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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.

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General comments

This study divides available sea level data in subsets and fits mantle viscosities and lithospheric thickness in a GIA model to each subset. Conclusions are drawn about the variations in thickness of the lithosphere. It is certainly interesting to see what variations in lithospheric thickness are obtained and if the values agree with the lithosphere as it is inferred by other methods. This is a way to get information on the 3D litho-

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spheric structure without doing time-consuming computations with GIA models with varying lithosphere, and thus it is possible to search a larger parameter space. This is an important step towards improving our knowledge of the regional Earth structure. However, some important issues have not been discussed and should be addressed in a revised version of the paper.

There are sources of uncertainty and correlation between the parameters that the authors have not considered or discussed, specifically: - 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). - 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. - 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. - Timing errors in the RSL data are not considered - The true rheology might not be a Maxwell rheology. When considering these sources of uncertainty in addition to observations errors, in equation 2, the confidence regions of the parameters become larger. 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. 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.

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

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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.

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?

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? 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).

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.

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

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18 to 24) and the abstract should be modified accordingly.

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. p. 2495 l. 19 to p. 2496 l. 3. Any ideas why Lambeck et al (1998) results are outside your confidence region?

p. 2496 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

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? 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.

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. 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. 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.

Technical corrections

abstract: I am not sure Fyn High is familiar enough to use in the abstract without expla-

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nation; l. 5: change “subside” to “have been subsiding” 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). p. 2486 l. 1: Remains challenging is vague; l. 23 change "an elastic rheology" to "a purely elastic rheology on the GIA time scale"; l. 25 “under debate”: not clear if the existence of an asthenosphere is under debate, or the viscosity in the asthenosphere?; p. 2487 l. 2 add a reference, because this statement you later confirm; l. 13: what is “the lithosphere” in this case? l. 23 please explain mesosphere? 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. p. 2488 l. 23: contribution to what? p. 2491 l. 9: at what depth is the boundary between upper and lower mantle assumed in the model? p. 2492 line 19: “small adjustments..” this sentence is isolated from the rest of the text. 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.

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

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