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

Anonymous Referee #2

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The manuscript by Rovira-Navarro et al., "GRACE constraints on Earth rheology of the Barents Sea and Fennoscandia" analyzes GRACE gravity data in order to constrain glacial isostatic adjustment (GIA) models in the two regions. They employ a number of different reconstructions of the Weichselian ice sheet and explore the effects of varying upper mantle viscosities and elastic thickness. The manuscript concludes a lower bound on the upper mantle viscosity of the the Barents Sea region and that that viscosity is a factor of 2 lower than the viscosity of the Fennoscandian upper mantle.

I find the manuscript well written and easy to follow. The application of GRACE data to an oceanic region like the Barents is important in order to gain a better understanding of the ongoing geodynamics and the extended uncertainty analysis in the manuscript seems like an appropriate way to go. I do find the manuscript a little brief on some

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points which I detail below.

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 chiˆ2 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 chiˆ2 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.

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.

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?

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 process? 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.

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?

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 4x10ˆ20 Pa s for the Barents and Auriac et al. (2016) has a very wide range of 2 - 20 x 10ˆ20 Pa s.

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?

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?

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

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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 chi^2 or something for this error estimate?

Detailed comments: - Including GPS data from Svalbard and northern Norway would have been helpful to constrain the models. Why was this not done? (See Auriac et al. (2016)) - 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? - Use regular non-italic font for units. Even in latex "Pa.s" can be made roman in math mode (if that is the problem)

Page Line Comment 1 3 You write insight into sub-surface structure. It is not really structure but rather rheology. 2 30 Same as above 1 4 Either spell out GRACE, or add "gravity" for clarity. 1 8 I would remove "deglaciation" here and describe the used time period in the paper. 1 16 Just to be clear, spell out GRACE or add gravity here the first time it is mentioned. 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. 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. 4 6 Define "gravity disturbance rate" as opposed to "gravity anomaly rate". 5 3 Reference to the ECCO model. 5 4 What are the GAB products? 5 8 No italics. 5 17 Ocean bottom pressure changes in the Baltic can be

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neglected? Are they so much smaller than in the Barents, or just relatively smaller? 6 28-29 This sentence need reformulation. 7 1 Are you using central Fennoscandia? If so, where is this? 7 9 In Figure 1 it is the gravity signal after processing, not necessarily the GIA signal. 9 5 "A second set..." Which is the first? 9 9 ".. gravity rate which that is larger than..." Fix this. 9 20 The authors should point out that 3D effects are indeed significant, e.g. Whitehouse et al. (2006), Steffen et al. (2006). 10 2 Which conclusion? 10 9 "...the reference model... a jump below 200 km" Please clarify which reference model and what the jump is, or refers to. 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? 10 23 Why did you choose the 1500 m contour? 10 31 You should define "significant", or rather add uncertainties. 12 1 "... the GRACE misfit"? The GRACE GIA models?

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