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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 #1

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Overview: This article describes what resolution a satellite gravimetry mission must accomplish in order to theoretically separate surface mass balance signals from glacial discharge on the Greenland Ice Sheet. The authors methodically present their simulation procedure, and walk through the results. This paper merits publication; however, I feel that it can use some additional clarification (particularly in the methodology and results sections) prior to publishing.

General comments:

- The authors go through painstaking detail to explain how they reconstruct synthetic C405

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surface mass balance and glacial mass balance signals for the simulation. However, there is no discussion on the actual inversion methodology, and the results are presented in a confused manner. The transition from Section 3 to Section 4 is abrupt, creating a confused environment to the reader. I recommend that you include some definition of the least squares inversion process that you are implementing, rather than leaving the burden upon the reader to connect the methods of your previous paper, with the methods of this paper. A high level overview is at least desirable. It is clear to me how you construct the "truth" signal (page 1319, second paragraph). You then perturb the SMB and/or the GMB according to Section 3.1 and 3.2 (also this description is adequate). But, exactly how you are doing the least squares inversion relating the "truth" to the perturbations of the "truth", and precisely what your state parameters are during the estimation process, is ultimately confusing and seemingly omitted from the text. Another paragraph (or section) needs to be added to clarify this.

- Section 4. Ultimately, this section of text is also confusing. I recommend constructing a table for the reader to very clearly articulate the parameters that you varied, and the parameters that were kept constant during the least-squares fit for each case. I assume that the first sentence in Section 4.1 should read "SMB-misfit-only simulation" rather than "GMB-misfit-only simulation"? What exactly is the difference between Figure 7 and Figure 8? I assume in Figure 7 that the GMB is perfectly known (no-noise added), but the SMB is varied? And vice-versa for Figure 8? I also assume that no stripes are added in either of these tests? These details are very important for proper interpretation of the results, and should be presented more clearly.

Specific Comments:

Page 1317, Line 22: I am sure you are aware, but perhaps it is worth mentioning that some GRACE solutions extend beyond degree 60, but these degrees are mostly contaminated by noise.

Page 1318, Line 25: You state that you use the RMS from the RACMO2 ice model. I

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believe referring to this as a regional climate model is more appropriate. Also, please include additional description of what you mean by the RMS. Is this the RMS of SMB anomalies about some mean time period, or are they absolute SMB values? What time period are you computing the RMS for? I can imagine that in locations for which the mass balance is dominated by dynamics, rather than SMB (such as Northwest Greenland and Southeast Greenland - see Velicogna, 2014), that this regional-dependent weighting scheme is insufficient and introduces errors which are unaccounted for in your process. Could you comment on this?

Page 1322: Why did you choose to make only 6 simulated data sets? This does not seem like enough to achieve statistical significance in your results. Are you confident that you have done so, and can you explain this choice?

Page 1323, Line 5: The use of GMB and SMB is perhaps a bit careless throughout the manuscript, and it shows very clearly here. In Figure 5c, you have the GMB of a glacier going positive. In fact, the GMB is always negative; glaciers are always discharging. I believe what you are showing in Figure 5 are anomalies with respect to a background discharge rate? Same comment with respect to SMB, for instance on Line 8, you state that the this discharge signal is much larger than the SMB signal. However, the full magnitude of the SMB is generally very large and very positive - I believe you mean "SMB anomaly" rather than "SMB signal", where the SMB is an anomaly with respect to some mean timeframe?

Page 1327, Line 7: Please explain what you mean by "relative strength of the SMB-misfit errors over the GMB-misfit errors". Even though there are clumps of 6 in Figure 9, it is difficult to see this, and the reader does not know which clump of 6 belongs to which run?

Figure 9: Here, you show the simulated mass balance for the glacier basins. I assume you could also show this for the other basins (where only SMB was modeled)? Is it possible to do this, or perhaps show just a couple of these, so to elucidate the reader

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on both your processes as well as your results?

Page 1330, Line 22: It is stated that a stripe reduction could allow for separation between SMB and GMB. However, this separation is only possible for annual timescales, since you must apply a yearly smoothing to the data, is this correct? Also, can you please address the issue of having other smaller outlet glaciers within your "SMB" basin? That is you are trying to separate SMB from the three largest outlet glaciers. However, inherently, there are other glacier dynamics that you are capturing in your SMB because of smaller outlet glaciers that you are not modeling. Is this correct? Could you comment on this in the manuscript?

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

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9, C405–C408, 2015

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