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Interactive comment on "Sensitivity of inverse glacial isostatic adjustment estimates over Antarctica" *by* Matthias O. Willen et al.

Anonymous Referee #2

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The manuscript by Willen et al. entitled "Sensitivity of inverse glacial isostatic adjustment estimates over Antarctica" explores the impact that various assumptions and input data sets have on the estimation of Antarctic GIA and ice mass change. The authors follow the methodology outlined in Gunter et al., 2014, but expand the analysis to include additional and updated data sets, such as the inclusion of additional altimetry measurements from Envisat and Cryosat-2, an updated RACMO climate model (v2.3), and an extended GRACE time series. The manuscript is well written, and is clear in describing the processing steps. Overall, it's a useful study and moves the state-of-the-art on this topic. That said, there were a handful of concerns/comments I came across while going through the manuscript. Below is a summary of the major and minor items, which I would kindly ask the authors to respond to:

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Major items: ————

1) The GRACE time series, regardless of processing center, are relatively consistent, so there is little variation in this input data set. The SMB data sets show some significant variation (Fig. 2), but the limitation here is that firn height estimates can only be computed from the RACMO model, and not from the MAR model. If I understood things correctly, the authors do perform an EOF analysis on the model differences, and then use these to generate uncertainty estimates for RACMO. It was unclear exactly how this was done, so I think it would help to expand on this in the text (end of p. 12). How exactly are the errors added (sqrt sum of squares of each EOF sigma at each grid cell point)? And was the same approach applied to the hdot_firn term? If so, is this realistic, since firn compaction works over longer time scales and may be non-linear? I also didn't completely follow the statement at the top of p. 15 regarding the creation of 32 separate GIA estimate from 32 different trend estimates. Did you, for example, take a trend difference from one of the 32, 7-yr windows, add that to the nominal RACMO trend, and then calculate a GIA solution?

2) An alternative, and perhaps more complete, assessment of the influence of the SMB models might be to run the combination analysis without the altimetry inputs. The altimetry only serves to update potential mismodeling in the SMB estimates, and to identify areas of glacial thinning. A fixed map of regions of glacial thinning could be developed, e.g., derived from published surface velocity plots, and used to remove the regions with ice density. This thinning map would only need to be representative, since the purpose is only to examine the sensitivity of the SMB inputs. Then, if you use the same GRACE time series, this would essentially isolate the contribution of the SMB model on the combination. And it would show what a combination with RACMO and MAR would look like in a side-to-side comparison.

3) The treatment of the altimetry data was a concern for me. The reference altimetry product was the multi-mission (MM), but no plots are shown of the default uncertainty estimates of the trends for this data set, although the authors do mention these uncer-

tainties are used in the combination. Furthermore, the altimetry does not appear to be calibrated to the LPZ like the other data sets. Without this, any reference frame offsets or other biases from the altimetry data will find their way into the combination solution. This is why the GIA and GRACE LPZ is implemented, and is why Gunter et al 2014 also estimated their ICESat biases over the same LPZ.

4) Following on the prior point, the application of the density term, rho_alpha, in the combination is going to be directly affected by the altimetry product (as recognized by the authors). An inspection of the density map in Fig 5 shows very few areas that appear to have values of zero. This suggests that for nearly all of Antarctica, including most of EA and the LPZ, the difference between the altimetry and FDM heights was > 2-sigma. This means that very few regions used the default mdot_firn value from the SMB model. Referencing Gunter et al, 2014, they note that the classification of the rho_alpha term was used to "only deal with potential residual signal observed between ICESat and the FDM. The majority of the surface mass changes come directly from the SMB estimates (i.e., mdot_firn) derived from RACMO2." See also their Fig 7, which shows where the dominant positive differences are found, which are limited to a few near-coastal regions. Is this also the case for the current study? There was not a difference map between the MM and FDM trends, so it's unclear whether the 2-sigma difference was large or small (and does this difference show near-zero change over the LPZ?).

5) It also appears that the MM altimetry is heavily influenced by the Envisat processing, as the density maps in the supplement (Fig S4) for the Envisat and MM look nearly identical. The ICESat density maps shows much more zero-density values. The Envisat altimetry shows large areas of EA (see e.g., the Dome Fuji region) with negative surface height change compared to the FDM, so these large areas are assigned a density of ice (917 kg/m3). Based on Fig 1., GRACE does not see mass loss in that region, so in the combination this difference is estimated to be GIA. This is why there is a large positive uplift seen in the Dome Fuji region. This positive BEC feature may

just be due a processing artifact of the Envisat data (e.g., an atmospheric correction or penetration bias, as described by Remy et al, 2014). It is these types of differences that I believe led the authors to state in their conclusions that using the rho_alpha criteria "does not lead to a physically evident pattern to account for processes in the firn and ice layer (Fig. 5A, S4). Furthermore it is sensitive to input data sets. We suggest to use predefined density maps with significance criterion accounting for all input data sets" (p. 22, ln 5). This raises some interesting points. First, the combination will always be sensitive to the input data sets – that's the nature of real-data combinations. It may be that the patterns seen are products of the input data sets, and not the combination methodology, and the solution will only improve when those input data sets are refined (to include GRACE, altimetry, climate data, etc.). Second, if a predefined map is used to designate regions of ice loss or unmodeled accumulation, then you might be forcing the data into a predefined result. And, what other data input would be used to generate this new map? It wasn't clear to me how this alternative approach would work, and what improvement it might have. Perhaps the authors can provide a sample case in which the suggested predefined density mask is used, and how this compares with the reference case. It's worth noting that Gunter et al 2014 do use a predefined density map similar to the Riva 2009 when assigning densities to the positive ICESat-FDM height changes > 2-sigma. It is only if this height change is negative and > 2-sigma that the density of ice is used, since it is assumed that such large negative height changes are due to ice loss.

6) A Cryosat-2 elevation trend map is not provided, but is a critical component to Sec 5.5 and 5.6, which claim that the combination approach is sensitive to the time interval used. No maps of the corresponding density for Cryosat-2 are provided either. Some mass change values are provided, and a match to Sasgen et al 2019 is implied, but only when the GRACE LPZ bias is ignored, but presumably with the GIA LPZ biased used. All of this does not provide very strong support for the claim that "GIA estimates depend on the used time period" (p. 21, ln 4). I would argue that as long as the input data is accurate, the time period shouldn't matter.

7) At several points in the paper, the authors present findings from a mixture of biased and dedebiased data sets. One example is in Sec 5.5 (p. 21, ln 4). Table S1 is another example. I can see the value in showing the magnitudes of the bias estimates, but a mass change result from, e.g., a debiased GIA solution and a biased GRACE solution, seems inconsistent. I would think you should only present either a fully biased or debiased solution to stay consistent. Otherwise, the various frame, deg1, and C20 biases get mixed differently depending on the combination chosen, and the solution becomes a mixture of global and regionally-constrained data.

8) Modeling the elastic correction as a constant scale factor (pg 9, ln 15) of the altimetry height change may introduce error, especially in regions such as the AP and ASE (where thinning and accumulation are significant). Were the actual magnitudes of these elastic corrections investigated? And how would these BEC corrections be distinguished from large viscoelastic responses suspected in these same regions?

9) While the authors are clear that it is a sensitivity study, there is no validation of the results (Gunter et al 2014 used GPS site displacements), so there is no assessment as to whether the variations observed by changing the input data sets are an improvement or not.

Minor items: ————

10) p. 12, ln 5: Just to clarify, the uncertainty you refere to here is the uncertainty as derived by the IMAU group for the mdot_firn term?

11) p. 12, ln 9: The 7-year window seems arbitrary. Why not 10 or 5 or 3 yrs? And, how does EOF analysis vary if another window timeframe is chosen?

12) p. 21, ln 4: certainly a longer period will result in more reliable results, but if the inputs are correct, the time period shouldn't matter. This statement should be rephrased to say that it depends on the quality of the input data sets.

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