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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 #3

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Major

1. Regressing meltwater runoff and ice discharge anomalies – I believe this type of regression is usually done with absolute runoff and discharge values (rather than anomalies), and I am unsure of motivation for doing it with anomalies from a (ultimately) arbitrary "normal" period. Also, the correlation with "four-year average runoff", presumably that is a lagging four-year correlation? Perhaps it would be good to put that in context to the analogous 5-year and 13-year lagging correlations of Bamber et al. (2012; GRL) and Box and Colgan (2013; J. Climate).

2. The "cross-validation" between GRACE and IOM seems to ignore that IOM should

(in theory!) only be sampling mass balance of the ice sheet proper, while GRACE should be sampling mass balance of both the ice sheet and peripheral glaciers. As peripheral glaciers are believed to be responsible for almost 40 Gt/a of mass loss (Bolch et al., 2013; GRL, Gardner et al., 2013; Science), ice sheet-integrated IOM mass loss should be approximately 15 % less than GRACE mass loss integrated across the entire island of Greenland.

3. With sections of "2. IOM Method", "3. GRACE", "4. Cross-validation", and "5. Conclusions", the structure of the manuscript is a little unconventional, making it difficult for a reader to discern precisely when "methods" transition to strict "results", and "results" correspondingly give way to more wide ranging "discussion". For example, section "3. GRACE" seems to contain both methods and results. Perhaps following the more conventional presentation flow might make it easier for the reader?

4. I find the mathematical notation is difficult to follow. Part of this stems from what I think might be unnecessary use of short-hand notation (i.e. nested notation of "D^{D-08}") but also the relaxed fashion in which variables are introduced. For example, Eq. 6 is meant to show the cumulative TMB anomaly (in Gt) is comprised of reference period SMB-D as well as observational period SMB-D. While the SMB and D terms for both periods should be in Gt/a, only the latter (observational period terms) appear inside a time-integral to deliver units of Gt consistent with TMB on the left-hand-side. I would have benefited from clearer equation presentation and a table of annotation that provided the units for each variable, to confirm that notation such as "SMB" is not variously convoying Gt and Gt/a quantities.

5. Section 2.4 – Spatially interpolating IOM mass balance values to the entire ice sheet is very novel, but receives very little description. I would think that "spatially interpolating" basin-specific IOM-derived mass balance values should yield unique, but spatially uniform, specific mass balance values (i.e. mass balance per unit area) in each basin. A figure of the spatially interpolated IOM mass balance values would be very helpful to understand if this is indeed happening, or if interpolated values are not

spatially uniform within a basin, how they are being distributed on a spatial resolution below their native basin-scale resolution?

6. I am not sure if replacing some GRACE spherical harmonic degrees with independently estimated values (i.e. C10, C11, S11, C20) is a conventional practice. I would be keen to see an explicit description (and citation) of when/why this has been done before, as well as the potential sensitivity of the ultimate cryospheric-mass loss solution to replacing these spherical harmonics. My sense is that most groups analyze the entire D/O 60 GRACE data, for better or for worse, and I am not sure if this is necessary to maintain internal consistency amongst the spherical harmonics.

7. The appendices seem small in proportion to the methods within the main body of the manuscript, so it is not immediately clear to me why the appendix material has been removed from the main body. I would think these extra few paragraphs of material could be merged into the main body, so that the reader is presented this information at more relevant opportunities.

Minor

1. "Colgan et al. (2014)" should be updated to: Colgan et al., 2015. Hybrid glacier Inventory, Gravimetry and Altimetry (HIGA) mass balance product for Greenland and the Canadian Arctic. Remote Sensing of Environments. 168: 24–39.

2. Instances of multiple references are currently listed in alphabetical order. I believe EGU journals may use chronological order in such instances.

3. Consistency on abbreviation choice, such as "Sect. 2" (P4666L3) vs "section 3" (P4666L8) or

4. Presumably RACMO "version 2.3", or is it really version 3?

5. P4669L26 – This interior thickening rate has been superseded by: Colgan et al., 2015. Greenland high-elevation mass balance: inference and implication of reference period (1961–90) imbalance. Annals of Glaciology. 56: 105-127.

6. P4671L3 – Are "months" really randomly sampled in the Monte Carlo, or is it supposed to be years? Is months are indeed being randomly sampled, presumably there is mechanism to maintain seasonally representative sampled (i.e. not overweighting a particular Monte Carlo simulation with months of a given season)?

7. P4679L18 – A spatial plot of this acceleration might be helpful to illustrate which drainage sectors it most influences.

8. P4683L18 – Do you really use 11 models of GIA, or rather 11 simulations derived from a smaller number of models?

Interactive comment on The Cryosphere Discuss., 9, 4661, 2015.