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Interactive comment on "Seasonal mass variations show timing and magnitude of meltwater storage in the Greenland ice sheet" *by* Jiangjun Ran et al.

Anonymous Referee #1

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1 Overview

Ran et al. (2018) quantify the amount of meltwater retained within the Greenland ice sheet using estimates of seasonal mass change from the Gravity Recovery and Climate Experiment (GRACE) and surface mass balance (SMB) outputs from regional climate models. The paper works to improve our understanding of mass changes over the Greenland Ice Sheet over seasonal time-scales. The work presented by the authors falls within the scope of The Cryosphere and is a very interesting approach to quantifying meltwater retainment. However, I am skeptical of the results and the estimated uncertainties. Uncertainty in the parameterization of meltwater refreezing and

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Discussion paper

retainment within a regional climate model could justify a relatively broad suite of results beyond the uncertainties presented here. I believe there are a number of issues that should be resolved before the publication of this manuscript.

2 Broad comments

- The overall organization and structure of the manuscript could be improved to make it to work better as a journal article.
- I think the weighting algorithm is penalizing semi-annual and interannual oscillations in your GRACE estimates. I'm not sure if it is causing signal leakage (to other mascons or out-of-the-system) or if it is simply dampening the signal. The semi-annual oscillation change could explain part of the discrepancy between your estimates that you mention on Page 14, Lines 17–18. This would affect the seasonal results for the weighted GRACE datasets. It might be better to simply use the unweighted model due to its correspondence with other independent GRACE estimates.
- Have you considered using the Fettweis et al. (2017) outputs of the MAR model? Estimates of regional snowmelt can be quite different compared with RACMO2.3. The model outputs are freely available online.
- I suggest detrending the data in the seasonal figures to be similar to Alexander et al. (2016). Alternatively, could remove the residual from an EMD method similar to (Luthcke et al., 2013) or a moving-average estimate. As is, a reader largely just sees the effects of the longterm trend in each estimate.
- Figures 13 to 15 could be merged in a single figure with subplots. I don't think the results need to be supplied in multiple units per region as it appears the averaging areas for each GRACE estimate are similar.

- The manuscript is not particularly well cited. Some of these procedures have been used before and some of these findings have been found before. A more thorough discussion about where this work fits in context of other studies would be beneficial.

3 Line-by-line comments

Page 1, Lines 1–2: The attribution portion of the first sentence is overly complex. I suggest removing “result of the changes in the complex ice-climate interactions that have been driven by global climate change”

Page 1, Line 5: Remove “Firstly, in agreement with previous estimates"

Page 1, Lines 6–7: I would mention the SMB-D estimate since you mention it is consistent with your GRACE results.

Page 1, Lines 9: The acceleration for SMB is mentioned here but not the acceleration in the GRACE ice mass estimate.

Page 2, Lines 1–2: I would suggest “The NASA/DLR Gravity Recovery and Climate Experiment (GRACE) mission is a powerful tool to monitor ice mass variations in Greenland, both the ice sheet (GrIS) and its peripheral glaciers.”

Page 2, Lines 2–6: I suggest breaking up this sentence. “The total mass balance of the ice sheet represents the summation of processes in Equation 1: surface mass balance (SMB), ice discharge (ID) and en-glacial and sub-glacial mass variations (Δm). GRACE ice mass balance is calculated after removing the impacts of Glacial Isostatic Adjustment (GIA), atmospheric and oceanic variability, and other time-variable geophysical processes.”

Page 2, Line 15: I would remove “so that they have to be monitored on a regular basis”

Page 2, Lines 16–17: Spatial resolution of the monthly solutions is worse than the long-term trend?

Page 2, Lines 21–23: I would mention some regional studies of seasonal ice discharge or surface velocity change.

Page 3, Line 5: I suggest using utilized instead of exploited.

Page 3, Lines 14–15: Why not compute results for the total available GRACE-period (2002–2016 for the best quality data)? Could restructure your table to have periods of overlap with prior studies.

Page 3, Lines 18–19: If I understand correctly, geocenter variations are inherently zero in the native GRACE reference frame (center-of-mass) and not really missing.

Page 3, Lines 24–25: I would cite either Alexander et al. (2016) or Velicogna et al. (2014) for the GRACE-like processing of SMB data.

Page 5, Line 26: Why is relative italicized here?

Page 5, Lines 26–27: I would include that this assumes that the ice discharge and SMB were balanced during the reference period.

Page 5, Lines 30–31: Can you be more descriptive of what you mean by this sentence? Are you saying that SMB-D can never be positive or that GRACE-SMB shouldn’t be negative? Also remove “For instance” from the beginning of the sentence.

Page 6, Lines 1–3: The inclusion of peripheral glaciers, ice caps and tundra regions has been investigated in other GRACE and SMB studies (Alexander et al., 2016; van Angelen et al., 2014).

Page 6, Line 5: I suggest "We estimate rates of ice discharge from two different datasets."

Page 6, Line 13: I would mention that you calculated monthly rates (assumed from Figure 11) of ice discharge and not simply intra-annual rates.

Page 6, Lines 13–14: Can you include a map with the locations of your 55 measured glaciers? For your sampled glaciers, what is the mixture between the 3 seasonal behaviors types described in Moon et al. (2014)? The overall seasonality in ice discharge will be dependent on this distribution.

Page 7, Lines 10-11: I would cite Jacob et al. (2012) as a reference for using 50% of the GIA signal as the estimated uncertainty. There are alternatives for calculating this number but the "back of the envelope" technique seems to be reasonable.

Page 7, Lines 15–17: I would write that you do not consider the uncertainty from atmospheric and ocean circulation as it is negligible. As written, it suggests Velicogna and Wahr (2013) were incorrect for calculating this error. Also, the seasonal and month-to-month uncertainty in these corrections is larger than the longterm.

Page 9, Line 1: As mentioned in the broad comments, I think the Ran et al. (2017) weighting algorithm is dampening some actual geophysical signal.

Page 9, Lines 16-18: What data suggests that precipitation is the component of SMB that causes the discrepancy for these two regions?

Page 9, Lines 18-21: This is suggesting that the ice discharge uncertainties from Enderlin et al. (2014) are underestimated for the region.

Page 9, Lines 31–33: Do the annual regressions include seasonal terms?

Page 10, Figure 3: Although the plots are relatively busy as is, it may be helpful to include the cumulative ice discharge change since you compare it with your GRACE-RACMO2.3 results.

Page 11, Table 2: 8 Gt/yr is a relatively large discrepancy between your weighted and unweighted results. Could you explain the difference? From Figure 2, the difference between the estimates doesn't seem that large over the longterm.

Page 11, Line 8: I would include percentages for the individual drainage systems (12 Gt could be relatively impactful).

Page 11, Lines 9–10: I would cite that the GRACE-like processing of SMB data is similar to Alexander et al. (2016) or Velicogna et al. (2014).

Page 12, Figure 4: The lag between GRACE and SMB that is shown here is quite different than the results in Alexander et al. (2016) and van Angelen et al. (2014). van Angelen et al. (2014) suggested the lag was 18 days, but here it is approximately 2 months. The results shown here suggests that the total mass begins changing 2 months before the onset of melt. Do you have a suggestion about what would cause this?

Page 12, Line 1: Minor comment: I suggest GRACE-SMB residuals versus Total-SMB residuals.

Page 12, Line 3: Minor comment: it would be GRACE errors and the correction uncertainties used in producing the ice mass estimates

Page 13, Figure 5: This should be probably be subplots as differentiating between 12 distinct lines is difficult.

Pages 13–14: While important for validating your results, I think the GRACE testing (different sets of eigenvalues, different processing centers and different geocenter estimates) should be moved into supplementary material.

Page 14, Line 4: Remove “To make the investigation even more comprehensive”

Page 14, Line 8: Remove “Obviously”

Page 14, Line 9: I’d go with “between GRACE estimates” versus “from case-to-case”.

Page 14, Line 8: Remove “For instance”

Page 14, Lines 17–18: See the first broad comment about the weighting algorithm.

Page 15, Lines 1–2: You mention the amount of meltwater subject to runoff, but the SMB outputs should already include a portion of refreezing and retainment within the firn and snow layers. Would it be better for these results to add back the refreezing estimates to the SMB results to get the full en-glacial/sub-glacial retainment? A figure comparing the results with the modeled meltwater refreezing and retainment from the climate model could be beneficial.

Page 16, Lines 1–2: I suggest something like “Estimates of non-SMB mass anomalies could reflect the delayed release of meltwater into the ocean and the variability of ice discharge. We test the effects of ice discharge variability using a monthly-resolved dataset of ice discharge for 55 glaciers in Greenland. These glaciers are largely located in the NW and SE Greenland DS’s, which are the largest contributors of ice mass wastage into the ocean.” Then mention the number of different Moon et al. (2014) seasonal types within this dataset. As written, it seems that the variability in ice discharge is a negligible contributor to total mass seasonality, which might be too strong of language.

Page 16, Lines 6–9: Do you think the seasonality of glacier discharge would scale similarly to the mean fluxes?

Page 18, Lines 6–7: Should include error bars on these estimates. Uncertainty in both GRACE and SMB is large enough to justify a larger range than 0.3–2.0 Gt.

Page 18, Lines 7–8: Should cite Joughin et al. (2008) or Joughin et al. (2012) about the seasonality of Jakobshavn Isbræ. These large seasonal amplitudes are a relatively longterm observation.

Page 19, Figure 10: Is there a reason why the uncertainty estimate reduces from approximately 50 Gt for GRACE-SMB in previous plots to 23 Gt here?

Page 22, Lines 1–2: replace "mass anomalies are consistent within the error bar" to "mass anomalies are consistent within error bars".

Page 22, Lines 2–3: I suggest "Most of the observed acceleration in ice mass loss can be attributed to changes in SMB."

Page 23, Line 3: I suggest "Seasonality in ice discharge is on the order of a few Gt and is relatively negligible compared with meltwater retention."

Page 25, Lines 5–6: I suggest "The method is adapted from the computational procedures proposed by Forsberg and Reeh (2006) and Baur and Sneeuw (2011)."

Page 25, Line 8: Replace "The goal of the first step" with "1)"

Page 25, Line 9: Remove "For brevity, they will be referred in the following as "gravity disturbances"."

Page 25, Line 10: Remove "In parallel"

Page 25, Line 11: Replace "In the second step" with "2)"

Page 26, Lines 4–5: Remove "It is worth mentioning that"

Page 26, Line 7: Remove "On the other hand"

Page 26, Lines 8: Replace "in our case" with "here"

Page 26, Line 14: Remove "Note that"

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