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The Cryosphere Discussions

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Interactive comment on "Quantifying the resolution level where the GRACE satellites can separate Greenland's glacial mass balance from surface mass balance" *by* J. A. Bonin and D. P. Chambers

Anonymous Referee #2

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The aim of this paper is to study the feasibility of recovering glacial mass balance from satellite gravimetry. Based on a suite of simulations, they find that this requires spherical harmonic solutions up to at least degree/order 90 with a noise reduction of a factor of 9 compared to current GRACE data. The paper is generally well written, and methods and techniques appear to be robust. Although one may argue that this is rather a geodesy than cryosphere paper, I believe this paper deserves to be published. However, there are a number of issues that need to be resolved prior to publication:

* The authors distinguish between glacial mass balance (GMB) and surface mass bal-

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ance (SMB). I'm not sure if glacial mass balance is the right word to use here, this is usually used to describe the total mass balance of a glacier (dynamic + SMB). Dynamic mass balance (DMB) might be a better choice.

* This brings me with the only major comment I have: the authors state that they aim to separate the two mass balance components. In reality, what this study shows is what data resolution/quality is required to isolate the coastal parts of glacier basins from the surrounding area. Even if these resolution/quality requirements are met and a GRACE-like mission would be able to retrieve mass changes in the 1x1 degrees grid cells focused on the glaciers (as in this study), the resulting signal would still be a combination of SMB and dynamic changes. Likewise, the mass changes in the interior basins would also be a combination of SMB and dynamics (although here SMB would be dominant). It is true that for the three glaciers in this study, the dynamic component dominates, but this isn't necessarily the case for other glaciers (and not even always for these three big glaciers, see the near-zero slope for Kangerdlugssuaq for 2002-2006 in fig. 5). Suppose a glacier shows no dynamical imbalance, but the cumulative SMB near the grounding line shows a trend. In that case, the authors would incorrectly attribute the observed changes to glacier dynamics. The parts where the 'separation of SMB and GMB' is mentioned should be rephrased to correct this.

* section 3.1: The authors designed a rather involved method to generate random SMB maps. I'm wondering if it wouldn't be an option from an independent model, such as MAR (freely available on the internet), to estimate the effect of SMB variability. Considering that you're only using data from 2002-2012, you could even consider to use RACMO data from different time intervals (although a scaling trend might be required). This would arguably produce more realistic maps (the current 3 degree-smoothing approach doesn't take into account that the SMB length scales are variable across the ice sheet) and would be much easier to understand for the reader.

* only six random simulations are used in the current set-up. This seems to be too low to base any robust conclusions on. The computational costs of the simulations appears

to be low, so I would suggest to increase the number of simulations.

technical comments:

page 1316, line 3: remove 'precipitation-based'. As you mention later on, SMB consists of much more than just precipitation.

page 1318, line 11: Here you say that you don't use any constraints or regularization. But wasn't the conclusion of Bonin and Chambers that regularization is required to stabilize the solutions (see eq. 4 in Bonin 2013)? Please include a discussion of why you chose not to use regularization.

page 1318, line 25: - RACMO2 does not include ice dynamics, it's a regional climate model - What time period did you use to compute the RMS, the GRACE period? - I assume you use cumulative SMB anomalies (which is what GRACE would measure), not the monthly SMB as output by the model? The same applies for the GMB signals, as in fig. 5.

page 1319, line 22: I don't understand what you mean by 'the external signal is kept constant'. Did you represent ocean and hydrology using a random month from the models and kept this constant? Ocean and hydrology loading vary in time, and so does the effect on the mass inversion. If you pick a random month, you may under-/overestimate the effect (same applies to using the average, which will underestimate the effect). Using time-varying model data appears to be more correct.

page 1320, line 16-17: How did you compute these numbers (83 and 95%). Did you use cumulative SMB anomalies (running sum of monthly SMB minus long-term mean SMB) or just cumulative SMB? The numbers seem high, I suspect that the numbers would be lower for cumulative SMB anomalies.

page 1321, formula 1: I suggest to use a different symbol for rˆ(3 deg). When I first glanced at the formula, I assumed this means a local 3 degree smoothing radius.

page 1322, line 10: what is the beta-value (.85) based on. This actual value will vary

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from location to location. Instead of using a fixed value, it would make more sense to estimate the local value from the RACMO data, using the lag-1 autocorrelation value.

page 1322, line 18: why only 6 simulations? Seems a bit low to base any convincing

page 1329: square root missing in denominator?

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