

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

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« please note that author replies are in between « and ». Page numbers refer to the revised manuscript which is attached to this reply with track changes. We thank the reviewer for the comments which helped us to strengthen our manuscript, especially the introduction and conclusions. »

This study quantifies and presents the contribution of the sediments deposition in the GIA signal for the region of Fennoscandia for the first time. The authors implement a published methodology in their own codes and use sediment data retrieved from different (available) sources. Therefore the work is original and the results are sound. They clearly show that the sediment contribution for Fennoscandia is quite small and

C1

comparable to measurements error, but it is a potential source of systematic bias.

« That is a good summary of our work »

I do not have major concerns; however the English and the overall presentation can be certainly improved. In particular I found the motivation in the introduction poor and not clear. Consequently also the discussion (and conclusion) sounds just drafted. For example it is not clear at all if the result was expected or not. Some sentences suggest that the expected effect should be more important than what actually found, but there is no discussion about it at all. The main motivation for this work is that the sediments deposition is expected to have a contribution comparable to the sea level feedback. However, on one hand the glacial erosion can build up kilometres of sediments in million years, so its absolute contribution is “large” (P 12, L 13 of the MS) but the deposition rate is actually small, usually few millimetres per year. On the other hand, the sea level feedback is a global effect that cannot be ignored, and the local sea level loading effect can be much larger than few millimetres per year. For example the retreating ice is replaced by the water, and if it is not correctly included the error is comparable with the ice loading effect. The difference between the effect of the sea level feedback and the sediment deposition should be addressed (in both introduction and discussion).

« This is a good comment. We improved the logic in the introduction, explaining how sedimentation compared to sea level and why it could be expected to affect GIA observables Please see the revised text in the introduction. »

The authors note that in some other part of the world the sediment deposition has been proven to cause present-day subsidence. The state of the art, of those studies in particular, should be described in the introduction and comparison should be made in the discussion. Has that deposition occurred with higher rate? Is it more localised? Is there any similarity in those studies to the Fennoscandia sediment deposition? Or what are the differences?

C2

« A more extensive discussion of the magnitude of the viscous effect to sediment loading is provided in the introduction and comparisons are made in the discussion and conclusions, please refer to those sections. »

Points that need to be cleared or rephrased: P 1, L 25: "sediment deposition in glaciated regions vary from millimeters per year to centimeters per year, which is comparable to changes in sea level during the last glacial cycle". Do you mean the relative sea level? Do you mean local RSL in the same glaciated areas or in general the sea level effect in other regions?

« clarified and reference added in p2 l1 »

P 2, L 7: "2,5 million years resulted in a sediment layer of several km thickness". This is misleading. For example 5 km in 2.5 million years is 2 mm/yr.

« The number is replaced by an estimate for the last glaciation p2 l29 »

P 2, L 22: The Method section is not clear enough. Concepts defined in older studies are used here without properly recalling them. It is confusing even for people familiar with iterative method for solving the sea level equation.

« More explanation is added, please see the methods section »

In Eq. 1 the SL variable (supposedly Sea Level) is not defined.

« done »

P 3, L 2: In Eq. 2 the L is not defined. Add L: The total surface mass load "L" is defined as the sum...

« done »

P 3, L 5: "At each time step"... It is not declared that it is solved with a step-evolution. P 3, L 9: "inner iteration" is not clear since you have not defined the method as iterative in a general way. P 3, Eq. 4:  $C_j$  is used without defining it.

C3

« These are included in the revised explanation of the method »

P 3, L 18: Why "the effect of erosion on the location of coastlines is expected to be small"?

« rephrased to 'smaller than the loading changes of erosion and sediment deposition itself, which is the topic of this study' p4 l 25 »

P 3, L 24: "The response of the Earth : : . is computed with the normal mode method (Vermeersen and Sabadini, 1997) which is benchmarked in Spada et al. (2011)." Which code? Most codes benchmarked in Spada et al. (2011) implement the normal mode. And is therefore the code an incompressible model?

« added 'multi-layer matrix propagation' on p5 l6 and 'incompressible' added p4 l28 »

P 4, L 17: ": : and only influences the effect of GISR through the distribution of meltwater possibly replaced by sediment, which is a small effect." What has a small effect? The melt water replaced by sediments? The fact that is small is not self-evident.

« changed to 'smaller than sediment loading itself' p 6 l 5 »

P 7, L 5: Is it so difficult to get Amantov data? And which is the original data accuracy?

« yes. The authors of that study have been helpful in explaining their method, but the model output described in the paper is proprietary. Amantov noted that it is not possible to put an accuracy on the model output. Our efforts therefore focussed on obtained two independent sets of GISR estimates. »

P 8 Figure 4: I don't think Figure 4 is really necessary. You can easily indicate in Fig. 5 the areas where the colour scale from Amantov picture is saturated. So you spare a picture and the issue of copy rights.

« figure 4 is removed »

P 8 Figure 5: This picture needs a colour scale bar.

C4

« added, this now figure B.2 »

P 10, Fig 7. RSL curves: the difference is not visible at all. The authors could make the red dashed. Since there's no visible difference this picture doesn't give more info than what you can tell in the text

« line is made dashed and the difference is also shown in what is now figure 4. »

P 11, L 8: "Thus, by interpreting the GPS rates as only resulting from ice unloading, the contribution from the ice is overestimated. This could result in biased inferences of ice thickness." This is one of the possible relevant effects and it should be (at least roughly) quantified. Is it a 1% overestimate or is it a 10%? However considering that most contribution of sediments to the uplift is in the sea and it is within the error, other source of error (such as the GPS reference frame error) could cancel it.

« We did not do the inversion for ice history, but numbers are given for the magnitude of the uplift rate in table 1. Discussion is added to 'discussion and conclusion' on the significance of the effect. »

P 11, L 19: "To evaluate the magnitude of the effect, Table 1 provides the maximum gravity rate that occurs in the areas that are used for GIA studies, Scandinavia and the Barents Sea" Why not showing this with a picture?

« figure 6 has been added. »

P 12, L 13: "Although the amount of sediment transported is LARGE, the effect is small compared to the ice loads that" Large compared to : : ? This sentence needs to be rephrased and this small effect should be explained (or discussed better). It's not clear if such small effect was expected or not. From the introduction I would guess that a larger effect was expected. So what is the main cause for a "large" amount of sediments to produce a "small effect"?

« 'large' is removed. Comparison with earlier studies is added further on in the discussion and conclusions »

C5

Minor comments P 1, L 29: "has been shown known" -> shown or known?

« done »

P 2, L 7: "2,5" -> "2.5"

« sentence removed »

P 2, L 12: "Models exist which couple : : ." does not sound like good English

« rephrased »

P 2, L 23: "The so-called sea-level equation is solved, which computes : : ." does not sound like good English

« the sentence does not appear to be problematic to us »

P 4, L 9 and Figure 1: "M8-128-150 and M4-16-80" are these M04 and M08 in the figure? The names in the legend are not fully self-explanatory.

« names added to the caption of figure 1 »

P 4, L 18: "modelv1.2" what is this? I understand that is a sort of update of the ICE-5G, is there a link or more detail to find that? What's the difference from the original?

« This is in fact the version of ICE-5G that was provided and widely used and referred to with Peltier (2004), but not always labelled correctly as v1.2. »

P 6, L 17: Why citing QGIS and MATLAB, GDAL, which are only tool that implement algorithm, and not instead citing the specific algorithms used?

« The contour drawing is done by hand. Converting shape to raster is a standard procedure for GIS software and not much value is added if the algorithms internal to the software are stated. »

P 9 Table 1: This table is not immediate to read. nn/nn/nn is visually not effective. Spaces could help, for example nn / nn / nn or even using 3 sub columns for each main

C6

column

« / is replaced by | with extra spaces. »

P 12, L 9: "Sediment transport from the Barents Sea to the west would have the opposite effect on the gravity rate but as of yet sediment transport is not yet detected in GRACE measurements." This sentence must be rephrased, I had to read it three times to understand it.

« rephrased p12 l18 »

P 12, L 13: "ice loads" -> "ice loads effect"..

« rephrased »

P 12, L16: "RSL data are not significantly affects" -> "RSL data are not significantly affected" and "because those data are located location near..." -> "because those data are located near..."

« done »

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/se-2017-18/se-2017-18-AC4-supplement.pdf>

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