

Interactive comment on “Sediment loading in Fennoscandia during the last glacial cycle” by Wouter van der Wal and Thijs Ijpelaar

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Note: author reply in between « and »

This paper presents a clear and very well presented discussion of the incorporation of sediment loading effects into calculations of sea level change and other observables related to GIA. I think this discussion is warranted and contributes to the accuracy with which GIA effects are calculated. I would suggest this paper be accepted with minor technical revisions which I identify below:

« We thank Dr. Purcell for the effort and for the helpful comments. In our reply below , please note that line numbers refer to the document with tracked changes which is attached to this reply. »

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1) Page 1, Line 29: "shown known" should simply be "shown"

« done »

2) Page 2, Equation 1: This formula is mis-labelled and is incorrect. The quantity represented here is not sea-level it is water-depth. In this context I should be multiplied by since it is not total ice thickness that is important, only that portion of ice thickness that displaces water.

« We follow the definitions of Dalca et al. (2013). In there (p. 460) the sea-level is defined as the height of the sea-surface equipotential relative to the solid surface. In that case the solid surface includes the ocean bottom + sediments + the top of the ice sheet. That means the topography will be equal to the negative of the sea level, i.e. the height of the solid surface with respect to the sea-surface equipotential. To make this more clear we add "Defining the sea-level as the difference between the equipotential corresponding to sea-level and the solid surface" p3 l18. Ice that is floating is not considered, cf the check in equation 4. »

3) Page 3, Equation 3: With the amendments suggested above the grounding line becomes $SL_j > 0$

« Given the definition that we follow the check is first to see if there is ocean in the absence of ice. Then the check is to see if the weight of the ice height is larger than the weight of sea-level that it replaces, which is the G-R-H which is $SL+I$. To make the statement hopefully clearer we added "sea level is positive in the absence of ice and" on p4 l5. »

4) Page 5, Line 3: "entire" should be deleted or replaced with "all of" or "the whole of"

« changed to "all of" »

5) Page 5, Line 7: "largest of such" should be "largest such"

« done »

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6) Figure 3: The projection used for this figure makes the geography a little difficult to interpret.

« This figure is moved to appendix B and latitude and longitude are added »

7) Figure 4: It is very hard to make out the modern coastline which makes the figure difficult to interpret.

« This figure is removed »

8) Figures 2, 3, 4, 5 and 6 use different projections. It would, I think, be better to standardise.

« figure 3 and 5 now have the same projection and figure 4 is removed. New figures 4 and 6 have the same projection »

9) While there is a discussion of the uncertainty in the viscosity model and the observational record the uncertainty in the ice load seems to be the most significant element for this analysis. If GIA data are used to constrain the ice sheet and sediment changes are not considered then the change in ice thickness will be biased to compensate for the neglected sedimentary load. The implicit assumption that ice thickness is fixed and known is inaccurate.

« We think we did not suggest that the ice load is perfectly known, only that when the ice thickness is inferred from GIA observations such as uplift rate, the estimated ice thickness is biased by neglecting sediment transport and other model errors. Model errors are now also mentioned in the conclusions p14 l6. »

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/se-2017-18/se-2017-18-AC1-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2017-18>, 2017.

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