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

## To reviewer: (Anonymous Referee #1, 30 Sep 2015):

**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 “*.doc*” 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 δF2000 = 0, but Ft2000 = F2000 + δF2000 and F2000 is not 0. If (SMB0up = F2000) and (SMBdown+δTMBup =Dt):*

*δTMBup = SMB0up −F2000 +∫(SMBtup −F2000) dt = ∫(SMBtup−F2000)*

*δTMBdown = SMB0down + F2000−Dt+∫(SMBtdown +F2000−Dt)dt*

*=∫(SMBtdown+F2000-Dt)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 citied 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.