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 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 delta m_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 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 delta_ICESat and delta_FDM?

Equation 11 assumes that delta dg_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 delta dg 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
