a general way to that, the last three sections are, frankly, quite chaotic mixing results, discussion and conclusions. This eventually

leads to much repetition of information and tiring of the reader. Some facts are written four times (including the abstracts). Authors should carefully go through the manuscript and especially the last three sections. Results should only summarize the main values for each model combination and where/when to find them. Discussion evaluates the results. Conclusion is the summary, which should also include an outlook. The latter can also be a separate section if quite long.

2. Please avoid the term “elastic thickness of the lithosphere” when you talk about the elastic outer layer of the Earth as it is used in GIA modeling. And moreover, do not use the symbol T_e! It can be a lengthy and quite exhausting discussion whether or not the lithosphere in GIA modeling equals what others have in mind when talking about T_e. There are different opinions about it, and there are indeed colleagues who state that what is defined as lithosphere in GIA modeling is not a lithosphere at all! To make this discussion short, simply use “lithosphere” and “lithosphere (or lithospheric) thickness” as it has been used in the last decades in GIA-related papers. I also suggest that you have a look into McKenzie and Fairhead (1997), Eaton et al. (2009), Tesauro et al. (2013) and Watts (2001) to get an overview of what is discussed as lithosphere, how different definitions of lithospheres are related to each other and how (effective) elastic thickness of the lithosphere fits into that.

3. There are more papers besides Steffen et al. (2010) that compared different ice models in Fennoscandia. Zhao et al. (2012) compared the latest ANU-ICE (note that they write ANU-ICE, not ANU) to ICE-5G briefly, and used the Lidberg et al. (2010) GPS result to estimate earth model parameters. This has to be included in the introduction, and your results need to be compared to their results in the discussion. Van der Wal. (2013) used next to ICE-5G an ice model developed by Stocchi et al. (2010) for Fennoscandia, which is also independent of any earth model information and is based on simple ice dynamics and meltwater contribution proxy. The difference between the ice models themselves is not

extensively discussed, but different results are compared. Note that the time span and resolution of the ice models used in all these studies is different from your comparison – this should be stated in the manuscript to further highlight its significance. Finally, note that Sasgen et al. (2012) used a thermo-mechanical for North America to compare to GRACE. Thus, such models are more and more widely used in GIA modeling.

4. I do not see the benefit of discussing the models with just one mantle viscosity. This is basically just a special case of what you call a 2-layer model, e.g. in Fig. 6 that are diagonals in the upper left corner in the 9 subplots on the right. We know that there is upper and lower mantle, so why play around with just one layer? Or do you try to estimate Haskell's value (see e.g. Mitrovica 1996)? Furthermore, you do not specify why you do it. Removing this part will reduce the length of the paper and may also give room for additional interesting analysis (see further below).

5. You should use Lidberg et al.'s 2010 BIFROST solution as well. Show the main results in Table 2, and add the main results from Zhao et al. (2012) so that the reader can compare. Additionally, it would be good to list and/or discuss further GPS results as listed in Steffen & Wu (2011), and maybe results based on other data as well. Are you results much different of those? As you write so much about horizontal velocities, include them in the misfit calculation and show the residuals in the figures - or do not discuss such results, but then state clearly why you don't use them.

6. I was wondering about your model set-up that you describe in Table 1. Do you have just one lower mantle layer? And is it infinite? Did you check that this does not lead to errors? There is a core-mantle boundary at about 2900 km depth, which is an important boundary condition in GIA models. The lower mantle can be subdivided in several layers (at least shallow and deep lower mantle),

which also have different (average) material parameters than the ones indicated there! Your set-up here is, as far as I understand, different from the original one introduced by Wu (2004). Are there any boundary conditions? How do you apply the so-called foundations?

7. Regarding bifurcation in your results, I wonder if you tested what would happen if you only take GPS stations in the center, those with largest uplift rates. Does bifurcation appear as well? It may be that the chi-square value for models that fit the central GPS station results well, but not those in the periphery, is similar to that from models that fit the peripheral stations well, but not the central ones.

8. It would be good to extend the comparison to RSL data. More data are certainly available, especially from the Tushingham & Peltier (1991) database. This should reduce the range of models with good fit. Note also a discussion in Wu et al. (2013) on the different meaning of GIA data.

9. Be careful when suggesting areas of improvement in the ice models in section 6. This may be the case, but note the missing ocean load along the coasts as well as in the Baltic Sea, and the accuracies of the GPS data. Compare c.f. Wu et al. (2010) where GPS data should help in GIA modeling.

Small remarks:

- Think of changing the title to something straight forward like “Comparison of different reconstructions of the Weichselian ice sheet and implications for improvement” or “Weichselian ice sheet reconstructions in GIA models: thermo-mechanical vs. GIA-driven ice models” Note that GIA-driven may not be the best word in this regard. Main input and physics are sea-level equation, sea-level equivalent and ice extent.

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- Reduce the information in the current abstract. Add a sentence regarding the earth model set-up and the space and time resolution of the ice models.
- P. 2347, L. 14-27: Add references to the statements. L. 21/22: This is not true as stated. In mountainous areas information can be retrieved along valley slopes. Also nunataks can provide information. However, that is just a tiny piece and won't help so much in continental-scale models.
- P. 2348, L. 1-3, 6-13: Add references.
- P. 2349, UMISM: can you provide a sea-level equivalent of this model?
- P.2351, l. 23: use another letter for the VM- earth models, otherwise the n suggest that ICE-5G has VM5 as associated earth model.
- P. 2357, l. 5/6: what is the model update and its benefit for this study?
- P. 2357, l. 9: Why do you expand it?
- P. 2360, l. 18: 160 km appears to be related to the resolution of GRACE. GRACE seems to be not able to resolve lithosphere thickness due to its wavelength.
- Parts of 5.1 can be moved to discussion.
- 5.1.1 is discussion.
- 5.2 should be a subsection of the discussion.
- P. 2367, l. 11: Baltic Sea. L. 21: British Isles.
- P. 2367, l.17/18: I would be careful with this statement as you do not include the ocean load. This may shift the center of mass to the expected location.

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- P. 2368, l. 4-14: Note that next to other data, also solving the sea-level equation is important. Also note results by Wu et al. (2010) when discussing importance of horizontal velocities.

- Please use RSL and postglacial or post-glacial, but not post glacial.

- Add a line for the average VM2 viscosity in the upper and lower mantle to Fig. 1.

- Use different color for Baltic Sea or negative values in Fig. 5. They are hard to distinguish.

- Delete left panel in Fig. 6. Why did you choose 120, 140 and 160 km? Please add two more, e.g. 90 km (the one used in ICE-5G).

- Remove lower part of Fig. 7, but add a row on top showing the modeled uplift field.

- Remove Figure 8.

- Delete right part of Figure 9. Integrate Figure 10 in Figure 9.

- Would be good to have another double-panel figure showing a cross section along one of the gravity lines in the Nordic countries. Upper panel shows ice thickness from each model there at LGM and lower panel current uplift as predicted by each best model.

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Interactive comment on Solid Earth Discuss., 5, 2345, 2013.