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Solid Earth
Discussions

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

Anonymous Referee #1

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"GRACE constraints. . ." by Rovira-Novarro, et al., investigates the constraints provided by GRACE measurements on upper mantle Earth rheology in the Barents Sea. Using a sample of deglaciation chronologies and a grid-search of upper mantle viscosity and lithospheric thickness, they identify a lower bound on viscosity and evidence of lateral heterogeneity moving from the Barents Sea to Fennoscandia. The investigation is a follow-on to the paper Root, et al. 2015a, contributing the parameter search in earth response modeling, additional ocean modeling, and a discussion of lateral heterogeneities. It is an important step in quantifying the uncertainties in rheology associated with matching the changing gravity field in the Barents Sea. However, I am concerned that the result does not accurately represent the state of knowledge. As I describe below, the result seems to rely on aggressive processing and a limited, pos-

sibly biased, range of ice models. With these issues addressed, I would look forward to seeing this article in press.

General 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.

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).

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 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.

Specific comments page-line 4-33: "However, the GIA" It is not obvious that this should be so. 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 10-32 might include "explicitly" in "not explicitly tied to a viscosity model" Figure 5 and Figure 6 - Could you note with a symbol the reference model and the best fit model in each of these plots?

Technical corrections page-line 1-2: in-> to "insight to the" 1-4: Split sentence 1-5 remove "a" in "a GIA models" 1-6 "is not negligible" and "should be taken into account" are redundant 1-16 Inconsistent use of "gravity disturbance rate" and "gravity rate" 3-7 missing word in "while best fitting models uplift rate measurements" 3-22 missing "and" in "GIA, and (post-) seismic" 5-4 GAB undefined 5-9 "respectively" has no antecedent. Consider "both the OMCT and ECCO ocean models" 5-15 "while when" is difficult to parse 5-22 missing word in "This still allows" 5-23 correct citation parenthesis 5-25 missing "the" in "that of the unknown" 5-26 remove nested parentheses 5-31 missing "the" in "the Earth's rheology" 8-12 I believe deglaciation starts earlier in T2 than in T1, unless I am much mistaken. 8-31,8-32,9-30 "lower upper mantle viscosity" is pretty cumbersome to read. Consider something like "less viscous upper mantle" 9-9 repeated word "which that" 9-12 large->high "high upper mantle viscosity" 9-16 typo "form"

Figure 3 and Figure 4 - inconsistent x-axis label

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