We thank again the reviewer for the thorough review and detailed comments.

General comments 1: *"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".*

Response: Thank you for pointing it out, we agree with the "redundancy" problem in the introduction. We intended to briefly introduce GRACE and the IOM method however as you have commented, the content is indeed too detailed.

Changes: We rewrote the section from P4663 L6 to P4664 L20, it is now as follows:

To quantify recent changes in GrIS mass balance, three methods are used: satellite altimetry, satellite gravimetry and the input-output method (Andersen et al., 2015; Colgan et al., 2013; Sasgen et al., 2012; Shepherd et al., 2012; Velicogna et al., 2014; Wouters et al., 2013). The latter two methods are used for this study.

The input/output method (IOM) evaluates the difference between mass input and output for a certain region. It considers two major mass change entities, i.e. Surface mass balance (SMB) and solid ice discharge (D). SMB is commonly estimated using climate models (Ettema et al., 2009; Fettweis, 2007; Tedesco et al., 2013; Van Angelen et al., 2012), whereas ice discharge can be estimated with combined measurements of ice velocity and the ice thickness, e.g. Rignot and Kanagaratnam (2006), Enderlin et al. (2014) and Andersen et al. (2015). The total SMB and D from 1960 to 1990 are sometimes used in order to reduce the uncertainties in the mass changes of SMB and D (van den Broeke et al., 2009; Sasgen et al., 2012). However, using the reference SMB and D may introduce new uncertainties in IOM. We will discuss the details of the IOM as well as the uncertainties of the reference SMB and D in section 2.

The satellite gravity observations from GRACE (Gravity Recovery and Climate Experiment), provide snapshots of the global gravity field at monthly time intervals. which can be converted to mass variations. GRACE observations are, however, influenced by measurement noise and leakage of signals caused by mass changes in neighboring areas. Besides, the GRACE data contain north-south oriented stripes due to measurement noise and mis-modeled high-frequency signal aliasing in the monthly gravity fields. Therefore, in order to estimate the mass balance for GrIS sub-regions from GRACE data, we apply the Least Squares inversion method (Schrama and Wouters, 2011) in this study with an improved approach to obtain constraints (Xu et al., 2015). Bonin and Chambers (2013) showed in a simulation study that the Least Squares inversion method introduces errors.

General comments 2: *"There may be issues with the coastal versus interior derivation in the IOM section"*.

Response: the derivation was unclear, and we made a major change to it.

Changes: please refer to the new section 2.2 in a separate "*newderivation.pdf*" file in the attachment.

General comments 3: *"The analysis at current seems circular (constrain GRACE with IOM and then compare with IOM)"*.

Response: The analysis may appear to be circular but in fact IOM doesn't directly constrain the mass balance from GRACE. The constraints are used because we have found that in some sub-regions, the GRACE inferred mass balance can be very unrealistic. For instance:

1) On one region the mass increases by hundreds of Gt in a month, while there is hundreds Gt of mass loss in the neighbouring region.

2) In particular in the interior regions, if one area shows positive trend of mass changes while the adjacent areas always show negative trend, this maybe be due to instability in the inversions, the effect of which we dubbed 'correlation error' in Xu et al. (2015).

Therefore we used the IOM in a simulation only assuming that it is a reasonable measure of the monthly variability and the inter-region correlation of the mass changes, but not necessarily the mass balance themselves. Furthermore, we have shown that the constrained results mainly depend on the GRACE observations, please see our early study as cited in the main text, i.e. Xu et al, (2015).

Comment 1: *"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)?"*

Response: We have changed this typo in the text. It should be *Andersen et al. (2015)*.

Changes: see P4664 L3

Comment 2: *"On page 4663 line 16: perhaps it would be better to list the regional climate model resolutions in kilometers rather than degrees?"*

Response: Our concern is the consistence of the resolution unit so we prefer to present it in degrees just like for GRACE.

Comment 3: *"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 mean SMB from 1961–1990 is used as the reference D assuming that the ice sheet is in balance over the period.""*

Response: we have rewritten this part in the text.

Changes: the new text is put in, P4664, L20.

Comment 4: "*On page 4664 line 3: GRACE level-2 data is available from April 2002 (not the end of 2002).*"

Response: This is indeed our mistake. This description is now removed according to general comment 1 (to focus on our contribution).

In section 3 we mention that we use the data series starting from Jan 2003.

Comment 5: "*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 timevariable gravity studies.*"

Response: The related content is deleted. The detail of post-procssing is now only described in Section 3.1 and Wahr et al. (1998) is cited in that section.

Comment 6: "*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 wdata."*"

Response: the part of using the constrained inversion approach is written according to your general comment 1. The updates of introducing this method can be found in the changes related with general comment 1.

Comment 7: "*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.*"

Response: This is a valuable comment. In the text we intend to show the statistical component when using this method, thus only Bonin and Chambers (2013) is referenced as we also want to limit the size of the introduction.

Comment 8: "*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?*"

Response: Yes, your comment provides a much better description so we adapt it in the text.

Changes: the new sentence is "and the relative contributions of SMB and D to the annual mass balances were revealed." (P4664 L27-29)

Comment 9: "*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.*"

Response: As you have commented, we mention the Least Square inversion approach in the new content on previous page, and cite the relative paper at that place.

Changes: see the changes for general comment 1.

Comment 10: "*On page 4665 lines 15-17: this is currently not a grammatically valid sentence.*"

Response: Thank you for point it out.

Changes: the new sentence is "The GrIS drainage systems (DS) definition of Zwally (2012) is employed in order to investigate the mass balance in GrIS sub-region. This definition divides the whole GrIS into 8 major drainage areas, and each drainage area is further separated by the 2000m elevation contour line, creating the interior and coastal regions for each drainage area." (P4665 L15-L17)

Comment 11: "*On page 4666 line 19: I think this should be in kilometers rather than degrees.*"

Response: as we have explained in comment 2 we use the spatial resolution in GRACE in degree, and we convert the km resolution of the SMB model to degree.

Comment 12: "*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)""*

Response: we have added the note as you suggested.

Changes: the updated content is: "Ice flux for glaciers with centreline or no thickness estimates using empirical scaling factors as derived in Enderlin et al. (2014)." (P4667 L12).

Comment 13: "*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.*"

Response: we move this part up to the introduction (also see the changes for general comment 1) and replaced it with a simple reminder.

Changes: the sentence is replaced by "Contrary to the GRACE which measures changes in overall mass (unit in Gt), SMB, D and TMB are estimates of rates of mass change (i.e., mass flux) in Gt/month or Gt yr^{-1}." (P4668 L20-L21)

Comment 14: "*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."

Response: we add more explanation of using the reference SMB and D.

Changes: the explanation is as below:
"In the previous study of IOM, when the estimation of D is missing in some regions (Rignot et al., 2008), the 1960 to 1990 reference SMB is used to bypass the influence of the missing regional D (Sasgen et al., 2012). Furthermore, due to the uncertainties in the SMB model, if we accumulate the TMB over a long time period, it may also indicate in unrealistic mass gains or losses (van den Broeke et al., 2009). By removing the reference, the influence of the large uncertainties and inter-annual variability in SMB and D can be reduced (van den Broeke et al., 2009). This reference period is chosen based on the assumption that the mass gain from the surface mass balance during that period is compensated by ice discharge, so the GrIS was in balance (no mass change)."

Comment 15: "*On page 4670 equations 7 and 8: There should be F2000 fluxes in these equations or else mass will not be conserved. With your assumption $\delta F^{2000} = 0$, but $F_t^{2000} = F^{2000} + \delta F^{2000}$ and F^{2000} is not 0. If $(SMB_0^{up} = F^{2000})$ and $(SMB^{down}+\delta TMB^{up} =D_t)$:*

$\delta TMB^{up} = SMB_0^{up} - F^{2000} + \int (SMB_t^{up} - F^{2000}) \, dt = \int (SMB_t^{up} - F^{2000})$

$\delta TMB^{down} = SMB_0^{down} + F^{2000} - D_t + \int (SMB_t^{down} + F^{2000} - D_t) dt$

$= \int (SMB_t^{down} + F^{2000} - D_t) dt$ "

Response: Firstly, we updated the equation and notations according to another reviewer's comment. Then we added better explanations in the derivation from Eq. (6) to Eq. (7) and Eq. (8). When we introduced two assumptions about the flux cross 2000m contour, we made some mistakes which are corrected according to this very important comment.

Changes: Please have a look at our new derivation provided in the attachment (a "*.doc*" file). This new content will replace the old version of section 2.2.

Comment 16: "*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.*"

Response: We have tested a very large number of random samples, i.e. 5000 for each run, and we run the experiment 5 times. The results are almost the same. Hence we believe the over-sampled problem has limited impact on the result.

Comment 18: "*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)""*

Response: we replace the content in the bracket as you suggested.

Changes: see the updated in P4673 L23-24

Comment 19: "*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).""*

Response: the grammar errors are corrected as in your comment.

Changes: see the updated in P4673 L24 to P4674 L1-3

Comment 20: "*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.*"

> **Response**: Indeed, the kernels can influence the leakage. Bonin and Chambers (2013) have tested several different combinations of kernels, as we cited before. However, our test is based on a given (fix) mascon definition, i.e. Zwally-12, and we only implement one inversion approach from Schrama and Wouters, (2011). So in this paper, our result is aiming to show that with given kernels, one can reduce the statistical leakage by applying our method as supported by and improved comparison with the IOM for the same mascon boundaries. We prefer to leave the testing of different kernels to a future study.

Comment 21: "*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?*"

Response: We use an old version of Paulson's model (Paulson et al., 2007). But we tested the new version, it has very small influence to GrIS mass changes estimates, and the differences are within the GIA uncertainty range, which is provided in this study.

Comment 22: "*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.*"

> **Response:** It is definitely not a circular analysis, in our opinion. The explanation is list in general comment 3. To summarize, the inversion results mainly come from

the GRACE, only 1) we used an a priori variance of the IOM but not the actual values as the constraints, 2) the inter-region correlations are constrained by the ones in IOM. A more detailed explanation can be found in Xu et al., (2015).

In the early phase of this study, we also worried about the uniqueness of the correctable bias. Our solution is to perform a large Monte-Carlo test (1000 sample size). In this test, we randomly alter the IOM model on the spatial domain, and we find that the corrections (linear correlation) for the bias are similar for each trial. We think the approximation error (or the regional bias) is not random (for the coastal region), but as we demonstrated in section 3.3, but is proportional to the actual mass changes.

In this study we show the feasibility of our solution by comparing it with others solutions (not only with IOM) and a better agreement is obtained.

Changes: The *Tiwari et al. (2009)* paper is now cited in the P4675, L23. The related reference is added as well, also see below:

Tiwari, V. M., Wahr, J., and Swenson, S.: Dwindling groundwater resources in northern India, from satellite gravity observations, Geophys. Res. Lett., 36, L18401, doi:10.1029/2009GL039401, 2009.

Comment 23: "*On page 4680 lines 2-4: differences between ICESat estimates are complicated. Could also be due to the firn correction and density conversion, the interpolation scheme, the elevation change method (crossover versus along-track versus overlapping footprints), etc.*"

    Response: we added some text to give more reason for differences

    Changes: we added more possible reasons listed in this comment, and update is as below:

    "This may be explained by the complicated regional ice surface geometry in the coastal areas (Zwally et al., 2011), or uncertainty resulting from the conversion of height changes to mass changes, e.g. different firn corrections and density conversions." (P4680 L2-L4)

Comment 24: "*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.*"

    Response: if the trend is not stable it should also be reflected in each solution. So the comparison within the same time interval is valid, in our opinion.

Comment 25: "*On page 4681 lines 9-12: What do you mean by "becomes similar?" Within errors of the GRACE results?*"

Response: yes, the new GRACE and IOM agree with each other within the uncertainties.

Changes: we change this sentence to "The mass changes rate agree with the GRACE mass balance in this region within uncertainties."

Comment 26: "*In table 1: missing a parentheses on the line for Barletta (2003–2012)."*

Response: the typo is corrected.

We thank again the reviewer for the thorough review and detailed comments.

To reviewer: (Anonymous Referee #2, 23 Oct 2015):

Major comments 1: *"The first major deficiency is the corrupt mathematical development in Section 2.2.*

> 1.1) *Let's start with Equ.5. I understand that SMBt and Dt are mass rates [in units of mass change per time] while δTMBt is a cumulative mass change [in units of mass].*

> 1.2) *It is really uncommon to denote an integration by the symbol δ. Very confusingly, this symbol is used to denote a difference some lines later (p. 4669, line7).*

> 1.3) *Also, t is used in two different senses in the same equation (as running variable and as upper limit of the integral). In equation 6, the upper integration limit is tn, instead.*

> 1.4) *In Equ. 6, I understand that SMB0 and D0 are cumulative mass changes [unit of mass] again. Then, the equation in the line after Equ. 6 cannot be correct because it contains a mass rate [mass change per time] at the left side and a cumulative mass change [mass] on the right side. All the later discussion in the manuscript on D0 and SMB0 suffers from the confusion in the formalism by which these quantities are introduced.*

> 1.5) *Equation 7 is wrong because the total mass balance in the interior must depend on ice flow across the 2000m contour."*

Response: First of all, we have rewritten all the derivation in section 2.2. The new section 2.2 is provide in a separate ".*doc*" file in attachment.

Your comment here is truly valuable. By considering them, we mainly made the following changes.

> 1.1) we clarified the notation and unit. Taking SMB as an example: "**SMB**" is the monthly SMB (*Gt per month*), "**ΔSMB**" is the cumulative SMB (*Gt*), "**SMB$_0$**" is the reference SMB (*Gt per month*), **δSMB** is the monthly SMB after removing the SMB$_0$ (*Gt per month*).

> 1.2) The "Δ" requires an explanation., since "Δ" is a notation normally used for a difference. In this manuscript, **ΔSMB** is obtained by integrating the monthly SMB over a certain period $\mathbf{\Delta SMB}_i = \int_{i_1}^{i} \mathbf{SMB}_t dt$. However, it is also a measurement of the mass anomaly between month i_1 and i, thus in our opinion "Δ" is reasonable to represent the cumulative SMB.

1.3) About the integral, we show another example here to demonstrate our changes. Before Eq. (7) was: $\boldsymbol{\delta TMB_t^{up}} = \int_{t_1}^{t_n}(\boldsymbol{SMB_t^{up}})\boldsymbol{dt}$, the new Eq. (6) becomes: $\boldsymbol{\Delta TMB_i^{II}} = \int_{i_2}^{i} \boldsymbol{SMB_t^{II}}\boldsymbol{dt}$. Note that we also change the superscript "up" and "down" to "**II**" and "**I**" to distinguish the IOM component above and below 2000m contour.

We now present the month index in a clearer way. We define that, for the months within the 1961 to 1990 period, the month index is from $\boldsymbol{i_0}$ to $\boldsymbol{i_1}$; and for the months after the reference period, the index is from $\boldsymbol{i_2}$ to $\boldsymbol{i}$ ($\boldsymbol{i \geq i_2}$).

1.4) The old version of Eq. (6) was indeed confusing. In our revision we follow van den Broeke et al, (2009). In Eq. (6) we remove the cumulative term of SMB during the reference period, instead we add a piece of text to explain that since we assume the GrIS was in balance, so the cumulative SMB and D during this period is cancelled, and in the new Eq. (6) we only show the cumulative terms after the reference period. The new Eq. (6) is $\boldsymbol{\Delta TMB_i} = \int_{i_2}^{i}(\boldsymbol{\delta SMB_t} - \boldsymbol{\delta D_t})\boldsymbol{dt}$.

1.5) We make the assumptions for Eq. (7) and Eq. (8) explicitly.

- $(\boldsymbol{F^{II}})$ is constant over time, which means $\boldsymbol{F^{II}} = \boldsymbol{F_0^{II}}$ ($\boldsymbol{F_0^{II}}$ is the $\boldsymbol{F^{II}}$ during the reference period), so $\int_{i_2}^{i} \boldsymbol{\delta F_t^{II}}\boldsymbol{dt} = \boldsymbol{0}$.

- the separate GrIS interior and coastal regions are all in balance during the 1961 – 1990 reference period, i.e. $\int_{i_0}^{i_1}(\boldsymbol{SMB_0^{II}} - \boldsymbol{F_0^{II}})\boldsymbol{dt} = \boldsymbol{0}$ and $\int_{i_0}^{i_1}(\boldsymbol{SMB_0^{I}} + \boldsymbol{F_0^{II}} - \boldsymbol{D_0})\boldsymbol{dt} = \boldsymbol{0}$.

Based on communication with Ian Joughin and Ellyn Enderlin, we agree that althongh the $\boldsymbol{F^{II}}$ acceleration is not completely 0 in some sub-regions it is reasonable to make this assumption, if we include the associated uncertainties. (See P4670 L8 – L15)

In section 2.3, we use the runoff to interpolate the reference discharge. (P4671 L22&L23) the notation $\boldsymbol{\delta}$ appeared again. So we updated the notation Before it was $\boldsymbol{\delta D} = \boldsymbol{SMB_0}$ – D and it is changed to $\boldsymbol{D'} = \boldsymbol{SMB_0}$ – D . Similar to R′. The same mistake in figure 2 is changed as well.

Changes: please check the new section 2.2 in a separate "*newderivation.pdf*" file in attachment.

Major comments 2: *"The second major deficiency is that the "IOM-based" simulation used to derive the leakage correction in the GRACE results is not well described. Therefore the reader cannot assess the validity of the leakage correction based on this simulation.*

2.1)   Page 4673, line 8: How can one interpolate D on a grid? Of course you can express the ice flowrelated mass balance component locally, in theory. But I don't think you have the data the evaluate it practically. The discharge D data is just the integral of the flow-related mass balance component over the entire basin.

2.2)   Note that the ice-dynamics part of the story is the complicated part because it is so spatially concentrated, different from the SMB part. To avoid, or "correct" leakage effects that are compatible with both kind of mass balance components will be challenging unless one uses oversimplified assumptions.

2.3)   Given the incomplete description of the procedure, it is not clear whether the perceived improvement of the GRACE method follows a circular reasoning: Adapt the GRACE results so that they better fit the IOM results, and subsequently "validate" the success by the same IOM results.

2.4)   From Table A1 it appears that in the simulation, the leakage errors of the different basins do not sum up to zero but to about -36 Gt/yr. This needs to be commented. Does it mean that previous applications of the mascon method were subject to an error of this magnitude.

2.5)   Page 4676, line 7: Does the vector y represent values of a grid?. If so, are the errors added in line 7 assumed to be spatially uncorrelated? Does the simulation use a full time series or just a linear trend?"

Response: Since we use a very similar simulation as in our previous study, see figure 2a) in Xu et al, (2015) (we provide a figure used in that paper below). we decided to keep a short description in this manuscript and cite our previous study. At the moment when we edited this section, our work was just submitted, see Xu et al, (2015), now the citation is correctly listed in the text. To answer to your comments, the responses are following:

2.1)   We use the discharge data from Enderlin, for 178 glaciers. And for each glacier geographical coordinates are provided. So when interpolating the discharge to a 1 degree by 1 degree map of GrIS, we add up all the discharge which are in the same grid. In this way, we obtain the lumped discharge distribution for the 1 degree resolution map. It is not distributing the discharge to the entire DS.

2.2)   Though the 1 degree grid is coarse compared to the 0.25 degree or 0.1 degree SMB resolution, it still valid to present the spatial concentration of the discharge at this resolution.

2.3)　Another reviewer made a remark about seemingly circular analysis from another reviewer. Please allow us to borrow from the response to that reviewer at this point.

"The analysis may appear to be circular but in fact IOM doesn't directly constrain the mass balance from GRACE. The constraints are used because we have found that in some sub-regions, the GRACE inferred mass balance can be very unrealistic. For instance:

　　1)　On one region the mass increases by hundreds of Gt in a month, while there is hundreds Gt of mass loss in the neighbouring region.

　　2)　In particular in the interior regions, if one area shows positive trend of mass changes while the adjacent areas always show negative trend, this maybe be due to instability in the inversions, the effect of which we dubbed 'correlation error' in Xu et al. (2015).

Therefore we used the IOM in a simulation only assuming that it is a reasonable measure of the monthly variability and the inter-region correlation of the mass changes, but not necessarily the mass balance themselves. Furthermore, we have shown that the constrained results mainly depend on the GRACE observations, please see our early study as cited in the main text, i.e. Xu et al, (2015)."

2.4)　In an idea case, if the result is wrongly distributed between basins, the sum of all the corrections to the GRACE solution should be 0. But also note that this correction comes with uncertainties, which come from the simulation model and the correction method. As you can see in Table A2 under the header "cor (3a)" we listed the uncertainties related with correction and it is ~7.5 for the entire GrIS.

From our point of view, the actual values of corrections for the GRACE solutions are for the inversion method used in this study, and whether there is similar magnitude of bias in other GRACE inferred solutions will be tested in a possible follow-up study. Note that, there are different inversion approached for GRACE, and we think that our simulation model approach can be also applied to other GRACE solutions to quantify approximation errors.

2.5)　In P4676 L7, the vector **y** is indeed a vector that represents 2-D map. The errors are not spatially uncorrelated because they are calculated from the errors for the GRACE spherical harmonics. The spherical harmonics errors themselves are assumed to be uncorrelated between degree and orders. The simulation enables us to estimate linear trends from the full time series. We add the note in the text (see the added explanation of the linear trends at P4673, L16), and thank you for mentioning it.
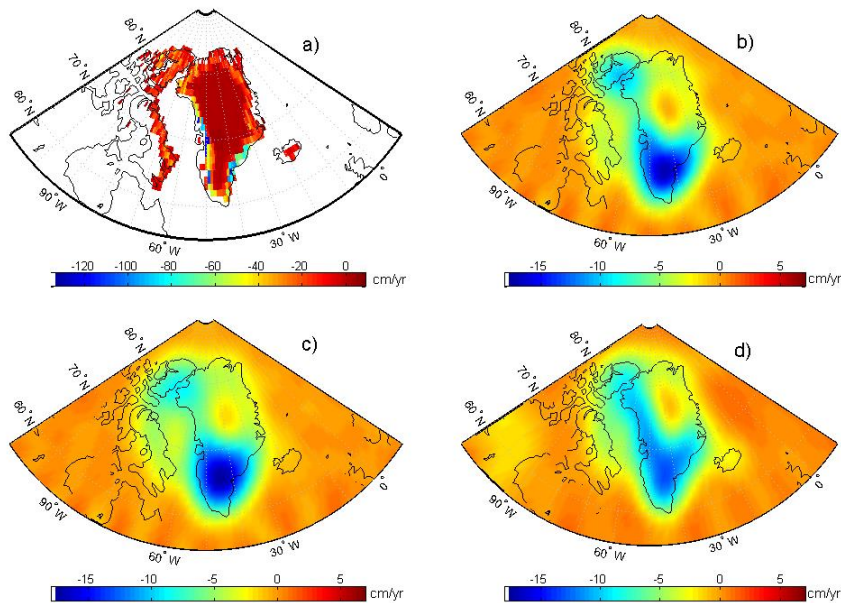
Figure 2: Mass change simulation model results based on the IOM. a) shows the gridded EWT change trend on a $1°\times 1°$ grid for the time period January 2003 to April 2012. The unit is cm/yr. b) shows EWT change trend of the simulation model $\mathbf{y}$. The simulation is based on a) after spherical harmonic analysis and synthesis up to degree and order 60 and Gaussian filtering ($r_{1/2}$=300km), and also includes noise in the GRACE data. The average EWT change trend for each region computed from the IOM is $\mathbf{x'}$, and the associated simulated GRACE data (after smoothing) $\mathbf{y'} = \mathbf{Hx'}$ is shown in c). d) shows the annual EWT trend retrieved from the GRACE data for the same time span.

Other comments

comments 1: "*Section 2.3: first 3 lines are unclear to me. I don't see why averaging should result in an error. Also, no averaging is done, but cumulation.*"

Response: To be clear, the reference SMB_0 and D_0 should always be the monthly average over the 1961 – 1990 period. But in the old version of the derivation we indeed show the accumulation over this period instead the monthly average. As we have dressed for your major comment 1, we change the derivation in section 2.2 and "*the reference is an average*" is well presented in the new text. Please check the response to your major comment 1.

We choose 1961-1990 as the reference period during which we assume the GrIS was in balance. Meanwhile this choice introduces addition uncertainty (the averaging uncertainty) as in van den Broeke et al, (2009).

comments 2: "*Page 46671, line 27: Unclear why discharge needs an SMB correction.*"

Response: Because the discharge estimates are based on the entire thickness of the ice sheet. The measured thickness includes snow, firn and ice layers. So the influences of mass layers other than ice have to be removed, this is usually called the SMB correction for ice discharge. See the discussion in Enderlin et al, (2014).

comments 3: "*Page 4672, Line 14: how can the discharge be negative?*"

Response: we agree and changed the signs in the text.

Changes: see changes in P4672, L14-17.

comments 4: "*Line 23: not clear to what numbers the word "they" refers.*"

Response: "they" refers to all the reference discharge item, including D_0 from D-08, D-14 and the ones with the runoff-to-discharge correction. We make this clear in the text

Changes: we replace "they" with "all three versions of reference discharge". (P4672, L26)

comments 5: "*References to figures and tables are wrong on p. 4672, line 12, page 4670, line 11*"

Response: we change the reference accordingly.

Changes: in P4672, L12 it is "Fig. 3" and in P4670, L11 the reference is "Table A2"

comments 6: "*Abstract, line 13:" runoff-based discharge estimates" is not clear without further explanation*"

Response: the comment is adapted in the text.

Changes: in P4662, L12 we change "runoff-based discharge estimates" to "a reference discharge derived from runoff estimates".

comments 7: "*Equation 9 is flawed: inconsistent fonts, inversion missing.*"

Response: We add the inversion for the bracket.

Changes: the new equation is: $\hat{x} = (\mathbf{H}^{\mathbf{T}}\mathbf{H} + \mathbf{P}^{-\mathbf{1}})^{-\mathbf{1}}\mathbf{H}^{\mathbf{T}}\mathbf{y}$. See in P4675 Eq. (9).

comments 8: "*p. 4680, line 18: No Ellesmere Island results are shown, actually, in Fig. 5*"

Response: We added a note in the text.

Changes: we rewrote the sentence, the new one is:

In this study, the adjacent regions of DS8 are DS1, DS7 and Ellesmere Island (northern Canadian Arctic) and in all three neighbour regions, the mass changes rate between GRACE and IOM solutions are similar, see Fig. 5. Note that Ellesmere Island is not shown in this figure, the corresponding changes rates are -36±7 Gt yr-1 and -29.4±3 Gt yr-1 for the IOM and GRACE solutions respectively. (P4680 L18)

comments 9: "*Same line: "This suggests …": I don't understand the line of argument*"

Response: As we mention in the text (same page, L12 – L16). This kind of approximation error usually exist in pairs. For instance, one region has a positive approximation error then a negative error can be found in adjacent region. (See Schrama and Wouters, 2011 and Xu et al, 2015). But for DS8, the comparison between GRACE and IOM show similar changes rate, so we believe the approximation error for GRACE in DS8 is insignificant.

Changes: we add new explanations as: "This suggests that the difference of the regional mass changes in DS8 is not due to the approximation error in the GRACE solution because there is no negative correlation between adjacent areas" (P4680 L18 – L20)

comments 10: "*There is so much repetition from Xu et al. 2015 (Geophys. J. Int.). Refer to this article more stringently and save some of the reader's time.*"

Response: There are two places where we think can be repeating description of our previous work in Xu et al. (2015).

The first place is when explaining the simulation model, i.e. in section 2.4. As you have commented in your major comment 1, the simulation needs some explanation in the main text.

The second one is when introducing the constrained inversion approach in Section 3.2. We think the current content is already reduced to the minimal length. For the readers to know the basic information of our GRACE method, we think this summarized content is necessary in the paper. It is an option to move this section to the appendix, but this will result an unbalanced structure between IOM and

GRACE. We think those two methods are equally important in this study so we prefer to keep this section in the main body of the manuscript.

comments 11: "*Appendix A2: The annual frequency is defined as 2π* 13/12, that is, with a period different from one year. Can this be correct?*"

Response: for the seasonal mass change, we consider a 13 months' seasonal circle.

comments 12: "*Fig. 4: "modified simulations" were not mentioned before, so that it is unclear what they are. End of the caption is missing.*"

Response: during the Monte Carlo test, we create a large number of simulations which are similar to the original one, but randomly altered more or less. (see P4670, L4 - L11). It is for testing the sensitivity of the approximation error correction. We use "modified simulations" to refers these randomly created simulation alternatives. This term can be confusing, so in the update, we change it to "simulations".

Changes: The missing End if added in the caption. (P4693, Fig 4)

comments 13: "*Table A1: k0 and k1 are α0 and α2 in the main text. k0 must have a unit.*"

Response: the typo is changed in the table. And the unit of α_0 is Gt, we add it in the table as well.

Changes: see updated in P4697, table A1.

comments 14: "*Reference Noel et al. is missing in the list. Reference Colgan et al. could rather use the final version in Remote Sensing of Environment.*"

Response: the references are added accordingly

Changes: the new references as below:

Noël, B., van de Berg, W. J., van Meijgaard, E., Kuipers Munneke, P., van de Wal, R. S. W., and van den Broeke, M. R.: Evaluation of the updated regional climate model RACMO2.3: summer snowfall impact on the Greenland Ice Sheet, The Cryosphere, 9, 1831-1844, doi:10.5194/tc-9-1831-2015, 2015.

Colgan, W., Abdalati, W., Citterio, M., Csatho, B., Fettweis, X., Luthcke, S., Moholdt, G., and Stober, M.: Hybrid inventory, gravimetry and altimetry (HIGA) mass balance product for Greenland and the Canadian Arctic, Remote Sens. Environ, 168, 24–39, doi:10.1016/j.rse.2015.06.016, 2015.

Reference:

Enderlin, E. M., Howat, I. M., Jeong, S., Noh, M. J., Angelen, J. H., and Broeke, M. R.: An improved mass budget for the Greenland ice sheet, Geophys Res Lett, 41, 866-872, 2014.

van den Broeke, M., Bamber, J., Ettema, J., Rignot, E., Schrama, E., van de Berg, W. J., van Meijgaard, E., Velicogna, I., and Wouters, B.: Partitioning recent Greenland mass loss, science, 326, 984-986, 2009.

Xu, Z., Schrama, E., and van der Wal, W.: Optimization of regional constraints for estimating the Greenland mass balance with GRACE level-2 data, Geophys J Int, 202, 381-393, 2015.

Schrama, E. J. and Wouters, B.: Revisiting Greenland ice sheet mass loss observed by GRACE, J Geophys Res-Sol EA (1978–2012), 116, 2011.

We thank again the reviewer for the thorough review and detailed comments.

To reviewer: (Anonymous Referee #3, 03 Nov 2015):

Major comments 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)."*

> **Response**: We choose to correlate the anomaly of runoff and the discharge as motivated by Rignot, et al., (2008), where the anomaly of discharge and the anomaly of SMB are correlated. The correlation coefficient between R and D, should be the same as between SMB_0-D and R-R_0.
>
> The four-year averaging is indeed our mistake. We intended to say an averaged runoff with preceding 4-year. When including the current year, it should be 5-year averaging. We change the mistake in the text.
>
> We agree that it will be a good idea to test the influences of using different lagging period, maybe we can find an optimal average period by comparing with GRACE. However, we applied this approach mainly to explain the fact that the determination of reference discharge would create a noticeable uncertainty, resulting in a disagreement between GRACE and IOM in some regions and we don't think it is necessary to include different lags in the correlation.
>
> **Changes:** We add the reference to Bamber et al and Box and Colgan together with another sentence as: "Note that the lagging correlation is discussed in Bamber et al. (2012) and Box and Colgan (2013)." (P4671, L23)
>
> Two new references are:
>
> Bamber, J., van den Broeke, M., Ettema, J., Lenaerts, J., and Rignot, E.: Recent large increases in freshwater fluxes from Greenland into the North Atlantic, Geophys Res Lett, 39, L19501, DOI: 10.1029/2012GL052552, 2012.
>
> Box, J., and Colgan, W.: Greenland Ice Sheet Mass Balance Reconstruction. Part III: Marine Ice Loss and Total Mass Balance (1840–2010). J. Climate, 26, 6990–7002, doi: http://dx.doi.org/10.1175/JCLI-D-12-00546.1, 2013.
>
> We correct the 4-year to 5-year in P4671, L22.

Major comments 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."*

Response: It is correct that the discharge excluded the mass loss of peripheral glaciers. So in theory indeed a "perfect" IOM should present less mass loss than a "perfect" GRACE solution.

However, we are using SMB mass loss estimates for all of Greenland (not only the GrIS), so we can probably account for the majority of mass loss from peripheral glaciers and ice caps. There are far less marine-terminating glaciers draining the glaciers and ice caps than draining the ice sheet: according to Gardner et al. (Science, 2013), less than half of the glaciers and ice caps are marine-terminating in Greenland (see his Fig. 1). Also, given the relationship we found between glacier width and area for the ice sheet's marine-terminating glaciers, we suspect that discharge from these glaciers is quite small and that changes in mass are dominated by changes in SMB. Basically, the GRACE-IOM difference will likely be on the order of only Gt/yr due to the exclusion of discharge from peripheral marine-terminating glaciers and ice caps as long as you are using SMB for all of Greenland, not just the ice sheet.

Changes: In order to make it clear, we add addition explanation of SMB in Section 2.1 as: "Note that the RACMO2 model also provides the estimates of SMB in the peripheral glacier areas, which we have included in this study." (P4666, L25).

And in Section 4, when we find the regional mass changes differences between GRACE and IOM we also explain that:

"Previous studies, e.g. Bolch et al. (2013) and Gardner et al. (2013), show that approximately 40 Gt yr^{-1} mass losses are from the peripheral glaciers. Yet, these portion of mass losses are not considered in our IOM solution. However, given the relationship we found in our discharge data between glacier width and area for the ice sheet's marine-terminating glaciers, we suspect the discharge from these glaciers is quite small and the regional mass changes in these glacier areas are dominated by changes in SMB. Ideally, the GRACE-IOM difference will likely be on the order of only Gt yr^{-1} due to the exclusion of discharge from peripheral marine-terminating glaciers and ice caps as long as we consider the SMB for the whole of Greenland, not just the ice sheet." (P4679, L2)

The two new references are:

Bolch, T., Sandberg, S. L., Simonsen, S.B., Mölg, N., Machguth, H., P. Rastner, P., and Paul, F.: Mass loss of Greenland's glaciers and ice caps 2003–2008 revealed from ICESat data, Geophys. Res. Lett., 40, 875–881, doi:10.1002/grl.50270, 2013

Gardner, A. S., Moholdt, G., Cogley, J. G., Wouters, B., Arendt, A. A., Wahr, J., Berthier, E., Hock, R., Pfeffer, W. T., Kaser, G., Ligtenberg, S. R., Bolch, T., Sharp, M. J., Hagen, J. O., van den Broeke, M. R., and Paul, F.: A Reconciled Estimate of Glacier Contributions to Sea Level Rise: 2003 to 2009, Science, 340 (6134), 852-857, DOI:10.1126/science.1234532, 2013

Major comments 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?"*?

Response: In order to better improve the structure, we made the following changes:

1) We moved section 2.3 (a 'result' section where we investigate the reference SMB and D) to section 4 as a new section 4.1.

2) We moved section 3.3 (a 'result' section where we correct the approximation error in GRACE) to section 4 as another new section 4.2.

3) We moved section 2.4 to the appendix.

4) We change the overview of each section content in the Introduction section. P4666, L6-L11.

5) We change the section reference in the text accordingly. That is in P4673, L5; P4675, L13 and P4677, L17

Thank you for this very helpful comment.

Major comments 4: *"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."

> **Response**: The derivation in section 2.2 was also commented on by other referees, thus we decided to rewrite this section. Following the comments of all referees wit think the mathematical notation is much improved such that the units for each variable can be understood. The new section 2.2 can be found in the attachment.

Major comments 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?"*

> **Response**: I think you mean the spatial resolution of the simulation we used which is mainly based on the RACMO2 and the discharge from Enderlin. We add a citation in section 2.3 to Xu et al., (2015), where the simulation model is described in details. We do this in order to keep the description simple in this manuscript and to reduce the structural complexity. But to give an idea about the simulation, we paste a figure used by our previous study in Xu et al, (2015), please see below:
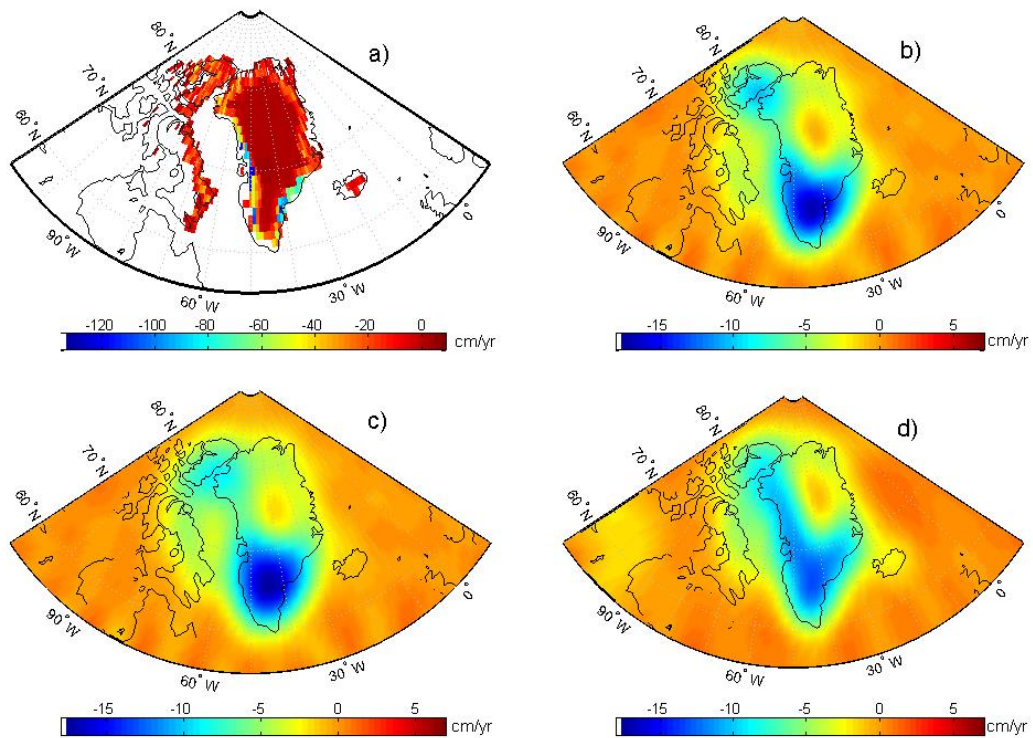
Figure 2: Mass change simulation model results based on the IOM. a) shows the gridded EWT change trend on a 1 °x1 °grid for the time period January 2003 to April 2012. The unit is cm/yr. b) shows EWT change trend of the simulation model **y**. The simulation is based on a) after spherical harmonic analysis and synthesis up to degree and order 60 and Gaussian filtering ($r_{1/2}$=300km), and also includes noise in the GRACE data. The average EWT change trend for each region computed from the IOM is **x′**, and the associated simulated GRACE data (after smoothing) $\mathbf{y′ = Hx′}$ is shown in c). d) shows the annual EWT trend retrieved from the GRACE data for the same time span.

The Simulation uses a 2-D spatial grid with a resolution of 1 degree. To obtain the SMB we sum up all the SMB estimates (0.1 degree resolution) from RACMO2 within the same grid. Note that Enderlin's discharge estimates (Enderlin et al., 2014) contain the discharge of glaciers at 178 different geographic locations, so to get discharge estimates per basin we add the discharge of all glaciers within each basin.

However, we understand your confusion. So instead of saying it is a IOM based simulation, we say it is a GrIS simulation. And this simulation is based on the RACMO2 model and Enderlin's discharge.

Changes: We change the caption to "2.4: The GrIS Simulation". P4673, L6

We replace L7 on the same page with: "The GrIS monthly mass balance simulations that will be used in section 4.2 are based on the RACMO2 model and the discharges estimates from Enderlin et al., (2014). Note that the discharge

"

We change the sentence in P4676 L4 and L5 to say that the simulation is based on the SMB and D estimates and cite Appendix A5.

Major comments 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."*

Response: Because the orbit center of GRACE satellites is identical to the mass center of Earth, the degree 1 harmonics cannot be directly observed by GRACE. Because of aliasing errors the C20 coefficient in GRACE is more uncertain than that observed by laser ranging to other satellites. Thus, as is indeed common practice, we use laser ranging estimates for C20. We used Swenson et al. (2008) model to add mass balance resulting from geocenter motion. The magnitude of the influences of replacing these coefficients is detailed in for instance Schrama et al. (2014). It is a small influence for the GrIS. The replacement of these degree and order are the one common post-processing step for GRACE spherical harmonics, c.f. (Bonin and Chambers, 2013, Sasgan et al., 2012, Schrama and Wouters, 2011; Schrama et al., 2014; Swenson et al., 2008; Swenson and Wahr, 2006; Velicogna et al., 2014; Wouters et al., 2013, etc).

The influences of replacing C20 or using different degree 1 harmonics are documented in Schrama et al., 2014. We don't test the regional influence as because it is not our main focus in this study.

Major comments 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."*

Response: During first several versions of this manuscript, most of the appendices belonged to the main body. The discussion in the appendices are minor but describing some very specific details. Beside the text in the appendix may look short but is attached with figures and tables, which makes them not small. So we decided to keep them into appendix. We do notice that appendix A2 is not anymore mentioned in the main text, thus is removed.

Changes: appendix A2 is removed. And the index is updated accordingly. P4683, L3-L5.

Minor comment 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.*"

Response: this reference is updated accordingly.

Changes: see P4685 L8 – L10.

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

Response: Thank you for point it out. We have updated our reference list.

Changes: The problematic references are for Swenson (re-positioned in P4687, L29), Velicogna (re-positioned in P4688, L27) and for Wouters (re-positioned in P4689 L6).

Minor comment 3: "*Consistency on abbreviation choice, such as "Sect. 2" (P4666L3) vs "section 3"(P4666L8)*"

Response: we change the citation of section all to "section xx"

Changes: see P4663 L28; P4665 L14; P4666 L11; P4673 L5; P4677 L17; P4683 L20; P4698 L9.

Minor comment 4: "*Presumably RACMO "version 2.3", or is it really version 3?*"

Response: it is version 3.

Minor comment 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.*"

Response: Thanks for you noticing this. Colgan et al, (2015) should be the correct citation here instead of Colgan et al, (2014).

Changes: We changed the citation see P4669, L26 and updated the reference

Colgan, W., Box, J. E., Andersen, M. L., Fettweis, X., Csathó, B., Fausto, R. S., Van As, D., and Wahr, J.: Greenland high-elevation mass balance: inference and implication of reference period (1961–90) imbalance, Ann. Glaciol, 56, 105-117, DOI: http://dx.doi.org/10.3189/2015AoG70A967, 2015.

Minor comment 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)?"

Response: yes, we randomly sampled by months, and we also had the concern of an unbalanced sampling problem. Instead of apply an internal mechanism, we run the Monte Carlo simulation 5 times, and in each time 5000 combinations of months are created. We compared the 5 runs, and found the differences to be small.

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

Response: The acceleration is only mentioned in the discussion and not a main contribution, so it is not highlighted in the text. But to give you the idea of the spatial distribution of the acceleration, please check the map on the right column of Fig. A2.

Minor comment 8: *"P4683L18 – Do you really use 11 models of GIA, or rather 11 simulations derived from a smaller number of models?"*

Response: To make it clear, we use 5 basic GIA models with a total of 11 different realizations (model parameters).

Reference:

Rignot, E., Box, J., Burgess, E., and Hanna, E.: Mass balance of the Greenland ice sheet from 1958 to 2007, Geophys Res Lett, 35, 2008.

Bolch, T., Sandberg, S. L., Simonsen, S.B., Mölg, N., Machguth, H., P. Rastner, P., and Paul, F.: Mass loss of Greenland's glaciers and ice caps 2003–2008 revealed from ICESat data, Geophys. Res. Lett., 40, 875–881, doi:10.1002/grl.50270, 2013.

Bonin, J. and Chambers, D.: Uncertainty estimates of a GRACE inversion modelling technique over Greenland using a simulation, Geophys J Int, 194, 212-229, 2013.

Gardner, A. S., Moholdt, G., Cogley, J. G., Wouters, B., Arendt, A. A., Wahr, J., Berthier, E., Hock, R., Pfeffer, W. T., Kaser, G., Ligtenberg, S. R., Bolch, T., Sharp, M. J., Hagen, J. O., van den Broeke, M. R., and Paul, F.: A Reconciled Estimate of Glacier Contributions to Sea Level Rise: 2003 to 2009, Science, 340 (6134), 852-857, DOI:10.1126/science.1234532, 2013

Sasgen, I., van den Broeke, M., Bamber, J. L., Rignot, E., Sørensen, L. S., Wouters, B., Martinec, Z., Velicogna, I., and Simonsen, S. B.: Timing and origin of recent regional ice-mass loss in Greenland, Earth Planet SC Lett, 333, 293-303, 2012.

Schrama, E. J. and Wouters, B.: Revisiting Greenland ice sheet mass loss observed by GRACE, J Geophys Res-Sol EA (1978–2012), 116, 2011.

Swenson, S., Chambers, D., and Wahr, J.: Estimating geocenter variations from a combination of GRACE and ocean model output, J Geophys Res-Sol EA (1978–2012), 113, 2008.

Velicogna, I., Sutterley, T., and van den Broeke, M.: Regional acceleration in ice mass loss from Greenland and Antarctica using GRACE time-variable gravity data, Geophys Res Lett, 2014.

Wouters, B., Bamber, J., van den Broeke, M., Lenaerts, J., and Sasgen, I.: Limits in detecting acceleration of ice sheet mass loss due to climate variability, Nat Geosci, 6, 613-616, 2013.

Colgan, W., Box, J. E., Andersen, M. L., Fettweis, X., Csathó, B., Fausto, R. S., Van As, D., and Wahr, J.: Greenland high-elevation mass balance: inference and implication of reference period (1961–90) imbalance, Ann. Glaciol, 56, 105-117, DOI: http://dx.doi.org/10.3189/2015AoG70A967, 2015.

New derivation

2.2. Cumulative TMB anomaly

For the whole GrIS or a complete basin from ice sheet maximum height to the coast, the total mass balance is:

$$\text{TMB} = \text{SMB} - \text{D} \qquad (1)$$

In this study, we further separate each GrIS basin in a downstream (I) and upstream (II) region separated by the 2000m surface elevation contour line. Thus, for the sub-divided regions Eq. (1) becomes:

$$\text{TMB} = \text{TMB}^{\text{I}} + \text{TMB}^{\text{II}} \qquad (2)$$

Where

$$\text{TMB}^{\text{II}} = \text{SMB}^{\text{II}} - \text{F}^{\text{II}} \qquad (3)$$

And

$$\text{TMB}^{\text{I}} = \text{SMB}^{\text{I}} + \text{F}^{\text{II}} - \text{F}^{\text{I}} \qquad (4)$$

in which F^{II} refers to the ice flux across the 2000 m elevation contour, and F^{I} refers to the ice flow across the flux gate. Note that F^{II} is cancelled if the study area includes both the regions below and above the 2000m contour, but F^{II} has to be considered when the upstream and downstream regions are considered separately. As described above, we assume that SMB changes downstream of the Enderlin-14 flux gates are negligible and that $\text{F}^{\text{I}} = \text{D}$.

In order to fit the temporal resolution of the modeled SMB data, we interpolate the yearly D on a monthly basis. Significant seasonal variations in ice velocity have been observed along Greenland's marine-terminating outlet glaciers (Moon et al., 2014). However, since we focus mostly on long-term changes in mass in this study, monthly variations in D should have a negligible influence on our analysis and we assume that D is approximately constant throughout the year. The monthly GRACE data represent the gravity field of Earth at that particular month. By subtracting the gravity field from a reference period (e.g. the 2003 – 2014 average), the gravity variations with respect to this reference can be obtained. These can be converted to mass variations assuming that all mass variation takes place in a thin layer near to the Earth's surface. Contrary to the GRACE data, the SMB, D and TMB are estimates of rates of mass change (i.e., mass

flux) in Gt per month. Hence in order to compare with GRACE, one has to integrate the SMB and D from a certain month (or year), which yields:

$$\Delta TMB_i = \int_{i_0}^{i}(SMB_t - D_t)dt \tag{5}$$

where ΔTMB_i is the cumulative mass change at month i in IOM (unit is Gt) and the integration time period is from a certain initial month i_0 to month i.

In previous study of mass balance IOM, when estimates of D are not available for some regions (Rignot et al., 2008), the 1961 to 1990 reference SMB is used to approximate the missing regional D (Sasgen et al., 2012). Also, due to the uncertainties in the SMB model, accumulating the TMB over a long time period may also lead to unrealistic mass gains or losses (van den Broeke et al., 2009). By removing the reference, the influence of the large uncertainties and inter-annual variability in SMB and D can be reduced (van den Broeke et al., 2009), for instance the uncertainties due to model configurations could be the similar in very month SMB estimate, and cumulating over long period may result to a large uncertainty. The reference period is chosen based on the assumption that the mass gain from the surface mass balance during that period is compensated by ice discharge, so the GrIS was in balance (i.e. no mass change).

For the reference period we defined the month index to run from i_1 to i_1, from i_2 to i_n afterwards. Since we assume the GrIS was in balance during this period, $\int_{i_0}^{i_1}(SMB_t - D_t)dt = 0$. By removing the reference SMB and D (i.e. SMB_0 and D_0) Eq. (5) becomes:

$$\Delta TMB_i = \int_{i_2}^{i}(\delta SMB_t - \delta D_t)dt \tag{6}$$

where $i \geq i_2$, $\delta SMB_i = SMB_i - SMB_0$ and $\delta D_i = D_i - D_0$. Note that SMB_0 and δSMB_i are both rates of mass change, similar to the discharge.

As explained before, when Eq. (6) is used to compute the mass balance for the regions below and above 2000m separately, the ice flux across the 2000m contour (F^{II}) has to be considered. Therefore we introduce two assumptions, i.e. 1) F^{II} is constant over time, which means $F^{II} = F_0^{II}$ (F_0^{II} is the F^{II} during the reference period), so $\int_{i_2}^{i}\delta F_t^{II}dt = 0$, and 2) the separate GrIS interior and coastal regions are all in balance during the 1961 – 1990 reference period, i.e. $\int_{i_0}^{i_1}(SMB_0^{II} - F_0^{II})dt = 0$ and $\int_{i_0}^{i_1}(SMB_0^{I} + F_0^{II} - D_0)dt = 0$ Assumption 1) is necessary since there is a lack of

yearly measurements of ice velocity across the 2000m contour. An estimate of decadal change by Howat et al. (2011) suggests it is reasonable to assume a constant F^{II} for the entire GrIS, except for a few glaciers, such as the Jakobshavn glacier in basin 7 where the F^{II} may be higher than F_0^{II} after 2000. In Andersen et al. (2015), the mass balance of the interior GrIS (in their study defined as the ice sheet above the 1700 m elevation contour) was 41±61 Gt/yr during the 1961-1990 reference period and in Colgan et al. (2015) the ice flux across the 1700m contour was estimated to be 54±46 Gt/yr for the same time period, indicating the assumption of balance approximately holds within the uncertainties.

Based on these two assumptions, we apply Eq. (6) for the interior and coastal GrIS regions, yielding:

$$\Delta \text{TMB}_i^{II} = \int_{i_2}^{i} \text{SMB}_t^{II} dt \qquad (7)$$

And

$$\Delta \text{TMB}_i^{I} = \int_{i_2}^{i} (\text{SMB}_t^{I} - D_t) dt \qquad (8)$$

We quantify the combined uncertainties of assumptions 1) and 2) by comparing the results from Eq. (8) to the regional mass balance derived from GRACE by Wouters and Schrama (2008) and derived from ICEsat by Zwally et al. (2011), resulting in ~±15 Gt/yr uncertainties for the entire interior GrIS. The regional uncertainties are summarized in Table A2. Note that for each region, the same uncertainty is applied to both the interior and coastal areas. For the whole basin the uncertainties associated with assumption 1) and 2) will vanish, because these two assumptions are needed only when we separate the coastal and interior regions.