

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-278

Anonymous Referee #1

Referee comment on "Basal Water Storage Variations beneath Antarctic Ice Sheet Inferred from Multi-source Satellite Data" by Jingyu Kang et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-278-RC1>, 2021

I still acknowledge the innovation of the paper in facing the challenge of estimating the BWSV. The authors apply a stepwise and iterative procedure to estimate this effect in a data-driven approach. For this purpose, temporal gravity changes of GRACE are used. These gravity changes are caused by different sources. The authors try to determine all causes, except the basal mass balance (BMB), by making use of additional information (altimetry (ICESat), firn modelling, GIA modelling, and density assumptions). Those are subtracted from the total gravity change, leaving gravity changes due to BMB and error. In the final step, BWSV is determined by using modelled basal melting rates. Nevertheless, methods for combining satellite data sets over ice sheets have been investigated in several studies in recent years. It should be noted that only the ICESat period (2003 to 2009) is considered here, although much more recent data sets such as CryoSat-2, ICESat-2, GRACE-FO, etc. are available. BWSV are certainly also relevant over the ICESat period, but it limits the novelty as well as the impact of the study. At the end the authors stated that data sets with a higher spatial and temporal resolution would come to a "more elaborate result". However, these data sets have been available for several years.

Compared to the originally submitted version in the first half of the year, there has been an improvement and many of the points raised by the reviewers have been taken into account. The explanation of the methodology is now more comprehensible. However, the existing manuscript needs another thorough revision. The first impression of the revised version does not seem very profound due to typos and imprecise formulations in some places. However, it is better than the original version. Nevertheless, because it is a resubmission I would have expected more thoroughness. Perhaps an external editing would be helpful before the editor and reviewer assess the manuscript again. To give examples: Some abbreviations are introduced several times (BMB is introduced ten times). Which value is chosen for R in section 2.2?

I suggest that the major concerns and especially the methodological aspects of the manuscript should be clarified first and that another review iteration should examine in detail whether the presentation of the results and the conclusions are justified. I have

started to itemise specific comments for section 1 and section 2.1, but I suspect that further revision will be necessary in the following sections. I would like to ask the authors to first thoroughly revise the rest of the text and to check sentence by sentence for meaningfulness.

General comments

So far, the introduction lacks references and a brief discussion of previous satellite data combination studies that focus less on estimating BWSV but more on the challenges of the mentioned data combination. For example, the authors refer to Gunter et al. (2014) in section 2.2. They use almost identical data sets over the same time period. However, this study is not discussed in more detail. Gunter et al. (2014) applied a bias correction to account e.g. for geocenter motion. Is it necessary to account for this potential bias in the presented study? Remarkable, Gunter et al. (2014) found an ice mass change of -100 ± 44 Gt/yr. This uncertainty of 44 Gt/yr is quite similar to your results. That the uncertainties of previous ice mass balance studies are of a similar magnitude as the results presented here is not mentioned to the reader. For example, the abstract lacks an uncertainty statement for the given ice mass balance.

The authors refer to Wahr et al. (1998) explaining how they convert the GRACE observations (level 2 data) into gravity changes at the Earth's surface (equation 13). Wahr et al. (1998) provide a formula for calculating surface density changes (mass change per area) that has been used to derive mass changes in other combination studies. This is also the case in Gunter et al. (2014) cited by the authors in section 2.2. Are integrated surface density changes from GRACE suitable to derive BMB? What is the need for using the more complicated layered gravity inversion (Equations 5 and 9)? This should be argued in the introduction of the methodology and supported with references. I think a statement of comparison to Gunter et al. (2014) is also necessary here. The authors should not misunderstand me. But, for the reader, the "gravity inversion approach" falls somewhat from the sky and as a reader I ask myself why this approach is useful for deriving the temporal BMB changes. Are there some similar use cases that the authors can refer to?

I suggest to restructure sections 2 and 3 more concisely. The structure of the methodology and data section was already criticised in one of the first reviews and only partly improved. First, all processes that are relevant could be described with a link to Figure 1 and the symbols of the related mass changes and height changes could be introduced. All the relevant effects, that are important for the reader (e.g. the mentioned supercooling condition), could be introduced here. Further a brief description of the spatial characteristics of the mass changes and height changes would be helpful. Afterwards, the bridge to the satellite data could be built. Observing characteristics could be discussed, and the necessary additional information could be named, which is necessary to quantify the effects of the involved processes. This could be followed by the methodology to resolve BMB and BWSV as well as the uncertainty estimation. Finally, the data processing section could be provided. An example: I 112: The consideration of dh_{IVM} and the iteration to estimate Δm_{BMB} can only be understood by the reader after section 2.3.

It is not entirely clear for me what the authors mean by "ice flow" and how it is observed by GRACE. The term "ice flow" might be imprecise. If the ice flow is constant, no mass changes or height changes would take place, or have I misunderstood something here? Do the authors possibly mean mass changes and height changes due to changes in ice dynamics?

Equation 4, determination of the surface density. Gunter et al. (2014) use a similar equation to link of the temporal height changes and mass changes caused by processes in the firn layer and ice layer. They further use modelled SMB data. The authors' text from l. 95 reads as if the actual density distribution of the ice sheet is derived here and one wonders how the density can be 0. Moreover, the actual density distribution in the firn layer over time is already provided by the FDM, isn't it? Applying this equation would lead to the fact that glacial thickening is not converted with density of ice but with a surface density. Does this leads to a wrongly considered mass change which is (at least partially) assigned to BMB and BWSV (Figure 11, Basin 9). l. 106 How do you determine the uncertainties Δ_{ICESat} and Δ_{FDM} ?

Equation 11 assumes that Δdg_{BMB} is determined by addition or subtraction of dg_{GRACE} , dg_{ICESat} , dg_{FDM} , dg_{GPS} , dg_{GIA} . This is not the case according to the methodology shown. How are dg_{ICESat} , dg_{FDM} , dg_{GPS} determined?

The symbolism using Δdg is might be potentially misleading. I had understood the "d" used by the authors as temporal derivatives, so "dg" could be the rate of "g(t)". The additional "delta" might be understood as the change of dg over a time period.

Would it be possible to derive dg_{surf} directly from modelled SMB?

Section 3.3: The equilibrium assumption underlying the FDM is not explained to the reader. What if this is not fulfilled?

I would like to ask the authors to take a closer look at the effects of glacial isostatic adjustment, because of the large uncertainty in Antarctica. The description from l 57 onwards and section 3.4 is imprecise. This was also criticised in one of the first reviews. Terms such as mantle convection are misleading in this context, because GIA refers to the viscoelastic deformation of the solid earth and the motion of the surface of the solid earth due to glacial load changes in the past. Mantle convection might point the reader to plate tectonics, mantle creep processes etc.

There is still little discussion of potential limitations due to the methodology itself and the data sets.

You conclude "A more elaborate result is desirable if utilizing more reliable and higher spatial/temporal resolution data, which deserves further studies." Please explain how this should work in detail if you apply a 300 km Gaussian smoothing and please explain what do you mean by "reliable" in this context.

Specific comments

I 13: Please provide an uncertainty of the total mass balance of the Antarctic Ice Sheet and the related time period. This is important to rank the provided numbers in the abstract. This implies to better write a percentage range rather than one number.

I 26: Do you mean a change of basal water storage can trigger a change in the ice flow velocity?

I 34: Can you please provide a reference for this statement?

I 36: What do you mean by "surface ice sheet"?

I 42: What do you mean by "gravity variations over Antarctic ice sheet (AIS) are considered as a combination of the gravity variations"? Do you mean a mass change or a mass redistribution due to ... lead to a temporal gravity field change?

I 60: What do you mean by "defined in geoid". Do you mean gravity field changes expressed e.g. as geoid height changes? What do you mean by "can be captured by GRACE"? Please be precise in introducing observations and processes.

I 61: You somewhat repeat what you have stated in I 54.

I 68: What do you mean by "integrated" in "integrated time-variable gravity variations"? Do you mean a spatial or a temporal integration?

I 70: Please explain gravity changes due to "ice flow".

I 73: Please introduce and explain "ice sheet's vertical movement".

I 76: Why do you provide methodological details about estimating dh_{CVD} here already?

References

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Wahr, J., Molenaar, M., and Bryan, F. (1998). Time variability of the Earth's gravity field: Hydrological and oceanic effects and their possible detection using GRACE. *J. Geophys. Res.*, 103(B12), 30205–30229. doi:10.1029/98JB02844