Responses to the editor and reviewers

We are grateful for the comments provided by the reviewers. Please find below our answers in red to the reviewers' comments in black and the suggested changes in the MS main text in red.

On behalf of all authors,

Jiangjun Ran

Anonymous Referee #2

GENERAL COMMENTS

Although GRACE has been used extensively to monitor Greenland ice mass loss in the literature, the authors have carved out a nice little niche with this manuscript. They try to quantify summer meltwater retention in the ice sheet in terms of magnitude and timing. Overall this is a welcome addition to the ever widening list of cryospheric/oceanographic/hydrological processes and phenomena that can be revealed by combining GRACE results with appropriate models. It is understood that these are initial results that should be corroborated by further research. The authors allude to that more or less, when saying in section 3.2.1 that "these features should be explained either by melt water retention or by errors in SMB- and GRACE-based estimates." Indeed, the "features" would easily fit inside the error bars. However, the following robustness/sensitivity analyses make it clear that the patterns are persisting. So, I would welcome to see this work published after certain revision, nevertheless.

We are grateful for the comments from the reviewer. We also agree with the reviewer that this study presents a novel application of GRACE, and is an initial study. Future studies are necessary to investigate the issue of meltwater storage, and make it clearer.

SPECIFIC ISSUES

The abstract mentions three achievements: (1) obtaining mass loss estimates using their own methodology that are consistent with published mass estimates; (2) examining mass loss accelerations; and (3) quantifying meltwater storage. I find the first two points hardly relevant in view of the third point. Obtaining estimates that are consistent with what is known in the literature may be a good validation exercise to the authors, but hardly relevant for the reader. I am particularly suspicious of acceleration estimates given the relatively short time span of GRACE. Numerically one will always get some value and LS estimation and testing theory will tell you that this value is "significant". If, after successfull GRACE-FO launch and operation, we look at this part of the time series, say 20 years from now, we'd probably see a long-term signal that is decidedly different from parabolic behavior. Moreover, there is hardly any serious discussion of the acceleration in section 3.1.

I thus strongly recommend to remove or at least tone down the estimation aspects and the acceleration estimation.

Done. We have tried to tone down the parts of the long-term trend and acceleration, by moving a large part of text and figures related to trend and acceleration to be an appendix. In addition, we removed the second achievements (as indicated as (2) by the reviewer) about the accelerations from the abstract and the conclusion section. In addition, we also agree that the acceleration estimates cannot be blindly extrapolated onto a longer time interval and may not represent properly the ice sheet behavior at the decadal time scale, because of the large climate variability in a limited time span of data. We included a short discussion in the main text to discuss this issue, based on Wouters et al. (2013).

This recommendation includes the removal (from text and graphics) of anything to do with the non-weighted solutions. The difference between weighted and non-weighted solutions is a technical geodetic detail that might be reported elsewhere, but constitutes distraction here. I do believe that leaving all these aspects out will strengthen the main line of the manuscript.

We agree with the reviewer that the investigation of the difference of weighted and non-weighted solutions is a technical geodetic detail, and may cause distraction. However, the first reviewer asked the opposite to remove the weighted one. Actually, at this moment, we do not know which one (weighted or unweighted) leads to better quality. Therefore, we decide to keep both of them, in order to inform the readers.

I also recommend the authors to reconsider the use of the phrase "(surface) mass balance". To me it is a misnomer. Equations (1) or (2) are mass balances: a bookkeeping of inputs and outputs, sinks and sources, left sides and right sides. Individual terms should not be called "mass balance". I know that this terminology is by now ingrained in cryospheric and GRACE communities, but I consider it wrong nevertheless. At a recent international conference I heard a presentation on "extreme mass balance". The author simply meant rapid ice mass loss.

MB is sometimes called Total Mass Balance or Ice Sheet Mass Balance, and SMB is not strictly the surface mass balance but the climatic mass balance, because it incorporates sub-surface processes such as refreezing and retention. We understand and respect that there are different terminologies (see P6 in <GLOSSARY OF GLACIER MASS BALANCE AND RELATED TERMS>).

In this study, we follow the standard terminology (see < GLOSSARY OF GLACIER MASS BALANCE AND RELATED TERMS > by International Association of Cryospheric Sciences (IACS) in 2011). We have adjusted the text and Eqs. (1) and (2) accordingly.

At the same time, I have the feeling that the authors aren't clear about the mass balance equations themselves. Eqn (2) is a balance of fluxes, so SMB is a flux quantity too, say in units of Gt/yr. How about eqn (1)? If SMB and ID are flux quantities, then MB and delta-m should be, too. But delta-m is explained as a mass variation, i.e. time-variable mass (units of Gt), which is not a flux but a state quantity. And how about MB? And "melt water production" (fig. 9) sounds like a flux to me, although it is indicated in Gt units.

Thanks for pointing out this interesting issue. All the quantities in Eqs (1) and (2) are fluxs in Gt/yr. We have changed delta-m to delta-m/delta-t, to make it a flux.

In short, we have updated the manuscript to make more clear that Eqs.(1-2) refer to fluxes (in Gt per time unit). We add that
"The quantities in Eq. 1 refer to fluxes (in Gt per time unit)."; "The units are in Gt per time unit." (For Eq. 2).

As for Fig. 9 (now Fig. 5 in the revised manuscript), it shows *monthly* meltwater production, so that we believe that showing the results in Gt is fine.

TECHNICAL DETAILS
- In the abstract mass losses are reported in terms of negative numbers. A negative mass loss is a mass increase.

Thanks for pointing this out. We have changed "mass loss" to "mass variations".

- A hyphen is not a minus sign. Please write a minus sign, wherever it should be a minus sign, including in the legends and captions of graphs and in headers, etc.

Done.

- The acronym DS is not very helpful. Write out in full everywhere.

Changed as suggested.

- Red pentagrams are not very visible in figure 1. Figure 1 can certainly be improved. Explain the blue patches outside Greenland briefly in the caption.

Done. The blue patches outside Greenland are briefly explained. The red pentagrams are changed to "×". We admit, since the locations of glaciers are quite close, making it difficult to identify. (Note that those glaciers are exactly the set of glaciers discussed in Moon et al. (2014).)

- Page 6, line 25: Least-squareS adjustment. (If it were singular, there is nothing to adjust).

Done.

- Be consistent in your mathematical typesetting. Take, e.g., the symbol "f" for flux gate in eqn (3) and in the line above. In the equation there are two different letters "f" and in the line above, the "f" should be set in math italic. Similarly, in line 24 (page 6), the v should be bold-math-italic. And check the N and i in the last line of page 6.

Done.

- Several graphs show mass variation with the unit (EWH: m). That is inappropriate. The quantity is (expressed in) EWH and its unit is m.

We have changed the y-axis to “EWH (m)”.

- Page 26, Line 5: The sentence "No regularization is applied..." is preceded by an explanation of truncated eigenvalue decomposition. Now that is definitely regularization.

We agree with the reviewer that the truncation of the eigenvalues is also a kind of regularization. However, it was the error covariance matrix subject to the truncation, and not the normal matrix. What we mean with the statement “No regularization is applied...” is that there is no spatial regularization applied to the normal matrix in the lease-squares adjustment. We have made it clearer in the main text.