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

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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. Figures in this response are referenced as R.

Major comments

1. My main concern is the lack of discussion of uncertainties in the resulting viscosities.

The conclusion that Fennoscandian upper mantle viscosity is a factor of 2 higher than that in the Barents Sea is given with very little discussion on uncertainties:

(a) Most studies would state resulting viscosities and elastic thickness as an interval determined one way or another from the statistics of the inversion process. A differently normalized chiËĘ2 range or a variance reduction, for example. On page 8, line 27, a 2 sigma interval is mentioned but not further referred to. The very different chiËĘ2 distributions for Barents Sea and Fennoscandia in Figures 5 and 6 make it difficult to asses which parts of the model space is appropriate to compare to one another. In addition, the color scale in the Figures does not enhance the well fitting regions very well, I suggest a scale with a better visible range.

We modify Figure 5 and 6 to give idea of the range of models that perform well. We define a confidence interval following Press et al. 1992 (Chapter 15) (8,L4). We indicate the values within the 95% confidence interval in grey. Additionally, to ease comparison, we indicate in red the best fitting model for each lithospheric thickness which eases the discussion in Section 3.2.

b) At least for the T1-T3 and S04 ice histories, even though the well fitting viscosity range starts at lower viscosities for the Barents Sea than for Fennoscandia, there is significant overlap at higher viscosity in Figure 5. This is less pronounced for ICE-XG and UiT in Figure 6, but is there at thicker elastic thickness. A more well defined range of which models are considered good fits would ease the comparison.

We agree there is an overlap between both regions. We chose to compare the best fitting upper mantle viscosity obtained for each lithospheric thickness in both regions (indicated in red in Figures 5 and 6) to illustrate the difference in upper mantle viscosity and elaborate more on the discussion (11,L11):

"We can infer lateral rheology changes by comparing the optimal Earth rheological parameters obtained for both regions. For each ice deglaciation chronology, we compare the two confidence intervals as well as the best fitting upper-mantle viscosity obtained for each lithospheric thickness. We observe that for the UiT, ICE-5G and ICE-6G model both the confidence interval as well as the best fitting models have a systematically higher upper upper mantle viscosity in the Barents Sea as compared with Fennoscandia. This is also the case for the T1,T2 and T3 models when the best fitting models are compared, although there is an overlap of models of high upper mantle viscosity and thick lithospheres with a good fit in both regions. This systematic difference is likely evidence of lateral variation in Earth rheology. "

c) The lower bounds on viscosity is very similar for all ice models in the two regions. That is a little odd. Is there some bias somewhere? Such as they having similar Earth models during construction?

The lower bounds obtained for Fennoscandia and the Barents Sea are quite similar. This can be understood by studying the sensitivity of gravity disturbance rates to mass changes during different epochs (Figure R1). As upper mantle viscosity decreases the results are less sensitive to mass changes in the past where differences between ice models are more acute (i.e., Figure 1), making it more difficult to distinguish between ice sheet models. This agree with previous studies that have used gravity data to constrain solid Earth parameters. For instance, Steffen et al. 2010 finds similar lower bounds for upper mantle viscosity using the ICE-5G and the RSES models.

To illustrate this point we add the Figure R3 and add an explain along these lines in section 3.1 (10,L10). "The lower bound obtained with the other ice models is similar as models with a low viscosity have little sensitivity to mass changes during the early deglaciation phase, where differences between ice models are more manifest (Figure 2)"

d) Elastic thickness is discussed very briefly in the manuscript. The clear correlations in Figures 5 and 6 between viscosity and elastic thickness should be discussed further. This is different to the results in e.g. Steffen et al. (2010), Root et al. (2015a,2015b). How much are the results for a thicker elastic layer affected by the GRACE filtering pro-

cess? In addition, there are surely estimates from seismology of the (seismic) thickness of the lithosphere in the Barents and in Fennoscandia. These could perhaps also be used for comparison purposes, even though the measure a slightly different property.

The effect of lithospheric thickness is more evident than in other studies of GIA of the region. We compute the χ^2 using the maximum gravity disturbance rates instead of averaging over a region as the shape of the signal in the Barents Sea is difficult to distinguish, for consistency we use the same strategy for Fennoscandia (Figure R2). A thinner lithosphere results in higher gravity rates and can counterbalance a low value of upper mantle viscosity. However, they also lead to narrow region of uplift which does not fit the signal well in Scandinavia. The effect of lithospheric thickness is also evident in Root et al. 2015a. An explanation along these lines is given in (10,L5)

We do not want to draw conclusions on the absolute values for the lithosphere thickness from our misfit plots, because we do not use the spatial pattern of the gravity rate (added in Section 3.1 10,L11) and this is why we compare best fitting upper mantle viscosity for each lithospheric thickness.

e) When concluding the factor of 2 viscosity difference between Barents Sea and Fennoscandia you should specify at which elastic thickness the comparison is made. If you use different elastic thickness for the different regions that should be explicit. Similarly for the seismic estimates of viscosity difference. These are at the same depth for the Barents Sea and Fennoscandia but to make a fair comparison it would be interesting with estimates of the seismic lithosphere thickness. How much does it matter for the comparison if there are differences in temperature and/or composition in the two regions?

As pointed out at b) the comparison is made for the best fitting upper mantle viscosity obtained for each lithospheric thickness. We add this explanation to the main text.

Differences in temperature are considered in the paper. With regard to composition, we now state that changes in composition are not considered but likely do not play as large a role as temperature, in the upper mantle (11,L28-30).

f) You should compare your inferred viscosity differences to other GIA studies of the Barents Sea area and Fennoscandia. The large number of varying results for Fennoscandia indicate that such a comparison is non-trivial. For the Barents Sea, Root et al. (2015a) indicates $4 \cdot 10^{20}$ Pa s for the Barents and Auriac et al. (2016) has a very wide range of $2 - 20 \cdot 10^{20}$ Pa s.

We try to put our results in context by comparing them to the results of Auriac et al. (2016) which found a similar bound for upper mantle viscosity in the Barents Sea. For Fennoscandia we refer to the recent work of Simon et al. 2018 that give an overview of different rheology estimates.

Moreover, we are also more cautious in our claim about upper mantle viscosity constrain as other GRACE studies (Root et al. 2015b, Steffen et al. 2014) show that the effect of lower mantle viscosity is not negligible and obtain optimal fits for higher lower mantle viscosities than the one used in this study (6,L21). We add this discussion in section 3.2 and stress this fact in the conclusions.

2. The four error estimates for the GRACE processing seem very appropriate, but I wonder: (a) Is spherical harmonic degree 60 really enough for this study? And the filtering out of smaller wave-lengths seems to retain only very large scale features, on the order of the whole basin

See response given to the first comment of Reviewer 1.

b) The statement on page 5, line 12, about the independence of the estimates. It seems to me a little strange that the hydrological signal would be uncorrelated with the ice loss signal? Ice mass loss usually means melting, which surely influences the hydrology, both in time and magnitude. Is this not an issue?

See response given to the first comment of Reviewer 1. Moreover, land surface hydrology models have little skill in predicting trends and they do not contain permanent

snow or glacier therefore not much correlation with ice melt is expected. We add this explanation below equation 1.

c) The estimate of ice mass loss from GRACE data does indeed seem circular, and a little difficult. A GIA model using GRACE data is used to estimate uncertainties in the GRACE data for GIA applications? On page 4, lines 21-22 the authors state that the current ice mass changes "...partly mask the GIA signal...", but on lines 33-34 that "... the GIA model used to obtain the mass changes has a small effect in the recovered gravity rate trend...". This seems contradictory to me and need more detailed explanation. Do you use different GRACE filters here to capture the spatially smaller current deglaciations? Also, how are the error bars estimated from the range of ice and Earth models? Do you have a range of reasonable chiËĘ2 or something for this error estimate?

We add the mass loss changes obtained using the different GIA models in Table 1 as well as some more details about rheology and ice sheet model affects the results (see response Reviewer 1). What we mean with "... the GIA model used to obtain the mass changes has a small effect in the recovered gravity rate trend.." is that the error introduced by using different GIA is similar as that given by GRACE measurement error (as shown in Table R1). We agree that this statement is confusing and thus we change it by (5,L7): " The error in the derived mass changes due to uncertainty in GIA is similar to GRACE's measurement error".

Minor comments

Including GPS data from Svalbard and northern Norway would have been helpful to constrain the models. Why was this not done?

The focus of this work was on what can gravity tell us about interior structure in this two regions. We agree that adding GPS and even RSL might help to constrain the models but we decided to focus on the gravity signal because the gravity rate is available in the center of the Barents Sea in the region of largest ice thickness where other measurements are absent.

Previous work (e.g., Auriac et al. 2016) have focused on GPS and RSL. To recognise the fact that we don't use these other data sets we add :

"We find that the ICE-5G, ICE-6G and UiT ice sheet models can be reconciled with GRACE observations provided the upper mantle viscosity is lower or the lithosphere thicker than in the VM2 model. The same conclusion is reached in Auriac et al. 2016 using GPS uplift measurements and RSL curves instead of gravity data."

It would be good to have a little bit more information on the ice reconstructions, especially with regard to the used Earth model physics for the non-GIA derived ice models. Do they have appropriate viscoelastic earths, or just simple hydrostatic adjustment, or...? Also, which time period do you use in the models? Just the deglaciation phase? If so, how are the ice sheets ramped up to the last glacial maximum?

We add more information about the different ice chronologies (see Section 2.2). We include both the build-up and deglaciation phase. We add (7,L21): "The only global ice sheet models are the ICE-5G and ICE-6G, for the other ice sheet models we use the ICE-6G ice model outside the EISC. We include the build-up and deglaciation phase of the last glacial cycle". Moreover, we add a new plot showing the deglaciation history in Fennoscandia (see Figure 2), and specify when the ice sheets start to build-up. We also include some information on how isostasy is implemented in the UiT model and S04 models.

Use regular non-italic font for units. Even in latex "Pa.s" can be made roman in math mode

We revise the text and make sure that Pa.s is non-italic.

1 3 You write insight into sub-surface structure. It is not really structure but rather rheology.

We follow the suggestion

2 30 Same as above

We follow the suggestion

1 4 Either spell out GRACE, or add "gravity" for clarity.

We follow the suggestion.

1 8 I would remove "deglaciation" here and describe the used time period in the paper.

For clarification we add "of the last glacial cycle" for clarification

1 16 Just to be clear, spell out GRACE or add gravity here the first time it is mentioned.

We follow the suggestion.

2 7 Here you could include the dynamic ice sheet model by Näslund et al. (2005): Näslund, J.-O., Jansson, P., Fastook, J. L., Johnson, J., and Andersson, L.: Detailed spatially distributed geothermal heat-flow data for modeling of basal temperatures and meltwater production beneath the Fennoscandian ice sheet, Ann. Glaciol., edited by: MacAyeal, D. R., International Glaciological Society, 40, 95– 101, doi:10.3189/172756405781813582, 2005.

We thank the reviewer for the suggestion but consider that the ice sheet models used in the study are enough to capture uncertainty in ice deglaciation chronology for the region.

3 7-8 "... best fitting models uplift rate measurements..." is difficult to understand. 3 29 "... we use ..." the software? There is an object missing in the sentence.

Indeed, we modify the sentence: "while best fitting models based on GPS uplift rate measurements have upper mantle viscosities"

4 6 Define "gravity disturbance rate" as opposed to "gravity anomaly rate".

Both terms are commonly used in physical geodesy, we add a reference to Hofmann-Wellenhof and Moritz physical geodesy book for clarification.

5 3 Reference to the ECCO model.

We add the reference to Forget et al. 2015 here.

5 4 What are the GAB products?

GAB files contain the ocean signal subtracted from GRACE and should be added back to restore GRACE's full ocean mass mass variations. We add a reference to Flechtner et al. 2015 for the GAB products.

5 8 No italics.

We follow the suggestion.

5 17 Ocean bottom pressure changes in the Baltic can be neglected? Are they so much smaller than in the Barents, or just relatively smaller?

Both, they are smaller in the Baltic Sea and the signal there is higher (see Figure R3), so we decided to not consider the error.

6 28-29 This sentence need reformulation.

We reformulate the sentence.

7 1 Are you using central Fennoscandia? If so, where is this?

We do not use a specific point in Fennoscandia but find the point with maximum rate.

7 9 In Figure 1 it is the gravity signal after processing, not necessarily the GIA signal.

The reviewer makes a valid point here. We reformulate the paragraph that now reads: "A clear positive anomaly is evident both in Fennoscandia and the central Barents Sea where the main domes of the Scandinavian Ice Sheet and Svalbard-Barents-Kara Ice Sheet were presumably located (Figure 1), we assume this signal to be entirely due to

GIA and call it the estimated GIA signal"

9 5 "A second set..." Which is the first?

We remove the $32 \cdot 10^{20}$ upper mantle viscosity from our plot as it corresponds to a higher viscosity than that used for the lower mantle. Doing so the second subset is less evident and thus we decide to remove this paragraph.

9 9 ".. gravity rate which that is larger than..." Fix this.

Done

9 20 The authors should point out that 3D effects are indeed significant, e.g. Whitehouse et al. (2006), Steffen et al. (2006).

This is a good point. We modify the text accordingly which now reads. We decide to mention this in the method's section (6,L10-12)

" While this approach has been used in other GIA studies (Lambeck et al., 1998; Steffen et al., 2014), it has been suggested that far-field viscosity variations are relevant in Fennoscandia (Whitehouse et al. 2006)."

10 2 Which conclusion?

We add "of lateral viscosity changes between the two regions"

10 9 "...the reference model... a jump below 200 km" Please clarify which reference model and what the jump is, or refers to.

Changed to "a jump in the seismic velocity anomalies in the reference Earth models PREM and AK135

10 14 Stress for the flow law is taken from the GIA model. How accurate is this? Neglecting tectonics, topography, sediment loads etc surely distorts the "correct" stress state. How important is this?

Deviatoric stresses in the mantle from topography is small because of the long time-scale. Stresses from sediments are small as current uplift rates are small (van der Wal and Ijpelaar 2017). We add to the text (11,L33) “Background stresses due to mantle convection are neglected as recent work suggest little interaction between GIA and mantle convection ”

10 23 Why did you choose the 1500 m contour?

We choose this contour to encompass the Scandinavian landmass and the Barents Sea, but avoid areas where the ice thickness was thin. A sentence is now added on page Section 3.3 (12,L9)

10 31 You should define "significant", or rather add uncertainties.

We delete “significant”.

12 1 "... the GRACE misfit"? The GRACE GIA models?

We change the sentence which now reads as: “This agrees very well with the results derived from the misfit of GIA models to GRACE data.

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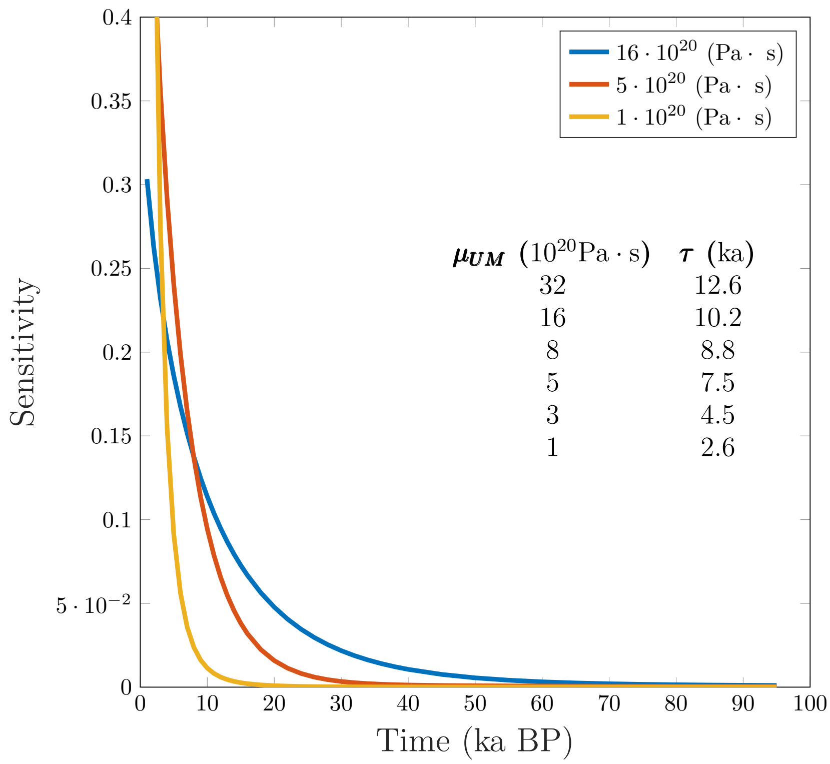

Fig. 1. Gravity disturbance rates sensitivity to mass changes at a given epoch for different upper mantle viscosities. Relaxation times for different upper mantle viscosities is also given.

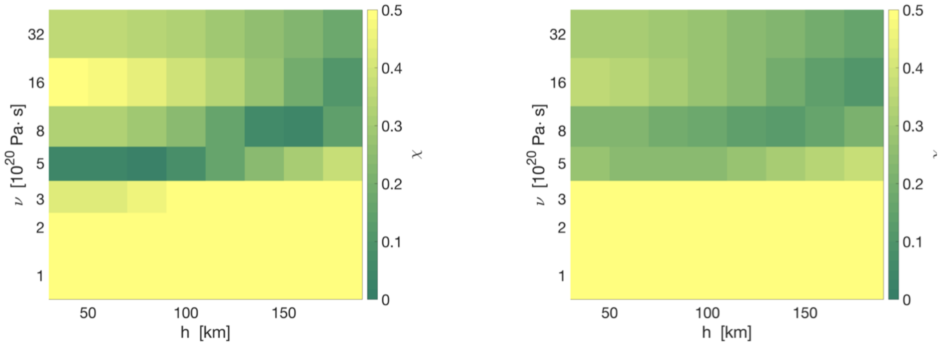

Fig. 2. Normalised \chi obtained using the maximum gravity rate (left) and averaging the \chi of points in Fennoscandia.

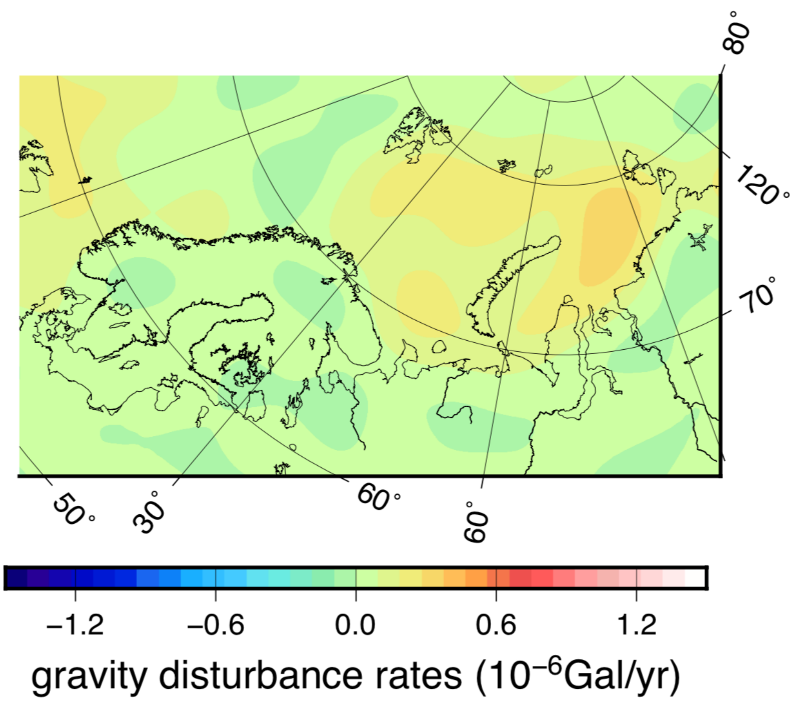

Fig. 3. Gravity disturbance rates due to ocean bottom pressure (from OMCT ocean model)