

The Cryosphere Discuss., referee comment RC1  
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## Comment on tc-2021-17

Anonymous Referee #1

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Referee comment on "Basal Water Storage Variations beneath Antarctic Ice Sheet" by Jingyu Kang et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-17-RC1>, 2021

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### General comments

The authors present a data-driven approach to estimate basal water storage variations (BWSV). From GRACE time-variability gravity Kang et al. reduce gravity signals due to surface processes, GIA, and basal mass balance induced vertical movement of the Antarctic ice sheet. For this purpose the authors use ICESat altimetry data and data from models. A gravity forward modeling approach is applied to the residual gravity rate to determine the gravity rate which is caused by changes of the basal mass balance. This is used with changes of the basal melt to estimate BWSV. The determination of BWSV from satellite gravimetry and satellite altimetry observations is novel and studies on this topic are very important for the understanding of the mass balance of the Antarctic Ice Sheet.

I see very fundamental issues in the presented work that do not meet scientific standards. I suspect methodological issues which attribute mass changes related to ice dynamics to BWSV. The presented investigation used only one GRACE processing, one altimetry variant, and a single GIA model. Further, the presented results are of similar magnitudes as errors of the input data are expectable in several regions. In those regions it remains open whether the presented result is a random result or whether significant signals of BWSV were indeed found by the authors. There are almost no independent observations for BWSV that allow external validation. Hence, it is very important to clarify the dependence on the input data. Furthermore, there are details missing that would allow a reproduction of the results. Processing choices are not completely transparent and justified.

I strongly recommend further investigations using other data sets and processing strategies, e.g. several GRACE solutions, altimetry products, and GIA models. In particular, investigations are necessary on the significance of the determined signals with respect to realistic uncertainties. Without being able to assess significance, the findings drawn are not robust to discussion. I judge the performed error analysis is not sufficient for this purpose. All processing steps and methodological assumptions made should be listed completely and described comprehensibly and transparent in a precise way. I recommend an extensive literature review on mass balance of the Antarctic ice sheet and related processes. Last but not least, language and structural revisions are needed. It is not comprehensible how much arbitrariness by the choice of the data sets and data processing choices is part of the result. I judge the presented investigations and the manuscript to be fundamentally and thoroughly revised.

## Specific comments

- A descriptive flowchart would help the reader to better understand the combination and underlying assumptions of all data sets during all investigation steps. Please provide all details on processing choices.
- In general, figure and table captions are poorly meaningful.
- The interpretation of the results is detailed, but too little attention is paid to discuss the limitations of the input data and the method itself. Here it must be carefully examined whether the result is significant and thus the conclusions can actually be made. Please provide a more quantitative discussion.
- The quality of the figures is poor. The axis labeling is inappropriate and only partly readable. A red-green colormap is not readable for readers with red-green deficiency. The caption of Figure 1 does not provide sufficient information.
- The consideration of elastic deformation is missing in section 2.1. How do you consider it for the used satellite observations?
- l 63: GIA not only causes deformation of the crust.
- Eq. 4: How do you take temporal changes of  $\rho_{\text{firn}}$  into account? How do you estimate  $\Delta_{\text{Ant}}$  and  $\Delta_{\text{FDM}}$ ? Please provide more details, e.g. a figure illustrating the surface density and its uncertainties.
- l 109: How do you estimate the temporal change of the basal melting during the investigation period?
- Section 2.3: You describe a general case. Please provide details, how do you proceed the inversion in the specific case. It is not transparent, e.g. how the discretization was done.
- Section 3.1: How do you tackle in detail the different resolution and sampling of the observational and model data?
- l 149: Does this mean that only months of GRACE were used in which ICESat simultaneously observed?
- l 158: "crustal viscoelastic deformation" is a misleading term.
- l 163: Dr. Hongling Shi is a co-author and should not be mentioned by name in the text. For this purpose there is the section "Author Contributions".
- l 170: Please provide details how you "forced the predicted basal uplift rates to the GPS observed uplift rates"?
- l 158+175: What do you mean by "annual trend" and how do you calculate it?
- Eq. 12: This should not be part of the results section. Please discuss and justify this approach of uncertainty estimation for the given input data sets. It is not comprehensible how  $m_{\text{surf}}$  is estimated. Please provide in detail how you estimate all uncertainties.
- Table 1: BWSV is provided in Gt/y but why is the standard deviation in Gt?
- l 192: Please provide references.
- l 194: Please discuss dynamic thickening of Kamb Ice Stream.
- l 206: Please discuss dynamic thinning (e.g. Shepherd et al., 2018, Mass Balance of the Antarctic Ice Sheet from 1992 to 2017, Nature). I strongly suspect that something has gone wrong methodically. Shepherd et al. (2018) provides a lot of references on the mass balance of the Antarctic Ice Sheet and related processes.
- l 236: Gt/y is not a volume.