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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.

General comments

"The manuscript presents a rigorous sensitivity assessment, and the results clearly show that it is problematic to simply assume one specific model/data set/product in this kind of analysis. Clearly the spread between different (equally valid) models/products can be larger than the uncertainty claimed for each product. This is a very important conclusion. The manuscript is well written, and the figures are clear and illustrative.

The complex study setup with many variables makes the manuscript a bit challenging to read though. Therefore, one recommendation for the authors is to consider if there is a way to present the results from Table 2 in a figure instead. In summary, I find that the methods applied are sound and robust, the manuscript well written and the results important, making the work worth publishing I have listed below some specific recommendation that I think the authors should address before the manuscript is published."

Thank you for your positive and constructive feedback. In Table 1 we provide an overview of all experiments and the used input for the sensitivity analysis. Fig. 6 and Fig. S7 illustrate the debiased and biased results from Table 2.

Specific comments

"p. 1, l. 1: strictly speaking there is also a bottom-melt term in the mass balance equation, even though it might be very small."

We added basal melt in the text.

"When you discuss firn it seems to me that you think of firn processes = SMB (e.g. p.1, l. 35). Do you not differ between firn and snow? I would thinks that part of the SMB signal (on short temporal resolution) is caused by changes in snow and not firn and therefore your definitions confuse me. Please clarify this."

We do not take into account a separate snow layer such as Zammit-Mangion et al. (2015). We summarise mass and volume changes of the ice sheet which do not take place in the ice layer with the term *firn processes*. We clarified this in the introduction of the manuscript.

"You argue that you can characterize the uncertainty of the SMB by comparing two models (RACMO and MAR), but do your results not imply that this might not be sufficient? The variability between those two are so large (fig 2) that it would seem very

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relevant to include more models. Please comment on this. Maybe no other models are available?"

Thank you for pointing on this as the uncertainty characterisation of the models is a challenging task. At the moment, there is no rigorous study on this topic. It would be very worthwhile to use an ensemble of climate model products for this, e.g. by forcing the climate models with changing input parameters. But they are highly computational expensive. RACMO2 and MAR are the only two regional climate models which are comparable with regard to forcing, time period, and spatial coverage. We added this limitation at the end of Sect. 2.4.

"Are the errors you mention in line 4, page 3 actually errors?"

Since the models do not provide uncertainties, we argue that the differences of the model outputs represent the uncertainty of them. We explain the characterisation in Sect. 2.4 and 4.1.

"In eq. 10, please explain the case of α = 0. I do not understand the physical meaning of this. Why is assuming 0 a better choice?"

The case of α = 0 is used if the $2\sigma_h$-criterion is not reached. In this case $\dot{m}_{GIA} = \dot{m}_{grav} - \dot{m}_{firn}$. This means that no mass change in the ice layer is considered and mass changes of the ice sheet are fully described by the modelled trend of cumulated surface mass balance anomalies. We extended the explanation of this case in Sect. 2.1.

"Your results are dependent on some assumptions, one of which is that the only region in Antarctica that experiences glacial thickening is the Kamb Ice Stream. I think that this is an important assumption. Can you back it up by more references?"

We added the references Retzlaff and Bentley (1993) and Wingham et al. (2006).

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“I find it a bit strange to state that one of your aims is to reproduce the method of Gunter et al., (2014). It might be something you have to do for you to reach another aim, but I don’t see it as your aim to reproduce previous results (p. 5, l. 16).”

We agree and removed the corresponding sentence.

“Regarding you assumptions on GIA-induced BEC: Please specify what threshold you use to define what is negligible. Also is there some references to back up you assumption that it is indeed negligible in the LPZ. (p. 5, l. 21-23).”

A GIA-induced BEC in this area is predicted by GIA models from approximately -3 until +1 mm/a (Whitehouse et al., 2019). We do not define a threshold. Gunter et al. (2014) argue, “if any genuine GIA over the LPZ does exist, then this would erroneously bias the empirically derived rates from the combination approach; however, as mentioned already, any error of this kind is believed to be much lower than that introduced by the various other (imprecisely known) bias contributors.” We discussed in Sect. 5.2 that the LPZ-based bias correction is a limitation of the combination approach. Further we extended Sect. 2.2 that the assumption of neglecting a small GIA-induced BEC in the LPZ introduces error.

“Can you please elaborate on why a consistent filtering of the quotient is not possible? (p.6 ,l.8). Is an ocean leakage mass signal of 4.5Gt/year not relevant to take into account? (p. 6,l. 13).”

A consistent filtering is not possible because we do not have access to an unfiltered $\dot{m}_{grav}$. GRACE derived monthly gravity field solutions are available with a theoretical spatial resolution of 150–300 km (Wouters et al., 2014), which is much less than Altimetry and the firn-process models with a resolution of roughly 10 km and 30 km, respectively. Furthermore, a filtering (smoothing) of the gravity field is unavoidable, because of the dominant error pattern. In the quotient this would be weighted with the high-resolution density mask. As we do not evaluate the sensitivity of filtering, the ocean-leakage mass signal is the same in every experiment. We clarified this in the manuscript.

"The sentence in p. 7., line 13-14 seems distached from the rest. Can you elaborate a little on what the implications of such low viscosity areas are for your study?"

If there is a very low viscosity the assumption of a linear GIA-induced BEC introduce error. We extended the paragraph in the manuscript.

"Can you please explain why the altimetry combined time series differs in spatial coverage? I understand why they may be different from one mission to another but why from month to month? Due to data loss in some areas?"

The combined altimetry time series is compiled from observations of various altimetry missions. For example, ICESat and Envisat observed parallelly. Whereas ICESat has a higher spatial coverage than Envisat (polar gap), but only with a campaign-style temporal sampling. As a result the combined monthly-sampled time series has a higher spatial coverage during the months with observations from ICESat and Envisat. Further, as you mentioned, differing data quality through time is another reason. We extended the corresponding paragraph in Sect. 3.1.

"Please back up the statements that the ITSG-Grace2016 has the highest s-t-n ratio with some reference(s)."

We added the reference Jean et al. (2018). Therein ITSG-Grace2014 shows the lowest noise level. We rephrased the paragraph, because release 6 solutions including ITSG-Grace2018 presumably show a higher signal-to-noise ratio.

"There is provided no explanation for why you choose a different annual precipitation as threshold for low precip. than what was used in Gunter et al., 2014."

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We used the 20 mm/a threshold used in Riva et al. (2009). We added the reference in the manuscript.

Technical issues

"p.2, l.3 : on Earth → on and in Earth"

"p.2 , l. 7: → .. through glaciations and deglaciations during the last.."

"p.2, l. 31 : are beyond → are larger that the"

"p.3., l. it is explained → we explain"

"Fig.2 : Clarify which altimetry product is visualized"

We implemented all suggestions.

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