

Interactive comment on “Improved GRACE regional mass balance estimates of the Greenland Ice Sheet cross-validated with the input-output method” by Z. Xu et al.

Anonymous Referee #1

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1 Overview

Xu et al. use a constrained inversion to produce a new GRACE-based mass balance estimate for the Greenland ice sheet. The estimate is compared with estimates from the mass budget method (input-output IOM) combining ice discharge with surface mass balance and with estimates from laser altimetry. The authors investigate different methods for calculating ice discharge for use in the mass budget approach. A primary objective is to constrain the ranges of uncertainty from GRACE and IOM. An interesting result is the partitioning of mass balances between the interior and exterior of each drainage system.

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There are a number of issues that should be resolved before I could recommend for publication:

2 Broad comments

- There are some structural issues that when resolved could make the paper more concise. The introduction should have a more high-level description of the overall problem. The highly detailed information about the GRACE and IOM methods should be in their respective data and methods rather than the introduction. This might reduce some overall redundancy.
- There may be issues with the coastal versus interior derivation in the IOM section
- The analysis at current seems circular (constrain GRACE with IOM and then compare with IOM)

3 Line-by-line comments

- On page 4663 line 3: Andersen et al. (2014) is cited but not in the references. Is this supposed to be Andersen et al. (2015)?
- On page 4663 line 16: perhaps it would be better to list the regional climate model resolutions in kilometers rather than degrees?
- On page 4663 lines 19-22: the sentence regarding the regional balance fluxes could be reworked (e.g. estimate missing D estimations). Perhaps something along the lines of: “For the IOM in regions missing fluxes from ice discharge, the

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mean SMB from 1961–1990 is used as the reference D assuming that the ice sheet is in balance over the period.”

- On page 4664 line 3: GRACE level-2 data is available from April 2002 (not the end of 2002).
- On page 4664 lines 3-5: the sentence regarding the conversion between GRACE spherical harmonics and global maps of surface mass density could be reworked. Perhaps also cite Wahr et al. (1998) in this sentence as per other GRACE time-variable gravity studies.
- On page 4664 line 10: I assume this is referring to the constrained inversion approach, but this is the first mention since the abstract. Perhaps something along the lines of: “Here, we employ an inversion approach to estimate the mass balance of sub-regions of the Greenland ice sheet from GRACE time-variable gravity data.”
- On page 4664 line 15-18: Just a comment: signal leakage has been a documented GRACE problem in both traditional regional averaging approaches (Swenson and Wahr, 2002) and post-processed mascons approaches (Tiwari et al., 2009) for some time before the Bonin and Chambers (2013) results. From my understanding, there are two distinct types of leakage: geophysical from processes not within the study (e.g. hydrology) and statistical (leakage of mass within or out of the system of mascons). Bonin and Chambers (2013) investigated how the statistical leakage component varies using different kernel designs, but the overall leakage problem was documented prior.
- On page 4664 lines 27-29 - page 4665 line 1: I had to read this sentence a few times to try to decipher the meaning. Is this about the relative contributions of SMB and D to the annual mass balances? Is there a figure showing these results?

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- On page 4665 lines 9-10: this is the first detailed mention of the least-squares inversion method with a citation. The method specifics and citation should probably be with the aforementioned (and possibly reworked) “By employing the inversion approach” sentence on page 4664 line 10.
- On page 4665 lines 15-17: this is currently not a grammatically valid sentence.
- On page 4666 line 19: I think this should be in kilometers rather than degrees.
- On page 4667 line 12: I might note that the empirical scaling factors are calculated using observations at fully surveyed glaciers, or note “as derived in Enderlin et al. (2014)”.
- On page 4668 lines 16-24: If mentioning methods used in GRACE within the data and methods of the IOM, perhaps the GRACE methods should be listed first.
- On page 4669 equation 6: The purpose of using a reference SMB and D for the cumulative SMB-D anomalies is not well explained. Is this just for regions where discharge is not known? GRACE should sense the cumulative SMB-D anomalies, and it is not fully reasoned why these reference periods are needed.
- On page 4670 equations 7 and 8: There should be F_{2000} fluxes in these equations or else mass will not be conserved. With your assumption $\delta F_{2000} = 0$, but $F_{2000}(t) = F_{2000} + \delta F_{2000}$ and F_{2000} is not 0.
If $(SMB_0^{up} = F_{2000})$ and $(SMB_0^{down} + F_{2000} = D_t)$:

$$\begin{aligned} \delta TMB^{up} &= SMB_0^{up} - F_{2000} + \int_{t1}^{tn} (SMB_t^{up} - F_{2000}) dt \\ &= \int_{t1}^{tn} (SMB_t^{up} - F_{2000}) dt \end{aligned}$$

$$\begin{aligned} \delta TMB^{down} &= SMB_0^{down} + F_{2000} - D_t + \int_{t1}^{tn} (SMB_t^{down} + F_{2000} - D_t) dt \\ &= \int_{t1}^{tn} (SMB_t^{down} + F_{2000} - D_t) dt \end{aligned}$$

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- On page 2671 lines 1-4: for this Monte Carlo approach, are there the same number of common months in each 20 year averaging period (i.e. 20 Januaries, 20 Februaries and so on)? If not, variations in annual SMB could impact the mean if a particular season was over sampled.
- On page 2673 lines 22-24: rather than “(associated with geocenter loading)”, I would replace with “(related to the motion of the Earth’s geocenter)”.
- On page 4673 lines 24 - page 2674 lines 1-3: there is a plurality problem as currently written (starts singular and ends plural). Perhaps: “The geopotential flattening coefficients calculated using GRACE data are less accurate than those from Satellite Laser Ranging (SLR) measurements. We replace these coefficients with the ones from Cheng et al. (2013).”
- On page 2674 lines 6-15: 2 sources of leakage: geophysical and statistical. The geophysical leakage from components outside the region of interest or from phenomena not of interest are removed using model results as you mentioned (either in the GSM processing stage or in the post-processing stage). With statistical leakage, the mass variation is leaked between mascons by signal misfit or by kernels being malformed.

With that, the size and shape of the mascons used in this analysis might be pushing the GRACE resolution (particularly 4ab and 5ab). Is there a possibility of calculating sensitivity kernels in the form of Jacob et al. (2012)? This would allow you to test the spatial sampling of the inversion. If the kernels are malformed, then the misfit results found in this analysis could be due to ringing, reliance on noisier high degree and order harmonics, or missampling of the averaging area.
- On page 2674 line 16-18: the Paulson model has been updated as of 2013 (A et al., 2013). Is this the model used in the analysis?

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- On page 2675 lines 20-23: is this saying that rather than treat the statistical misfit as an error, you are treating it as a correctable bias? If this is the case and you are using IOM as the constraints, are you creating a circular analysis by then comparing with IOM?

Following Tiwari et al. (2009), can you calculate what you recover using GRACE-(corrected retrieved results) with your mascon algorithm? If the problem was simply due to non-uniqueness of the solutions, then the new recovered numbers should all be approximately 0. If not, then the GRACE estimates could possibly be no longer unique from the IOM solutions.
- On page 4680 lines 2-4: differences between ICESat estimates are complicated. Could also be due to the firm correction and density conversion, the interpolation scheme, the elevation change method (crossover versus along-track versus overlapping footprints), etc.
- On page 4680 lines 7-9: ICESat-only estimates are only available from 2003–2009. It wouldn’t make sense to compare with a GRACE method over the longer 2003–2013 time period as the trend in Greenland is not stable.
- On page 4681 lines 9-12: What do you mean by “becomes similar?” Within errors of the GRACE results?
- In table 1: missing a parentheses on the line for Barletta (2003–2012)

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