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Discussions

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

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The referee comments are enclosed with accents and indicated in italics. Blue text is used to indicate the author's response and changes in the manuscript.

We thank you for the positive comment and your suggestion to overcome the deficiencies of the manuscript.

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?"

We used differences of estimated trends of cumulated surface mass balance anomalies (cSMBA). We assume those differences representing (a part of) the error of regional climate modelling. Unfortunately there is only the IMAU-FDM forced with RACMO2 outputs and no equivalent FDM forced with MAR outputs. For this reason we cannot directly get trend differences of firn thickness trends from two models. At the end of section 3.3 we explain how we estimate pseudo trend differences of firn thickness trends using density fields from MAR. We stated in the manuscript that this does not consider the correct evolution of the firn layer. The EOF analysis is done with the cSMBA trend differences. The normalised EOF is scaled with the square root of the particular eigenvalue (sigma). For the propagation towards the combination approach a pseudo firn thickness EOF is estimated using MAR density fields. The first three EOFs are added separately to the estimated cSMBA trend and firn thickness trend, respectively. We are aware of that this can only be a start of a rigorous uncertainty characterisation of climate model outputs and only consider a part of aspects. In the manuscript we extended Sect. 4.1 with a reference to Sect. 3.3 where we explain how pseudo EOFs and trend differences are computed. Yes, (1) we calculate the cSMBA trend difference, (2) calculate the pseudo firn thickness trend difference with the MAR density, and (3) add them to the nominal cSMBA and firn thickness trends, respectively.

With those updated trends we estimate a GIA solution. We do this for every trend difference resulting in 32 GIA solutions. We clarified this at the end of section 4.1.

"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."

In our study we focussed on the combination approach published by Gunter et al. (2014). From a historic point of view, Wahr et al. (2000) suggested the combination of satellite gravimetry and altimetry to isolate the GIA signal. Gunter et al. (2014) used climate model products to overcome some limitations of the geodetic satellite data. We agree it would be worthwhile investigating differing data/model combination strategies to isolate GIA. On the one hand, the suggested strategy would give more insights using climate model products in combination with GRACE-derived gravity fields. On the other hand, this investigation will increase the complexity of the study. This is a point of criticism from the first referee.

"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 uncertainties 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."

We extended Fig. S1 in the supplementary material with uncertainty maps of every used altimetry product. GRACE-derived area density changes are not calibrated to the LPZ prior the actual combination (Eq. 9). GRACE-derived area density changes and the GIA solution from the combination are calibrated over the LPZ to determine the mass balance. In other words: The combined result derived from GRACE, altimetry and firn process models, namely the GIA-induced BEC, is calibrated over the LPZ. Existing biases sum up in the combination and are jointly removed. We explained this in more detail in Sect. 2.2 in the manuscript. But still the calibration of the used altimetry products is different to Gunter et al. (2014). Gunter et al. (2014) uses the LPZ to estimate the ICESat campaign biases. This results in a zero trend of SEC over the LPZ. The campaign biases from Schröder et al. (2019) are calibrated with kinematic GNSS measurement over Lake Vostok. The inter-mission biases during relevant period are calibrated via overlapping observations. More details can be find in Schröder et al. (2019).

"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?)."

Fig. 7b in Gunter et al. (2014) shows differences between surface elevation changes derived from ICESat and the FDM. But only differences are shown which are greater than 6 cm a^{-1}. It is unclear to us why this threshold was used. Unfortunately, it is not shown where the difference is $>$ 2-sigma. Unlike us, Gunter et al. (2014) do not show their ρ_α map which is used in the combination and would provide information where the difference is $>$ 2-sigma. Fig. 1 (in this comment) shows the differences between ICESat and FDM we used as input without clipping. Those differences are small in EA. But those small differences are weighted with ice density, because they are $>$ 2-sigma. In comparison, Fig. 1 shows the map published in Gunter et al. (2014), with the 6 cm a^{-1} threshold.

"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."

Our results demonstrate the strong sensitivity towards differing altimetry products. ICESat and Envisat are different with regard to observing technique, spatial and temporal coverage, and temporal sampling. We agree that the limitations are due to the quality of the data which was not clear enough in the manuscript. We improved this. The case distinction of ρ_α is made to cope with apparent limitations of the firn-thickness trends and altimetry derived trends instead of using the formal approach (Eq. 8). A further investigation of different combination strategies would be very beneficial. Including the aim to find a better combination methodology would make the study more complicated. To avoid this, we removed speculative phrases on possible improvements in the manuscript and focussing on the sensitivity assessment of the investigated approach.

"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."

We agree that as long as the input data is "correct" there is no time dependency. As you mentioned, the true limitations of the input data is the reason for differing results. We rephrased the corresponding paragraphs. The Multi-Mission-Altimetry product is dominated by CryoSat-2 observations during the time period 2010-07/2016-08. We clarified this in the manuscript. Fig. S1F shows the SEC. We decided not to use an additional CryoSat-2 only experiment. Fig. 2 (in this comment) compares Multi-Mission-derived and CryoSat-2-only-derived SEC. As you already mentioned, also Fig. 2 shows that the Envisat processing influences the result.

"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."

We fully agree and only present completely biased or debiased estimates. We removed mixed values from the text and Table S1 from the supplementary material.

"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?"

The constant scale factor does introduce error but this is negligible (Riva et al., 2009, Groh et al., 2012). We pointed that out in Sect. 3.1. The strong Gaussian smoothing further mitigates the influence of this error because large local amplitudes are damped. For illustration Fig. 3 (in this comment) compares vertical elastic deformation rates calculated from smoothed Multi-Mission-altimetry trends. This is done (1) by modelling in the spatial domain and (2) by the constant scale factor of 1.5 %. For (1) we used a predefined density mask to estimate mass change rates and rheological parameters from PREM. The differences between (1) and (2) vary between approximately -0.1 to 1.0 mm a^{-1}.

"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."

As you mentioned, our aim is to address the sensitivity of an existing methodology from which we conclude limitations. The investigated approach might be inappropriate to judge the quality of input data sets with GNSS observations. All input data sets are combined with existing bias. This bias is jointly removed using the LPZ-based bias correction. During this step unknown systematic errors in the input data can cancel out each other. GNSS observations might judge those input data sets as an improvement.

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?"

This is uncertainty we receive (1) from our least square estimation when we estimate trends of cSMBA and (2) taking 10 % of the estimated cSMBA trend over the ICESat-observation period. We clarified this in the manuscript.

"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?"

We used a 7-year window, because this corresponds to the ICESat's observation period. We stated this in Sect. 3.3. The trend differences would increase if the time interval is shorter and decrease with a longer time window. Fig. 4 (in this comment) shows the first three EOFs using a 5 year, 7 year, and 10 year time interval, respectively. The dominant patterns remain with respect to spatial pattern and amplitude. Whereby the amount of the explained total variance changes.

"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."

We agree and rephrased this statement.

References

Groh, A., Ewert, H., Scheinert, M., Fritsche, M., Rülke, A., Richter, A., Rosenau, R., and Dietrich, R.: An Investigation of Glacial Isostatic Adjustment over the Amundsen Sea sector, West Antarctica, Global Planet. Change, 98–99, 45–53, https://doi.org/10.1016/j.gloplacha.2012.08.001, 2012.

Gunter, B., Didova, O., Riva, R. E. M., Ligtenberg, S. R. M., Lenaerts, J. T. M., King, M. A., van den Broeke, M. R., and Urban, T.: Empirical estimation of present-day Antarctic glacial isostatic adjustment and ice mass change, The Cryosphere, 8, 743–760, https://doi.org/10.5194/tc-8-743-2014, 2014.

Riva, R. E. M., Gunter, B. C., Urban, T. J., Vermeersen, B. L. A., Lindenbergh, R. C., Helsen, M. M., Bamber, J. L., van de Wal, R. S. W., van den Broeke, M. R., and Schutz, B. E.: Glacial Isostatic Adjustment over Antarctica from combined ICESat and GRACE satellite data, Earth Planet. Sci. Lett., 288, 516–523, https://doi.org/10.1016/j.epsl.2009.10.013, 2009.

Schröder, L., Horwath, M., Dietrich, R., Helm, V., van den Broeke, M. R., and Ligtenberg, S. R. M.: Four decades of Antarctic surface elevation changes from multi-mission satellite altimetry, The Cryosphere, 13, 427–449, https://doi.org/10.5194/tc-13-427-2019, 2019.

Wahr, J., Wingham, D., and Bentley, C.: A method of combining ICESat and GRACE satellite data to constrain Antarctic mass balance, J. Geophys. Res., 105, 16 279–16 294, https://doi.org/10.1029/2000JB900113, 2000.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-95, 2019.

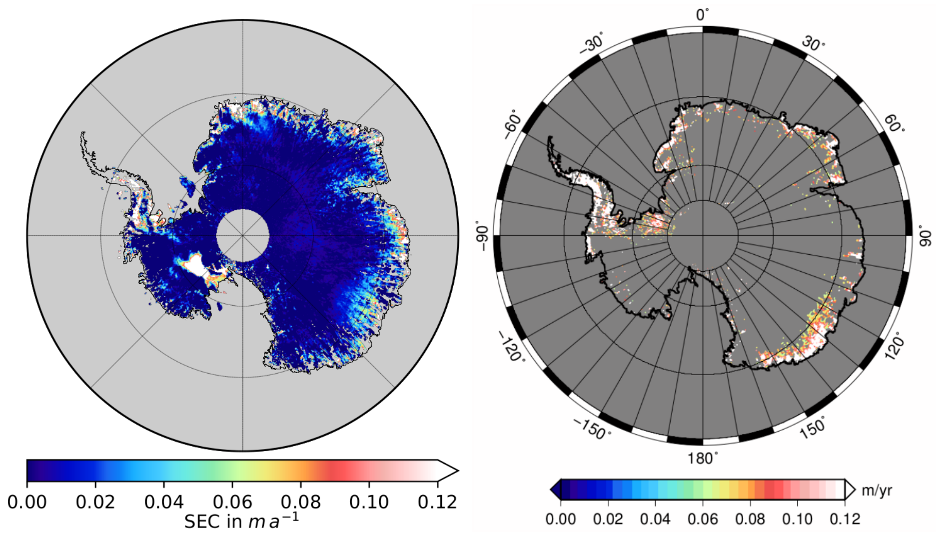

Fig. 1. Left: The differences between ICESat and FDM from input data sets we used. Right: The original figure from Gunter et al. (2014) with a threshold of 6 cm/a and the masked Kamb Ice Stream.

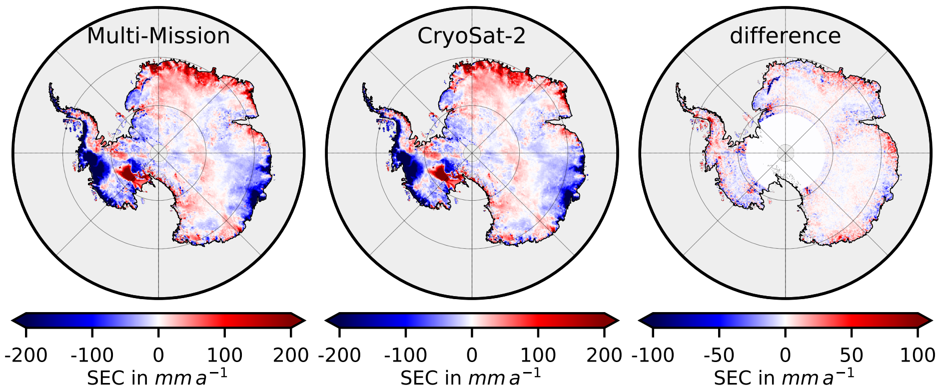

Fig. 2. The comparison of Multi-Mission-Altimetry derived trends (left), CryoSat-2-only derived trends (middle), and the difference of Multi-Misson-CryoSat-2 (right). Note the different value range.

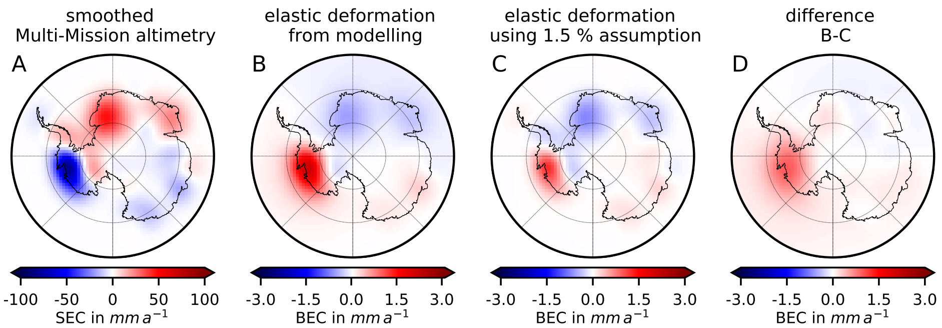

Fig. 3. A: Trends from MM altimetry (Gaussian smoothing, half response: 400 km). Elastic-induced BEC using modelling (B) and estimated with -1.5 % from A (C). D: The difference map between B and C.

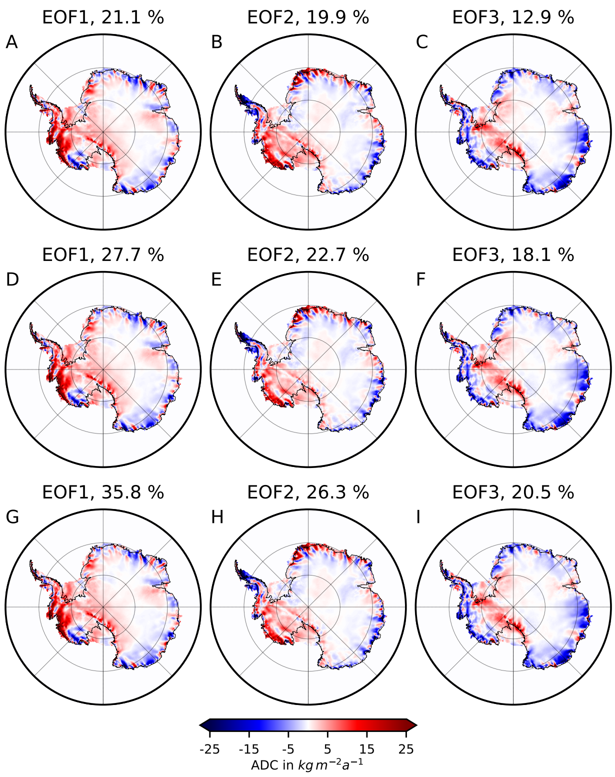

Fig. 4. Comparison of the EOF analysis using different time intervals. A–C, D–F, and G–I show the first three EOFs estimated over 5-year, 7-year, and 10-year time interval, respectively.