Solid Earth Discuss., 5, C1126–C1134, 2014
www.solid-earth-discuss.net/5/C1126/2014/

Interactive comment on "Lithosphere and upper-mantle structure of the southern Baltic Sea estimated from modelling relative sea-level data with glacial isostatic adjustment" *by* H. Steffen et al.

H. Steffen et al.

holger-soren.steffen@lm.se

Received and published: 4 April 2014

Comments to the reviewers' report on 'Lithosphere and upper-mantle structure of the southern Baltic Sea estimated from modelling relative sea-level data with glacial isostatic adjustment' by Holger Steffen, Georg Kaufmann and Reinhard Lampe to Solid Earth.

We have revised the paper taking into account both reviewers' comments. Below follows a detailed list of how we have responded to the individual comments (marked

in *italics*) by Wouter van der Wal. The revised manuscript has been attached as supplement. Changes are marked in bold letters.

The reviewer notes our study as an important step towards improving our knowledge of the regional Earth structure, without performing time-consuming 3D models. However, he suggests that sources of uncertainty and correlation between the parameters should be addressed and discussed before a publication.

The ice models have an implicit lithosphere thickness, so inferences with such ice models are biased towards a certain lithosphere thickness (and upper mantle viscosity).
Yes, we already provided this information in the introduction of the ice models. We have extended this introduction and also discuss this issue now in the Discussion as well to inform the reader.

It is clear from figure 4 that there are errors in both ice loading histories that are not canceled even for variation in earth model parameters.
It is clear that the ice-load histories are not perfect (otherwise they would be identical...). We mention possible errors in our area of interest now.

Using a 1D models for different regions is not the same as using a model with 3D variation in lithospheric thickness for the entire region.
This is discussed now in the text.

Timing errors in the RSL data are not considered.
The reviewer is wrong here. They are indeed considered: they are converted into an additional height error (Lambeck et al. 1998) to ease a misfit calculation. The height error then includes the effect $|dh/dt|_t\sigma_t$ (Lambeck et al. 1998), where $|dh/dt|_t$ is the rate of sea-level change at time t and σ_t the age error. Hence, the height error becomes larger while the time error is set to zero.

The true rheology might not be a Maxwell rheology.

We mention that other rheologies are discussed in the literature.

When considering these sources of uncertainty in addition to observations errors, in equation 2, the confidence regions of the parameters become larger.
Of course, this is clear and is now mentioned in the text.

Also note that some of Lambeck et al (1998) results that you discuss on page 2495 are outside the confidence region in this paper. The correlation between ice history, lithospheric thickness and mantle viscosity worsens the problem. As example, the ice loading history could be totally off, but the error can be absorbed by the free parameters in the model (lithospheric thickness for each subregion, upper mantle viscosity, lower mantle viscosity) such that you can still get a good fit with a wrong model.
Yes, this is possible.

Given that a combination of GIA observations can separate only two layers in the mantle because of correlation between viscosity in different layers (Paulson et al. GJI 2007) can you really expect that with a smaller set of the RSL data you can constrain all these parameters? I suggest a more extensive discussion of the error sources and how these might affect your conclusion.
Point taken. It is now discussed.

Finally, when looking at the confidence regions in figure 3, the inferred lithospheric thicknesses for most regions are not statistically different, while the conclusions in the paper and in the abstract suggests otherwise. The conclusions and the abstract should be modified to match the confidence regions in figure 3. Specific examples and other comments are given below.
We have revised the manuscript according your suggestions and discuss lithospheric thickness in view of its statistical significance.

Specific comments

- *p. 2489 l. 25 and fig. 1: please explain what criteria you used to select the*

subsets. It would greatly improve the paper if you show results for at least one other selection of subsets as this might have a large impact on the results. Also, please provide more details, perhaps in the supplementary material, about the criteria you used to select reliable data (p. 2490 l. 11). Are any of the RSL data also used in the construction of ICE-5G and RSES ice models?
We have added a description about the criteria for our grouping and also provide more results for other groupings in the text and in Table 1. The criteria for “reliable” RSL data is explained. According to our information, the data of Oslo Fjord, Limfjord, the Great Belt and 44 data of the southwest Sweden subset were used by Lambeck et al. (1998) for the construction of the ice model. Most of the other data were found, dated and published after 1998, thus it appears unlikely that they were used by Lambeck et al. (1998). Except the RSL data collection of Tushingham and Peltier (1992), we do not have access to the complete dataset used by Peltier (2004) to construct Ice-5G. We can only speculate if a similar dataset as compiled by Lambeck et al. (1998) was used. It is possible though to assume that German Baltic Sea RSL data published after 2004 were not included in the construction of Ice-5G. This also holds for both ice models for the archaeological data from the Hensbacka site in southwest Sweden.

- *p. 2491, l. 20: What is the effect of cutting off at degree 192? Is there a large Gibbs effect at the continent boundaries in the ocean function?*
  Cutting off at degree 192 is sufficient for 1x1 degree investigations. Although computation time is not the biggest issue with the code, it saves some time when running almost 1100 different earth models and their comparison to a few hundred sea-level data.

- *p. 2492 l. 2: please explain why no timing errors for the RSL data are used (as I understand from figure 1?), while these are shown in, for example, Vink et al (2007).*
  There are timing errors in the data, but are recalculated into a space error, see

our explanation above.

- *p. 2493 line 6: In some cases (figure 3b, 3g, 3h) you find a best-fitting lower mantle viscosity which is equal to the lowest value in your suite of models. Therefore technically you can not conclude that the lower mantle viscosity is larger than the upper mantle viscosity.*
  We have rephrased this part in the manuscript.

- *p. 2493 l. 2-15: From figure 3 the ranges of acceptable lithospheric thickness values are: Oslo: 60-75 km; SW Sweden: 100-160 (but really 60 to 160 if you accept both ice models could be true); Fyn High: 70-150 km; German Baltic Sea: 60-155 km. Given that any value within the range is equally likely, one can not conclude that the lithospheric thickness is increasing from west to east. For one sigma the only significant difference is between Oslo and SW Sweden. In fact, the lower mantle viscosity is for most regions equally well determined as the lithospheric thickness. You conclude this yourself on p. 2494, l. 11: "lithospheric thickness not strongly bounded" and on l. 17 "lower mantle viscosity... clearly determined". The text in the conclusions (p 2487 line 18 to 24) and the abstract should be modified accordingly.*
  We have modified both abstract and conclusions accordingly. We also not that p. 2494, l. 11: "lithospheric thickness not strongly bounded" relates to the discussion of the bifurcation feature. We have revised this part as well.

- *p. 2495 l. 6: A low number of samples does not explain a large misfit because the definition of the misfit accounts for the number of samples. In fact, a smaller sample could in practice be easier to fit. Could there be some other systematic errors in the data or the modeling? See also abstract line 19 which should be modified because your confidence region for Poland is actually smaller than for the German Baltic Sea coast*
  The reviewer is right. We have deleted this sentence as it is misleading. We

kindly rebut the suggestion that there is a systematic error in the modeling. We can only speculate what the reason for the misfit is. It can be data themselves, unknown tectonic behavior or subsidence affecting the data, the ice model, or a combination of both. Such a discussion has been added to the conclusions in view of the general comment.

- *P. 2495, last part of the second paragraph: Here it is shown that both ice models can not fit the data within observational errors. Please explain how the error in the ice model would affect the misfit and the confidence region.*
  An explanation has been added.

- *p. 2495 l. 19 to p. 2496 l. 3. Any ideas why Lambeck et al (1998) results are outside your confidence region?*
  We use an updated ice model of the one used in Lambeck et al. (1998) as well as a different sea-level data set for southwest Sweden. We have added this information in the manuscript.

- *p. 2496 l. 11: Please give the method by which the lithospheric thicknesses are interpolated to obtained the map; Did Vink et al (2007) and Steffen and Kaufmann (2005) use the same ice load histories?*
  The interpolation has been done with the GMT pscontour function. Both Vink et al. (2007) and Steffen and Kaufmann (2005) used the same ice history. This information has been added.

- *l. 20: it seems weird to say that because there is no good fit a thorough analysis is hampered. One could also say a thorough analysis is required? At least an explanation should be offered why the seismically and thermally inferred lithospheres are much thicker.*
  This part has been rephrased regarding the different definitions of lithosphere.

- *p. 2497 l. 24: "a perfect match is not possible" why not? A lot can be said about*

this, I suggest to remove this sentence here.
Agreed, sentence removed.

- *l. 25: "upper mantle viscosity is about [2-7] x 1020 Pas." How did you get these values? In figure 3g I see acceptable values for the upper mantle viscosity that are well below 1020 Pas.*
  Sentence rephrased.

- *line 27-29: Here you accept an upper mantle viscosity to be able to reject an ice model (see also p. 2492 l. 14). I would suggest that you remove this conclusion because in most of your paper you accept the ice histories and use upper mantle viscosity as a free parameter.*
  Removed.

Technical corrections

- *abstract: I am not sure Fyn High is familiar enough to use in the abstract without explanation*
  We have replaced Fyn High with Ringkøbing-Fyn High, which is a commonly used name for this feature. One can easily find several 100 publications using this name (e.g. check Google Scholar). We thus think there is no explanation needed.

- *l. 5: change "subside" to "have been subsiding"*
  Changed as suggested.

- *p. 2485 l. 1: Remains challenging is vague*
  Changed "More investigations have to be undertaken".

- *introduction: The introduction is in my view too long. In particular the first three paragraphs can be shortened as for example GPS is not very relevant to this*

paper, while a detailed discussion of existing lithospheric thickness estimates is better done in the discussion of the results. On the other hand, references to studies with 3D models are missing (see general comments, second item).
The first three paragraphs are condensed, i.e. the GPS part has been removed. The discussion regarding lithosphere and the models has been moved to the discussion. Reference to 3D models are included now.

- *p. 2486 l. 23 change "an elastic rheology" to "a purely elastic rheology on the GIA time scale"*
  Changed as suggested.

- *l. 25 "under debate": not clear if the existence of an asthenosphere is under debate, or the viscosity in the asthenosphere?*
  We have moved this part to the end of the introduction and added more information so that the meaning becomes clearer.

- *p. 2487 l. 2 add a reference, because this statement you later confirm*
  Reference added.

- *l. 13: what is "the lithosphere" in this case?*
  Explanation from Eaton et al. (2009) added.

- *l. 23 please explain mesosphere?*
  This term was used by Gregersen et al. (2002) and we have used it in reference to this paper. It goes back to a definition of the 1940s. Mesosphere refers to a mantle region, which is located under the lithosphere and asthenosphere, but above the outer core. Gregersen et al. refer to it in Figure 6 of their study as relatively high velocities in the upper mantle. According to this figure, the lithosphere and mesosphere of the Fennoscandian Shield cannot be distinguished. We have rephrased this part to avoid the term mesosphere as it is apparently not much used and known.

- *p. 2487 l 21 to p. 2488 l. 17 This text is not used to formulate the research question so it would be better to place it in the discussion of the results where you also discuss figure 5.*
  Moved to discussion.
- *p. 2488 l. 23: contribution to what?*
  Changed to "analyse the isostatic behaviour".
- *p. 2491 l. 9: at what depth is the boundary between upper and lower mantle assumed in the model?*
  670 km, which is added now.
- *p. 2494 line 19: "small adjustments..." this sentence is isolated from the rest of the text.*
  Sentence rephrased to fit into context of this paragraph.
- *p. 2497 line 15: "determined" better replace by "investigated" Figure 3 is according to me the most important figure in the paper, please enlarge the plots and font size.*
  Word replaced. Figure was so small due to the format of the discussion paper. Please see the supplement/attachment how the figure is intended to be sized in the final version.

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/5/C1126/2014/sed-5-C1126-2014-supplement.pdf

Interactive comment on Solid Earth Discuss., 5, 2483, 2013.