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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

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Thank you for your helpful review. As I understand it, your main concern about our paper is that, even assuming ideal, noiseless data, the method may not really separate SMB from dynamic effects. We understand your concern and appreciate you pointing out this confusion to us. To clarify our motivation and also to better explain exactly what we're doing, we have added the following paragraph in the introduction section:

"We expand this technique to include regions designed to contain the mass signal

of the largest of Greenland's glaciers: Kangerdlugssuaq, Helheim, and Jakobshavn. These glacial regions experience two different physical processes atop each other: the localized DMB signal and the broader-scale SMB signal. Unlike most places in Greenland, the DMB signals in Kangerdlugssuaq, Helheim, and Jakobshavn glaciers are expected to be larger than the local SMB signal. That fact allows us to potentially separate the dynamical effects from the SMB effects in these regions, by making a pair of assumptions. First, since SMB is correlated over fairly large regions, we assume that the SMB signal across each of the large glaciers is similar to the SMB just outside the glacier. Second, we assume that any local signal which is not defined by the broader SMB signal is caused by glacial dynamics. The latter is a reasonable assumption in the case of these three glaciers, due to the relatively large size of the expected DMB signal compared to discrepancies in local SMB relative to nearby SMB. This allows us to use two overlapping basins to separate the two independent signals: first, a large SMB basin, similar to those used in previous studies, and second, a small basin covering only the area just around the glacier. The smaller basin is designed to trap the localized signal, which we know to be largely caused by the DMB, while the larger basin will trap the underlying larger-scale signal, which we know to be largely caused by the SMB."

As to your specific comments:

1.) The authors distinguish between glacial mass balance (GMB) and surface mass balance (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

We have made this alteration.

2.) 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. Con-

sidering 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 ... and would be much easier to understand for the reader.

We did originally try using the difference with the 1993-2003 RACMO2 data as a way to measure errors. But the trends and long-term effects are really what we want to measure most, and those change so much between the 1990s and the 2000s that we found it was really not an appropriate of errors. That was what led us originally to creating a randomized version of the slopes and climatology, and the residual followed naturally from there. You are right that we could have (and possibly should have) used the MAR model instead. However, when we started, we frankly did not know about it. Even now, we have no feel for how many correlated errors there would be between MAR and RACMO2, which subtracting the two series would not reveal. Given how large the GRACE errors prove to be relative to any possible errors in the model, we decided not to download and learn to use a totally new (to us) data set, since the general results (that GRACE errors are too large to separate SMB from DMB) would surely not change. I hope that is acceptable. We have downloaded the MAR dataset for similar use in the future, so we thank you for the suggestion.

3.) The current 3 degree-smoothing approach doesn't take into account that the SMB length scales are variable across the ice sheet

You are correct. To fix this, we have used the RACMO2 data to create a map of localized length scales. As you expected, they vary significantly over the area, from low near the coasts to +/-5 degrees in central Greenland. We have altered our code to use these variable length scales, rather than a single 3-degree one.

4.) 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.

We chose a small number of simulations because of the time commitment required. The time needed to run these cases is not long for the 60x60 cases, but gets very lengthy for 120x120 and 180x180, since it increases as maxdeg^2. (For the record, the creation of 250 SMB and 250 GMB simulations at 5 maximum spherical harmonic degrees is still continuing as I type this seven days later. It's not hard to do, just time-consuming.) However, since all of the reviewers felt this was a major concern, we are currently recreating the cases to have 50 simulated runs.

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

Done.

6.) 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.

We have done so, briefly: "Although in Bonin and Chambers [2013] we determined that a diagonal constraint matrix assisted in the optimization, experimentation since has demonstrated that when using non-uniform basin weights, such "process noise" does not improve accuracy. As such, our least squares inversion technique computes the set of optimal basin multipliers using no additional constraints or regularization."

7.) page 1318, line 25: - RACMO2 does not include ice dynamics, it's a regional climate model

Corrected.

8.) What time period did you use to compute the RMS, the GRACE period?

2002-2012. We have now added this information in the paper.

9.) I assume you use cumulative SMB anomalies (which is what GRACE would mea-

sure), not the monthly SMB as output by the model? The same applies for the GMB signals, as in fig. 5.

Correct. We have updated the term in the paper to “cumulative SMB” rather than just “SMB”.

10.) 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.

I apologize for the confusion in this line. All that was meant is that the same (time-varying) external signal, meaning the hydrology and oceanography everywhere outside of Greenland, is used in all simulations. I have removed this line and slightly altered the remaining lines to make that clearer.

11.) 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.

Yes, these are actually the values for the cumulative SMB anomalies, from RACMO2. To compute them, I separated the trend and climatology from the residual, then computed the RMS of that and compared to the RMS of the full signal in each grid cell. Especially over the coast, a climatology (not a pure sinusoid, though) and a trend really do capture almost all of the mass signal. I was surprised, too.

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

I have totally altered this section, both for clarity and to include the variable length

scales you suggested earlier. Hopefully things will now be more clear.

13.) page 1322, line 10: what is the beta-value (.85) based on. This actual value will vary from location to location. Instead of using a fixed value, it would make more sense toestimate the local value from the RACMO data, using the lag-1 autocorrelation value.

Agreed. We have now done that.

14.) page 1329: square root missing in denominator?

Oops! Corrected.

Thank you so much for your suggestions,

Jennifer Bonin

Interactive comment on The Cryosphere Discuss., 9, 1315, 2015.