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 #3

The aim of the manuscript is to investigating the separation of SMB-induced and dynamically induced Greenland ice mass changes from satellite gravimetry. In a fully controlled simulation environment, simulation experiments are carried out following the forward modeling (or 'mascon') approach frequently applied in GRACE analyses.

Synthetic signals are converted to gravity field changes and reduced in spatial resolution according to the resolution characteristics of GRACE (and possible GRACE successor missions). Simulated GRACE-like noise is added in some of the experiments. Based on the pseudo-observations thus generated, multipliers to a set of prescribed

mass change patterns are estimated in a least-squares sense. The prescribed mass change patterns contain patterns that ought to represent SMB-related changes and patterns that ought to represent dynamically induced changes. The quality of the separation is discussed based on the discrepancies between the simulation results and the "true" input signals. The authors find that with the current quality of GRACE solutions the envisage separation is impossible. This will not be surprising to anyone who has dealt with GRACE. Based on the chosen inversion setup, the authors find that the current GRACE errors would have to be reduced by a factor of 9 to achieve separability.

The monitoring of ice sheet changes and their attribution to the underlying physical processes is extremely important and satellite gravimetry has been playing, and will continue to play, an important role in this endeavour.

However, in my view, the manuscript is of limited relevance for cryospheric sciences and hence for The Cryosphere. Its submission was really premature. I explain my point in comments (a)-(c).

(a) Despite many details in the description of how the simulation datasets were generated, there is confusion about the actual experiments performed and the results presented. Section 4.1 starts with discussing "GMB-misfit-only simulations", a term which has not been introduced before. The related figure caption says the figure is about "SMB-only simulations". So what? - GMB or SMB? Line 12 on p. 1326 suggests to me that the simulations used the pure GMB signal as input signal (because the large amplitudes of the GMB signal are used as an argument). But line 19 suggests that the experiment discussed above uses pure SMB input signals. I can't figure it out.

(b) Not only does the reader get puzzled. On p. 1326, line 26-28, the authors are puzzled themselves by results that are at odds with plausible expectations. Namely, the resolution capability is worse at higher spherical harmonic resolution than at lower spherical harmonic resolution. Since the input data and the calculations are fully controlled by the authors, I suggest that the authors find and explain the reason, or the

error in their calculation, and ideally do this before submission.

(c) More fundamentally, dynamic imbalance happens at tens of glaciers or more. While the three glaciers Kangerlussuaq, Helheim and Jakobshavn cause about half of the dynamic imbalance at present, their isolation cannot be mistaken as the separation of dynamic changes from SMB, even less so for the future with unknown dynamic evolutions of the Greenland glaciers. It is clear right from the beginning that prescribing dynamic changes from a more comprehensive set of glaciers will make the dynamic-versus-SMB separation from GRACE alone even more impossible than in the manuscript. Luckily, such a separation from GRACE alone is not too relevant either, because complementary observation and modeling techniques are at hand to assist the separation.

Minor, but still crucial, comments:

- "Glacial mass balance" is not the right term to denote mass changes due to changing ice flow dynamics. Why should SMB be "non-glacial"??

- A more intricate point about the SMB-versus-dynamic separation: While the authors oppose dynamic signals to atmospherically forced signals (p. 1316, line 19), dynamic signals are partly triggered by SMB (and hence by the atmosphere) through complex feedbacks. [e.g., Murray et al., doi:10.1029/2009JF001522, Zwally et al.,10.1126/science.1072708]

- Line 2 of the abstract: "precipitation based SMB" sounds odd. Both precipitation and ablation determine SMB.

- Line page 1318, line 13: "roughly based off the island's drainage": not clear to me what this means.

- Line 25. RACMO2 is not an ice model.

- page 1324 line 8: You removed the JPL_ECCO ocean model? This sounds like you didn't account for the fact that an ocean model has been already removed during

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GRACE processing?

- page 1325, line 15 computed -> compared?

- page 1326, line 10: areas -> errors?

- page 1327, line 12: "certainly a plausible achievement...": Make clear here that you know about the n_max=96 solutions by CSR. There are, by the way, n_max=120 solutions by ITSG (TU Graz). As long as you disregard errors (as you do in this section) you may think about solutions at any resolution, anyway. You should incorporate a bit more GRACE insights here.

- page 1328, line 11: "quadrature summed" sounds odd to me.

- Line 15 "SNR increases everywhere". Incorrect, as far as the figure shows for basin 2.

- page 1329, line 19-21: The argumentation is not convincing. Large north-south elongated SMB basins are as well prone to absorb stripes.

- page 1330, line 10: The sentence about the 90 x 90 resolution has limited sense, given the existence of such solutions.

- Fig. 13) looks like the signal consists of two grid cells, contrary to the explanation in the main text.

- Fig. 10b: wrong ordinate axis label