Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-105-AC1, 2019

Interactive comment on "GRACE constraints on Earth rheology of the Barents Sea and Fennoscandia" *by* Marc Rovira-Navarro et al.

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Received and published: 3 October 2019

We thank the reviewer for the comments provided, we think they helped improving the quality of the manuscript and clarify some relevant points. Below, we address the reviewers' comments (in blue). A manuscript with the changes done to the manuscript shown in blue is also provided. References to pages and lines (page,L line) refer to the new version of manuscript, lines and pages mentioned in the reviewer's comments correspond to the first version of the manuscript

Reviewer 1: **Major comments**

Reviewer 1 express his concerns about some of the processing techniques used in this study. Below we address these comments:

My primary concern is about the substantial signal lost through the filtering and processing of the measurements and low resolution of the modeling. The limitation of maximum order number 60, which yields a minimum resolution of approximately 300 km, or one quarter of the linear extent of the Barents Sea. It is also cuts off a significant potion of the power of the authors' bandpass filter ranges. Thus the shape of the bandpass filter dominates the shape of the processed and modeled measurements.

The cut-off degree and filters used in GRACE data are equally applied to the simulated GIA signal (7, L22). This way the comparison of GRACE and simulated gravity rates is consistent

The effect of cutting the GIA signal at degree 60 is illustrated in Figure 1 and 2. Figure 1 shows the GIA signal obtained with the ICE-5G model cut at different degrees, Figure 2 gives the maximum gravity disturbance rate obtained in the Barents Sea for different cut-off degrees. Both plots evidence that the GIA signal does not have a high content of high degree harmonics and therefore little signal is lost.

The filters used to process the data are carefully chosen following the work of Root et al. 2015 (see supplementary data). A low pass filter is needed to filter out small wavelength noise. We vary the filter halfwidth between 200km and 300km. Below 200km noise becomes dominant and above 300km the positive signal located in the Barents Sea is very small (see Figure 3). The half-width of the high-pass filter is chosen using Figure 1 from Root et al. 2015 supplementary material. They use a synthetic GIA signal to show the effect of using a high-pass filter. From that figure it is concluded that a high pass filter between 500 and 700 km keeps the GIA signal while removing other long-wave signals.

This filtering occurs after a series of processing steps to extract the LGM signal. The GRACE measurements are processed one way to estimate the current mass loss off the archipelagos, another to estimate the ocean signal, and a final way to estimate the response to LGM deglaciation in the Barents. So, while I really appreciate the attempt to quantify all of the sources of error, the assumption that they are uncorrelated (page 5, line 12) requires further explanation. I would similarly like elaboration of the effect the GIA model chosen has on the estimate of mass loss (page 4, line 33).

It is true that GRACE is used to (1) recover the LGM signal, (2) obtain the mass loss changes in the islands of the Arctic archipelago and (3) in a smaller degree in the ECCO model which makes it possible for the errors to be correlated.

We start by addressing the use of GRACE to estimate mass changes in the Arctic Archipelago. The "circularity" of the problem is explicitly mentioned in page 4, L32. We use an ensemble of ice sheet models and solid Earth rheologies to estimate the uncertainty in mass changes. Table 1 now gives the different combinations of ice-sheet and rheology models used to correct GRACE estimated mass changes. Four different ice sheet models and three different rheologies are used.

The ice sheet models correspond to two runs of the Glacial System Model for Northern Europe, the ICE-5G and the W12 models. As for the rheology we use the VM5a model and two models with an upper and lower mantle viscosities of $16 \cdot 10^{20}$ and $512 \cdot 10^{20}$ and $10 \cdot 10^{20}$ Pas and $100 \cdot 10^{20}$ Pas. We see that a weaker mantle leads to higher mass loss rates for the islands of the Artic Archipelago. We add this table as well as an explanation on the effect of the GIA model on the mass loss estimations in the tex.

GRACE data is not used in the creation of the OMCT ocean model but it is used in the ECCO ocean model. However, this is only 1 of the 40 data sets that are used to constrain the dynamic MITgcm ocean model and as shown in ECCO's documentationGRACE is one of the worst fitted observations. This fact is also evident in the results of Yu et al. 2018 who compare GRACE derived bottom pressure anomalies to the ECCO ocean model for the Argentine Gyre. We do not use the ECCO ocean model in our estimate itself, but only as a proxy of how much error we might expect from uncertainty in the ocean signal. Given the weak contribution of GRACE to the final output we think this is appropriate.

The correlation of GRACE's measurement error and that from ice loss estimates cannot be ruled out. The error in ice loss estimates have two components (1) GRACE's accuracy error and (2) error due to uncertainty in the GIA model. While the second is not correlated with GRACE's measurement error, it is true that the first can be. In light of this discussion we decide to add the following discussion after equation (1).

"The assumption that errors are uncorrelated requires further discussion. GRACE data is assimilated in the ECCO ocean model. However, GRACE is only one of the 40 data sets used in the inversion process and the final product does not fit GRACE data well (Yu et al. 2018). Therefore there will be only a weak correlation with the GRACE data used in our estimation. Correlation between land surface hydrology models and present-day ice melt is not expected, because hydrology models have little skill in predicting trends and do not model areas of permanent snow. Finally, ice loss changes errors (σ_{ice}) arise due to uncertainty in the GIA model and GRACE measurement error, we cannot rule out that the second error component might be correlated with σ_{GRACE}. "

In light of this concern, I would ask the authors to: 1) further quantify the effects of their processing technique for this area. In particular, by adding more discussion of the technique for idealized measurements in the context of the Barents; and 2) consider acknowledging the processed nature of these results by referring to them as "estimated gravity rates" rather than "observed gravity rates." I feel this is particularly important when the authors substitute the phrase "observed gravity rate" for the estimated maximum gravity rate (e.g., page 6 - line 31).

The processing techniques were detailed in the supplementary material of Root et al. (2015), their effect for an idealised measurement in the Barents Sea is shown there. Following the suggestions of the reviewer we: (1) Refer to the Supplementary material of Root et al. 2015 for a detailed explanation of how our processing affects an idealised signal (4,L5). (2) Include a Table showing the estimated mass loss changes obtained using different GIA models. (3) Use the term estimated gravity rates instead of observed (8,L14).

The argument additionally suffers from another small, but troubling, circularity. The ensemble of ice models was chosen to represent two classes: empirical ice sheets developed using GIA observables and ice sheets developed from independent, process based models. However, all of the models are actually calibrated, in some way or another, to GIA observables with an implicit dependence on the assumed viscosity structure. For instance, the Tarasov samples are drawn from a distribution trained on GIA observables using the Peltier VM5a rheology. If the authors could comment on this bias and how that might account for the reference model being very near the best fit valley in all figures but the Siegert and Dowdeswell 2004 model, which is the only one to prefer an anomalously high viscosity, most likely because of it's earlier ice-free time.

Although the problem is certainly there, the S04, but also the UiT model are not fitted to GIA observations. In the main text we distinguish between two different types of ice models, (1) those that do not include ice sheet physics (ICE-5G, ICE-6G) and are entirely based on GIA observations and (2) those that incorporate ice sheet physics. However, as pointed out some of the ice models in the second subset do also include GIA observations. Tarasov's models are calibrated using the fit to RSL curves and uplift rates obtained with the VM5a model, however the calibration accounts for spread in the decay times due to uncertainty in the viscosity model and is not tuned to a single viscosity model as much as ICE-xG models are (L. Tarasov personal communication). For the UiT model a simple hydrostatic model is used to account for ice-elevation feedbacks, but the model fit to GIA observables is assessed a-posteriori (Patton et al. 2016). We include this additional information extending our description of the ice models in Section 2.2.

Minor Comments

page-line 4-33: "However, the GIA" It is not obvious that this should be so.

We tried to clarify this point by adding the individual mass loss estimates for each of GIA model in Table 1. Moreover, we clarify this point by showing that the uncertainty due to the GIA model used to recover the mass changes is of the same order of magnitude as GRACE's formal error (5,L7).

8-19: citing the $\chi 2$ might make this point clearer. It is hard to tell that S04 is significantly worse than, say UiT, from figure 3.

We state that the model performs worse than the T1, T2 and T3 (which is clear from figure 3). Later on this is evidenced with the χ^2. We also add some new discussion on the fit of the S04 model for different Earth models in Section 3.2 (9,L32).

10-32 might include "explicitly" in "not explicitly tied to a viscosity model"

We follow the suggestion

Figure 5 and Figure 6 - Could you note with a symbol the reference model and the best fit model in each of these plots?

For each lithospheric thickness we indicate the best fitting model with a red line and the reference model with a red dot.

1-2: in-> to "insight to the" Done

1-4: Split sentence Done

1-5 remove "a" in "a GIA models" Done

1-6 "is not negligible" and "should be taken into account" are redundant Done

1-16 Inconsistent use of "gravity disturbance rate" and "gravity rate" We use gravity disturbance rates until it is stated that the term gravity rates will be used instead in 4-L9.

3-7 missing word in "while best fitting models uplift rate measurements" Done

3-22 missing "and" in "GIA, and (post-) seismic" Done

5-4 GAB undefined GAB is not an acronym. We add a reference to Flechtner et al. 2015 where the GAB files are defined.

5-9 “respectively” has no antecedent. Consider “both the OMCT and ECCO ocean models” We follow the suggestion

5-15 “while when” is difficult to parse We rephrase the sentence.

5-22 missing word in “This still allows” We rephrase accordingly.

5-23 correct citation parenthesis Done

5-25 missing “the” in “that of the unknown” Changed

5-26 remove nested parentheses Done

5-31 missing “the” in “the Earth’s rheology” Added

8-12 I believe deglaciation starts earlier in T2 than in T1, unless I am much mistaken. That is true, we correct the misspelling.

8-31,8-32,9-30 “lower upper mantle viscosity” is pretty cumbersome to read. Consider something like “less viscous upper mantle” We follow the suggestion.

9-9 repeated word “which that” Error corrected

9-12 large->high “high upper mantle viscosity” We follow the suggestion.

9-16 typo“form” Error corrected

Figure 3 and Figure 4 - inconsistent x-axis label We modify the label to ensure consistency.

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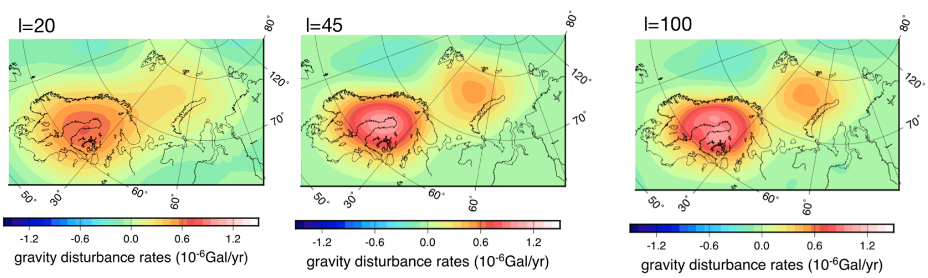

Fig. 1. Simulated GIA signal cut at different degrees

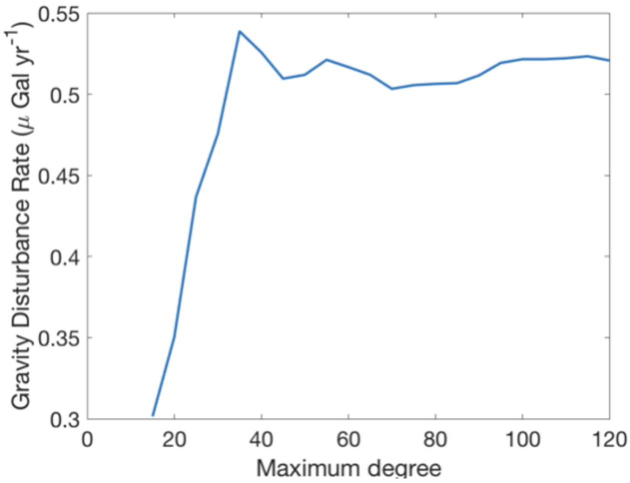

Fig. 2. Maximum gravity disturbance rate as function of cut-off degree