

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

Anonymous Referee #1

Received and published: 18 June 2019

Review of “Sensitivity of inverse glacial isostatic adjustment estimates over Antarctica” by Matthias O. Willen et al., submitted to The Cryosphere Discussions.

Summary:

The authors present inverse GIA estimates over Antarctica derived from a combination of satellite altimetry and gravimetry. Specifically, the authors investigate the sensitivity of such inverse GIA estimates to a suite of different parameters such as choice of altimeter record, surface mass balance model / firn-process models, and degree-1 and C20 products. An important component of their method is the bias correction applied (the mean GIA and GRACE trend is set to zero over the low precipitation zone in East Antarctica). The authors present the spread of GIA estimates caused by the different variables and conclude that the choice of altimetry product poses the largest

C1

uncertainty on the debiased mass-change estimates.

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.

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.

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 think 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.

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

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

In eq. 10, please explain the case of $\alpha = 0$. I do not understand the physical meaning

C2

of this. Why is assuming 0 a better choice?

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?

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).

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

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) .

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

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?

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

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.

Technical issues

p.2, l.3 : on Earth -> on and in Earth

C3

p.2 , l. 7: -> .. through glaciations and deglaciations during the last..

p.2, l. 31 : are beyond -> are larger than the

p.3., l. it is explained -> we explain

Fig.2 : Clarify which altimetry product is visualized

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

C4