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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 #2

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Review of paper by Alexander et al.

General comment

The paper by Alexander et al. entitled "Greenland Ice Sheet seasonal and spatial mass variability from model simulations and GRACE (2003-2012)" employs surface mass balance from the regional climate model MAR v3.5.2, and the ice sheet model ISSM, to obtain a good representation of the overall Greenland Ice Sheet Mass Balance changes between 2003 and 2012. The authors process model outputs performing a spatio-temporal filtering in order to make a fair comparison with mass changes obtained from the GRACE data using the mascon strategy developed by Luthcke et

al. (2013). They find a quite good agreement over the entire ice sheet and several sub-regions. Some discrepancies remain to date unexplained.

The paper is well written and illustrated. Succeeding in comparing GRACE and model outputs, the paper is a significant step towards the understanding of mass changes over the Greenland Ice Sheet but also at a more regional scale which is fundamental to understand the overall ice sheet system and its response to climate change.

Specific comments

p. 6359 - l. 26-27: "the Gaussian filtering procedure does not incorporate changes in mass". It looks to me that it is not entirely correct, or I may have misunderstood the filtering strategy. Indeed, the parameters sigma i, lambda i and sigma time seem to be determined using "the aggregated unfiltered MAR v2.0 data". However, leakage is usually a function of the mass change in mascon i and mascon j. So I wonder why the parameters mentioned above would not change using the aggregated unfiltered MAR v3.5.2 data given the differences mentioned in the manuscript between the two versions of the model. The entire GrIS MB may not significantly be affected because the differences between v2.0 and v3.5.2 of MAR may only result in the spatial distribution of the MB but not the total MB of the GrIS. However, for each mascon, MB might change from one model to another. Can you clarify this up?

p. 6360, l. 6: "to a daily temporal resolution" is quite not correct, you are not increasing the temporal resolution, you are just increasing the number of time sample which leads me to the question, can you really give in the following sections of your manuscript seasonal timing at a daily/weekly (p. 6365, l. 21: "roughly 1 week earlier") accuracy given that GRACE-LM and GRACE-like filtered models have a time resolution of 10 days?

p. 6360, l. 8: "two-year composite seasonal cycle" why two years ? need justifications here.

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p. 6361, l 2-8: As you say, "it is not possible to compare GRACE-derived mass changes directly to MAR" however you can compare MAR v3.5.2 with ISSM, so why not doing it earlier so that your assumption regarding the ice discharge changes can be justified?

p. 6368, l. 3: It looks to me that the GRACE-LM peak occurs also in late February? Is the difference significant to take the November peak? Would it change your interpretation?

p. 6370, l. 28: What type of GRACE errors might influence the variability in that context? Are, for instance, loading effects of surface mass changes of the GrIS and atmospheric changes taken into account when inverting for the KBRR data? Could it have an important impact on the signal, e.g. the amplitudes? Is the atmospheric model used in the GRACE-LM processing strategy the same as in the SMB model?

Technical corrections

p. 6347

l. 4f: "While several studies...are still lacking." A short sentence to explain why it is important to examine sub-annual and sub-basin-wide changes would be nice here.

p. 6349

l. 6-9: MB in l. 6 only defined in l. 9.

p. 6350

l. 4: you should precise the starting and ending months of the period of interest at least once but best everywhere the period appears.

l. 15: "between GRACE-derived and simulated" -> would estimated be better than simulated here?

p. 6352

l. 4-5: you should precise the starting and ending months of the two mentioned periods.

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p. 6353

l. 19-23: Following these lines I expected some results of the evaluation and the comparison just mentioned. Instead it comes much later (p. 6364). May worth to modify a little bit the text here for instance by moving these lines to p. 6364.

l. 24: Is the period 2003-2013 correct? Isn't it 2012? can you also precise the starting and ending months please ?

Could be nice for clarity to summarize that to represent GrIS MB and given the ISSM forcing you have to combine ISSM with MAR v3.5.2 and not MAR v2.0.

p. 6354

l. 1: "at a high resolution" -> temporal resolution? spatial resolution? both? Last sentence: I don't understand this sentence here. Isn't the job done by MAR and ISSM models? Or may be the sentence should be moved to the begining of the paragraph as an introduction?

p. 6356

l. 2-4: Any reference for the 2000 m elevation limit between positive and negative MB?

p. 6357

l. 1: Explain x and mu.

l 21-22: It looks to me that this sentence is a repetition of lines 16-18. Is there a physical meaning/interpretation for lambda i?

p. 6358

l. 12: explain t0.

l. 21: "iteratively adjusted" using the least squares method? Also sigma I or I is not explained or present in formulas (same in p. 6359 l. 1). How was the iteration initiated and modifications performed?

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p. 6359

l. 1: what was the initial values of sigma i, sigma time and lambda i?

p. 6360

l. 5: You should precise the type of interpolation you use here.

l. 6: please explain how you remove the linear trends (least squares adjustment, moving window average...)

p. 6352

l. 1: Would "Additional temporal Gaussian-filtering improves the agreement between the spatial Gaussian-filtered..." be better ?

p. 6363

Don't you have uncertainties on your trend estimates?

l. 17: "2000-2012" isn't it "2003-2012"?

p. 6364

l. 1-3: Consistency -> -150-30 = -180 Gt/yr different to -179 Gt/yr in l. 14, p. 6363. Same for -242+3 = -239 Gt/yr w.r.t -240 Gt/yr.

l. 4-end: It looks like only models can be wrong, what about GRACE-LM ? Are you accurately taking all geophysical effects into account?

p. 6365

l. 7-8. Can you suggest examples for independent evaluations of the models?

p. 6368

l. 1: "the maximum modeled mass occurs" -> "the maximum modeLled mass CHANGE occurs"?

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p. 6369

l. 15: Can you give examples of non-ice-sheet-related processes that may contribute to the discrepancy?

l. 17: I would change "Discussion and conclusions" to "Concluding remarks".

l. 23-25: "We also find that ... this effect." What about something like "We also applied a temporal Gaussian filter to the models to reproduce the attenuation inherent to the GRACE-LM processing strategy."?

p. 6370

l. 13-14: How many in situ stations? How many SMB measurements are available?

l. 19: Any suggestion for conducting independent evaluation of each model?

l. 27: "represented" -> observed? "results" -> predictions?

p. 6372

l. 8: "and may transition between"?

fig. 3: RMSD -> RSME?

fig. 5 and where it appears: errors bars are not that easy to identify.

fig. 6: can't see the pink shading surrounding the GRACE time series. 2003-2013? be consistent with the manuscript where it is mentionned 2003-2012.

fig. 9: 2003-2013 or 2003-2012? In b), missing an x in maximum. It would be easier for comparison purpose to have the same colour scale in all figures.

fig. 10: "and blue colours indicate AND earlier date" -> typo?

fig. 12b: Could satellite altimetry data help assessing the difference between models and GRACE data?

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Interactive comment on The Cryosphere Discuss., 9, 6345, 2015.

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