Response to Reviewer 1:
Interactive comment on "Greenland Ice Sheet seasonal and spatial mass variability from model simulations and GRACE (2003-2012)" by P. M. Alexander et al.
Anonymous Referee #1

The manuscript is a valuable contribution on our current degree of understanding mass changes of the Greenland Ice Sheet on regional spatial scales and seasonal temporal scales. Mass variations derived from GRACE Level-1 data by the mascon method of Lutchke et al. are compared to modelled changes due to SMB and ice flow based on the MAR Regional Climate Model and the ISSM Ice sheet model. One of the merits of this work is the comprehensive explanation and illustration of the complex filtering associated to the GRACE mascon results and of the way how the GRACE-versus-modeling comparisons account for this filtering.

The manuscript is very well structured and well readable despite the technical nature of part of the discussion. The figures are excellent.

We thank the reviewer for valuable comments and taking the time to read the manuscript carefully.

I have just a few points.

An important point concerns the calculation of the seasonal cycles shown in many figures and introduced on p. 6360, line 4ff. It is unclear why (and how) a two-year composite seasonal cycle was constructed. Why not a one-year composite cycle? How does the two-year cycle relate to the one-year plots?

We thank the reviewer for raising this point, which ultimately should improve the manuscript. We created a one-year average seasonal cycle for all regions as seen in all figures. In order to compute the maximum and minimum peaks in the cycle, a two-year cycle was created from the average one-year cycle in which the one-year cycle was repeated, with the second year beginning at the mass value of the previous year. This two-year "wrapping" of the cycle was deemed necessary because the first and last values of the average seasonal cycle are not identical, as the reviewer has noted below. This can occur as a result of non-linear variations in mass across multiple years, which are not removed when we de-trend the cumulative timeseries. In this instance, the change in mass during the winter months must be examined in the context of a "hydrological" year, which spans the winter months.

However, we now believe that this two-year wrapping is unnecessary because it is not necessary to constrain the values for December 31 and January 1 to be equivalent, as the average cycle reflects the average fluctuations across the entire period examined. Therefore, a wrapped seasonal cycle should return to the same mass value at the start of the second year. We have therefore simply computed the maximum and minimum values from the one-year average cycle, and have adjusted all figures accordingly. This change, along with the addition of ±10 days in error bars to account for errors associated with temporal resolution, have caused the timing of seasonal cycles and uncertainty ranges to shift somewhat. As a result the GrIS sub-regions derived from the timing of seasonal cycles are also slightly different (Fig. 11a). We slightly adjusted the threshold on seasonal cycle timing from 30 to 34 days (Section 3.4) to produce a similar number of ice sheet sub-regions with distinct patterns of mass change. There are now nine sub-regions rather than eight, but the clustering of the regions is similar, with similar patterns of mass change. All figures that depict mass changes within the sub-regions have been updated, and portions of the results section have been adjusted in accordance with the changes, but our results and conclusions regarding the timing of mass changes in different regions have remained the same.

Seasonal cycles shown in plots like Fig. 8b, Fig 11d,e etc. sometimes show very different values at the left end and the right end of the plot, although both values are to represent Dec. 31 and Jan 1, respectively. Since the paper is on the seasonal cycles, it is important that the way of deriving these cycles be explained in more detail.

Further details have been added in the text (Section 2.4.4) discussing how the seasonal cycle is calculated and analyzed, as noted above. The method of deriving the seasonal cycle is fairly simple, however, and has already been noted in Section 2.4.4. We simply take the cumulative mass timeseries, interpolated to daily timesteps, for the region being examined, remove the 2003-2012 linear trend from this timeseries, and for each day of the year, take the average of the cumulative mass value for this day across all years. The values on January 1 and December 31 can be different due to non-linear fluctuations in the original timeseries. In particular, the high mass-loss year of 2012 likely reduces the average values towards the end of the year within some basins.

As we are primarily interested in comparing the models with GRACE-LM, we do not attempt to correct for non-linear interannual mass variations, which would be difficult to separate from seasonal variations. Non-linear variations in mass, and interannual variations in the seasonal cycle may contribute to some of the differences between GRACE-LM and the models, as we have noted in Section 4. For the purposes of this study we are mainly interested in identifying discrepancies between the models and GRACE-LM with respect to the average cycle across all years.

Section 2.4.4 has been updated as follows:
"We examined differences between the modeled and GRACE-LM seasonal cycles of cumulative mass change by first linearly interpolating filtered cumulative model and GRACE-LM timeseries onto daily timesteps. This was necessary because the GRACE-LM timesteps are not evenly spaced, and do not occur at the same point in time every year. We then subtracted the long-term linear trend for the entire timeseries

(2003-2012) obtained from least-squares regression, to remove the impact of differences in trends on the timing of the seasonal cycle. After removing trends, the cumulative mass value for a given day of the year was averaged across all years in the 2003-2012 period, to yield an average annual cycle for all years. The maximum and minimum peaks were computed from this average annual cycle. This was performed for the GrIS-wide timeseries, as well as for individual mascons and GrIS sub-regions."

The authors do an excellent job in describing the complex filtering inherent to the GRACE mascon solutions. The figures illustrate that the GRACE processing may, to some degree, distort (not just smooth) the spatial patter of signals. Most remarkably, Fig. 1 illustrates that the partitioning of GRACE mascons into mascons below and above 200m elevation does not precisely match the limits between distinct regimes of modelled SMB and dynamically induced mass balance. The authors could somewhat more account for these limitations when discussing the GRACE-versus-modeling results later-on in the manuscript.

The GRACE-LM processing does not involve "filtering", in the sense that observations of mass change are spatially smoothed or modified through processing. Rather the processing serves to estimate the mass changes within individual mascons given the observed KBRR data from the GRACE satellites. The resulting derived pattern of mass changes is in a sense spatially smoothed and distorted because the mascons are spaced at a distance that is smaller than the fundamental spatial resolution of GRACE. For this reason, it is necessary to apply a filter to our model outputs for comparison with the GRACE solution.

The objective of using the constraint regions is to reduce the "smoothing" effect to concentrate ice sheet mass loss into areas where it is known that mass loss is occurring. Certainly the 2000 m elevation boundary is not a perfect dividing line between high and low elevations, and future GRACE solutions may employ more complex methods to better capture spatial variability of mass changes. We conduct filtering on model results for the purpose of comparing the models with GRACE subject to the same kind of spatial patterns from GRACE-LM and are more interested in the comparison between GRACE-LM and models filtered to match the GRACE solution. But we agree that in the cases the reviewer has mentioned there should be further clarification, and have reiterated that model results being compared with the GRACE-LM solution are filtered, and therefore the model results are spatially smoothed and to some extent, distorted.

For example, on p. 6367, they write: "the timing of GRACE-LM peaks tends to be clustered in groups, suggesting that the spatial variations in GRACE-LM timing are not random." It could be discussed whether the observed clustering could be a consequence of the GRACE-LM filtering effect, even if its actual origin is "random".

Indeed, one would expect clustering in the GRACE results, given that changes within two adjacent mascons are influenced by mass changes occurring in overlapping

regions. This is a function of GRACE-LM spatial resolution. The regularization matrix used in the GRACE-LM processing constrains nearby mascons to exhibit a similar signal. However, there are differences between adjacent mascons, and such variations could potentially lead to differences in timing between mascons. The fact that there is a widespread discernable signal in the GRACE data suggests that there is a real signal in this region that is large enough to be detected by GRACE. We leave open the possibility that processes not related to ice sheet mass change, not accounted for in the GRACE processing could lead to the observed differences, although it is unclear what these processes might be. We discuss this further in response to the reviewer's comment about p. 6368, l. 28.
The sentence has been revised to read:
"The clustering of the GRACE-LM peaks, despite the large uncertainty in the GRACE timing, suggests that the observed variations in timing are not associated with random deviations between mascons, but reflect seasonal variations in mass detected by GRACE-LM, that are not captured by the models."

Likewise, when discussing the GRACE-versus-modeling differences in the zone above 2000m (Fig. 12b) it could be pointed out that these differences could well originate from modeling errors for regions *below* 2000m (given much higher signal amplitudes there), which may leak into the high-elevation results.

The purpose of the constraint regions is to minimize the leakage between areas above and below 2000 m to produce a more realistic signal for areas above 2000 m in elevation. Leakage across the constraint regions is therefore small; the impact on annual amplitude of the signal is on the order of the magnitude of estimated error (Luthcke et al., 2013). It is possible that the Gaussian filtering overestimates leakage, although the good agreement between GRACE-filtered and Gaussian-filtered model outputs (Fig. 3) suggests otherwise. We have noted that leakage from other regions may contribute to the differences, but that we expect the impact is small.

The end of Section 3.4 has been modified to take into account the potential influence of lower elevation changes on higher elevation fluctuations, and the following sentence has been added:
"Accumulation or ice flow errors could also affect differences at higher elevations, where the net ablation due to melting is small (i.e. above 2000 m in elevation). Such discrepancies could also be influenced by differences below 2000 m due to leakage between constraint regions, but the amount of leakage in terms of amplitude is small and is comparable to the GRACE-LM uncertainty (Luthcke et al., 2013)."

p. 6369, line 4f: It is not clear to what result or figure the "early start to the period of mass loss in the northeast from November through February" refers. Similarly, on p. 6732, it is not clear to what result the mention of "northeast Greenland" refers.

We meant to refer to "northwest Greenland" rather than "northeast Greenland". "Northeast" has been changed to "northwest" in both cases, and we now refer to Fig. 9a-c on p. 6369.

Minor points:

I was initially confused about the use of MAR v2.0 versus MAR v3.5.2. Maybe it could be mentioned at an early place that the comparison with GRACE is ultimately done for v3.5.2, while MAR v2.0 is used to assess different filters because the numerically most expensive filter was previously applied to v2.0 but not to v3.5.2.

We have added an explanation about this at the end of Section 2.2, where the MAR model outputs are introduced.

p. 6357, line 11 "A different sigma_i value is chosen for each mascon": Maybe add "as explained below", to keep the reader patient about an explanation.
We have added the phrase "as will be explained further below." to the end of the sentence, as suggested.

There is an unnecessary repetition about how lambda_ij are define, before and after Equation 8. Instead you could add "as explained below" again, to keep the reader patient about the mystery of these coefficients.

Agreed. The text has been changed as suggested.

p. 6358, line 21: Symbol sigma_I was not introduced before. Please homogenize annotation.

We incorrectly used an uppercase "I" here. It's been changed to a lowercase "i".

Line 6360 last line. For better clarity, write "GRACE-LM *filtering* vs. Gaussian filtering"

This has been changed as suggested.

p. 6364, line 17ff. It is not clear why the discussion concentrates on the region where ISSM underestimates ice thickness and it cannot be seen from any figure that ISSM underestimates ice velocities at these places.

Figure S4b suggests that ISSM underestimates ice velocities for glaciers along the northwest coast of the GrIS. These the thicknesses along the coast at these glaciers appears to be overestimated (Fig. S4b), but the thickness upstream of the glaciers is underestimated, possibly contributing to the underestimated velocities. We have added further discussion of spatial variations in differences between ISSM and observations, and how this may relate to the differences between ISSM + MAR v3.5.2 and GRACE on p. 6364:

"In particular, ice velocities tend to be underestimated for glaciers along the northwest coast of the GrIS (Fig. S4b), possibly as a consequence of an upstream ice thickness that is also underestimated (Fig. S4a). This may contribute to underestimated mass loss along the northwest coast. In other areas, ice thickness is generally overestimated by ISSM, but some outlet glacier velocities are overestimated while others are underestimated, making it unclear how ISSM contributes to the observed discrepancies in these regions."

p. 6364, line 23. Avoiding the SSA acronym (used at only one occasion) would make the text more readable.

We have replaced the SSA acronym with the "Shelfy Stream Approximation" for clarity and have removed the SSA acronym from Section 2.3.

p. 6370, lines 3-4: Please clarify. It is not clear to me what "it" and "The Greenland-Wide cycle" refer to.

We agree that the sentence was confusing. We have replaced the sentence with the following:

"As the filter extends the length of the modeled period of mass loss, and tends to bring the timing of modeled seasonal cycle peaks closer to those from GRACE-LM (which exhibits a longer period of mass loss relative to the unfiltered model results), our approach is conservative: in cases where the cycles disagree, there is likely a difference between the GRACE-LM and modeled seasonal cycles."

Fig. 4 Caption "a temporal filter has not been applied": The legend within the figure, instead, says "Gaussian(Spatial*, Time*) Filtered" for one of the curves.

The sentence in the caption was incorrect. It has been changed to read:

"Timeseries are shown for Gaussian filtered MAR v2.0 outputs subject to only spatial filtering (gray curve) and both spatial and temporal filtering (blue curve)."