

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

To Editor Etienne Berthier

Comment 1: *“Although it has been improved, there is still room for improvements and simplifications of the notations. They are still difficult to follow. What about replacing i_0 by 1961 (or 1960?) and i_1 by 1990 right away in the equations as these dates are not variable? Is there a point in using symbols instead of the actual limits of the reference period?”.*

Response: We have updated the content according to your comment, please see Page 10, L5&L13 and Page 11, L2&L4.

Comment 2: *“P3 L21-22. Still unclear. Either omit or explain well right away. Also be careful about the year where the reference period starts (sometime 1960 sometime 1961).”.*

Response: The reference period is assumed to always be 1961-1990, we have made it be consistent everywhere in the text. And we understand it can be confusing, because we say we use the reference SMB and D to reduce the uncertainties then right behind this statement, we say this will introduce new uncertainties. So change the content as below:

In order to reduce the uncertainties in the mass changes of SMB and D, the SMB and D from 1961 to 1990 are sometimes used as a reference when applying the IOM method (van den Broeke et al., 2009; Sasgen et al., 2012). However, introducing 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 Sect. 2. (see P3 L21 to P4 1-4)

Comment 3: *“P4 L1. At this stage, “reference” has not been introduced/defined”.*

Response: The updates associated with comment 2 now make clear that we use the SMB and D during a 1961 to 1990.

Comment 4: *“P4 L5. Not end of sentence (not full stop)”.*

Response: period is changed to comma.

Comment 5: *“P4 L17. Maybe start a new paragraph here? Or better connect the sentence “In Sasgen et al.” with the earlier part of the paragraph”.*

Response: We put “In Sasgen et al” and the following part as a separate paragraph and move the content before this part to one paragraph below. (see P5 L15)

Comment 6: “P5 L5. Full stop missing”.

Response: the period has been added.

Comment 7: “P5 L10. No comma”.

Response: the comma is removed.

Comment 8: “P5 L16. Use “found” instead of “find” if you describe here the result of your earlier study”.

Response: This is corrected.

Comment 9: “P6 L5. RACMO versions: sometime v2 sometime v2.3. Homogenize.”.

Response: It is now RACMO v2.3 consistently throughout the text.

Comment 10: “P9 L19. what is a long time period? 30 years? 10 years? Unclear”.

Response: we replaced it with ‘decades’. (see P9 L17)

Comment 11: “P10 L18. For clarity, rephrase to “where, after 2000, F^I may be higher than F_0^I ”.”.

Response: the sentence is adjusted accordingly (see P10 L16-L17)

Comment 12: “P13 L15. “Using a simulation based on the IOM...”. I think this is what leads one of the referees to the state that the reasoning is circular. In the manuscript (and not only in the rebuttal letter), can you convince us and all readers that this is not the case?”.

Response: We have added the explanation at the end of this paragraph, see below:

It has to be highlighted that the GRACE method used in this study is not constrained by the mass changes derived from the RACMO2.3 and the ice discharge estimates directly. Instead, from the RACMO2.3 and the ice discharge estimates, we derived the variance of the mass change in each region as the constraint. Moreover, in our previous study, i.e. Xu et al., 2015, we showed that the derived mass changes in the coastal regions are most sensitive to the GRACE data instead of the constraints, but for the interior. (P13, L16-21)

Comment 13: “P15 L12. “only for those””.

Changes: In this study we provide a runoff-based estimates for D_0 only for those ice sheet basins where the correlation between δD and δR is strong (Fig. 2). (see P 15, L14)

Comment 14: “P15 L23. Grammatical structure of the sentence is not OK.”

Response: we rephrased this sentence see:

In that region, D^{D-08} and D^{D-14} are similar, i.e. $20.1 \pm 1.9 \text{ Gt yr}^{-1}$ and $17.6 \pm 2.2 \text{ Gt yr}^{-1}$, respectively, but D^R is $8.9 \pm 4.7 \text{ Gt yr}^{-1}$. (see P16, L6-L7)

Comment 15: “P16 L8. “I.e. e all”. At this stage in the review process, make sure no such typographic errors remain in the paper.”

Response: “i.e.” is replace with “and” (P16,L14)

Comment 16: “P20 L22. Cap letter for “The””.

Response: the text has been changed. (P21, L13)

Comment 17: “P21 L9. Space missing”.

Response: the typo is corrected. (P21, L22)

Comment 18: “P21 L13. Basaed -> based”.

Response: The sentence is changed. (P22, L3)

Comment 19: “Figure 3. The lower legend “Method 2 (with ref, D-09)”. “D-08” instead. L9. Why this is then written D^{D-08} in the caption (i.e. with superscript?). This is really disturbing for the reader that has a very hard time following your reasoning because of these changing notations.”

Response: we correct the legend in the figure and rewrote the caption. (see P32)

Comment 20: “Table 1. In the sake of unbiased comparison with altimetry (ICESat), authors should also provide the result they get when simply considering the 2003-2009 time period. This was also requested by the referees. Would make the comparison more meaningful”.

Response: we have updated the table 1, we now compare our solution to previous studies for three different time periods. 2003-2009 is not computed because it is very similar to 2003-2010. Please check the new table below:

Basin	DS1	DS2	DS3	DS4	DS5	DS6	DS7	DS8
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Zwally -11 (2003-2007) ICESat	1±0	13±0	-51±1	-75±2	-10±0	-4±0	-14±0	-33±1
Sørensen (2003 - 2009) ICESat	-16±1	-16±3	-40±18	-43±11	-26±5	-51±7		-53±3
Colgan (2003 - 2010) GRACE	-21±6	1±6	-47±13	-28±7	-24±4	-33±7	-23±9	-42±12
Sasgen (2003-2010) IOM	-20±4	-16±5	-31±8	-66±21	-20±7	-66±20		-26±12
Sasgen (2003-2010) GRACE	-16±5	-12±5	-38±6	-42±6	-24±6	-56±7		-53±7
Barletta (2003-2012) GRACE	-17±2	-12±2	-36±4	-35±3	-23±2	-66±4		-44±4
This study (IOM solution in bracket)								
2003 - 2007	-14±6 (-22±7)	-1±8 (-9±4)	-49±6 (-55±12)	-60±13 (-73±21)	-18±5 (-11±6)	-6±9 (-14±8)	-25±6 (-17±3)	-32±6 (-13±3)
2003 - 2010	-19±4 (-25±8)	-9±5 (-10±4)	-33±5 (-37±7)	-50±11 (-61±20)	-19±7 (-16±7)	-24±8 (-32±12)	-34±5 (-26±5)	-46±5 (-28±6)
2003 - 2012	-20±4 (-26±8)	-10±4 (-11±6)	-35±5 (-40±8)	-51±11 (-59±18)	-20±7 (-18±7)	-38±8 (-46±14)	-37±5 (-38±6)	-49±5 (-30±8)

The related content is changed as well, i.e.:

We also compare our GRACE and IOM solutions to 1) GRACE 2) IOM 3) ICESat altimetry estimates from different studies, as shown in Table 1. All listed GRACE solutions agree within uncertainty levels in DS1, DS2, DS3, DS5 and DS8. In the southeast region DS4, there is a deceleration of the mass change after 2007. The regional acceleration of mass loss becomes negligible ($\sim -0.1 \text{ Gt yr}^{-2}$). Different GRACE solutions for the same time period do not result in the same mass change rates. This suggests that a large approximation error, which is associated with different approximation approaches, is likely present in this region in the GRACE solutions. As shown in Fig. 5, if we consider the time period of 2003 – 2013, the regional mass change is reduced by 29% in this region after applying the correction introduced in sect 4.2. (P22, L3-L11)

To Anonymous Referee #1

General comments 1: *“I still think the TC readership would benefit from more conventional and consistent mathematical notation (for example, why is 1996 discharge D_0 and 2000 discharge D_{2000})”.*

Response:

In the iteration, we improved our readability of our mathematical notations. For instance, we specified the upper and lower boundary of the integral in our equations instead of using i_0 , i_1 and i_2 . Now we directly use the year as the notation. Please see the changes in Page 10, L5&L13 and Page 11, L2&L4. Moreover, we change the notation of 1996 discharge to $D_{1996-08}$ to be consistent with the 2000 discharge $D_{2000-08}$. In the text, the changes are in P26 L5-L10

General comments 2: *“I might suggest that more of the content of the unusually long response letter should be in the manuscript proper. For example, the discussion about how GRACE samples both ice sheet and peripheral glaciers while IOM only samples ice sheet is quite useful in the response letter, but is reduced to two sentences on page 20. All the response supplied in the letter will not illuminate the average TC reader who does not have access to the response letter”.*

Response: We moved part of the response into the manuscript. Please check below:

One reason for the discrepancies could be the discharge from peripheral glaciers which is not included in the IOM but which does affect GRACE estimates. Previous studies, e.g. Bolch et al. (2013) and Gardner et al. (2013), show that approximately 40 Gt yr⁻¹ mass comes from the peripheral glaciers. However, this is not the reason for the difference between GRACE and IOM. In this study we are using RACMO2.3 SMB estimates for not only the GrIS but for entire Greenland, including the majority of the mass loss from the peripheral glaciers and ice caps. Discharge from the peripheral glaciers and ice caps is expected to be small because there are far less marine-terminating glaciers draining the glaciers and ice caps than draining the ice sheet. Less than half of the glaciers and ice caps are marine-terminating in Greenland (Gardner et al., 2013). Moreover, given the relationship found in Enderlin et al., (2014), the discharge data between glacier width and area for the ice sheet's marine-terminating glaciers, we expect the discharge from these glaciers to be small and the regional mass changes in these glacier areas to be

dominated by changes in SMB which are captured by RACMO2.3. The GRACE-IOM difference due to the exclusion of discharge from peripheral marine-terminating glaciers and ice caps will likely be negligible as long as the SMB for the whole of Greenland is considered, not just the ice sheet. (see P21, L1 – L15)

Also there is a comment from the editor saying we should also include the response of why our study is circular reasoning. Now we add the response into the content as well. Please see the changes in P13, L17 to P14, L2

General comments 2: *“An important specific: Table 1 lists “this study” twice, purporting to be the same epoch both times, but the drainage basin mass balance values are difference in each appearance.*

Response: The table has been updated and extended, with clearer row headers (see page 36).

To Referee #2

General comments 1: “1) What is the impact of the fact that the D_0 presented is actually not equal to SMB_0 (413 Gt/yr versus 403 Gt/yr)? 2) The use of reference periods seems to result in less agreement between methods, which likely means deficiencies in the reference period calculations.”.

Response:

If $D_0 \neq SMB_0$, say for instance $\varepsilon = SMB_0 - D_0$, then Eq. (6) (P10, L6) becomes:

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

On the right hand side, the term ε will cause a bias ε in every monthly ΔTMB_i , moreover the middle term $\int_{i_2}^i \varepsilon dt$ leads to a bias ε in the linear trend of ΔTMB_i . It is true that if we know the D and SMB perfectly (no uncertainty) for a certain period (e.g. 2003 to 2013), there is no need to introduce the reference period. However, in our case the RACMO2 model and the ice discharge estimates give the monthly SMB and D with an error. Considering the SMB as the example, there is one type uncertainty that is caused by the model set up which appears in every monthly SMB estimate. By removing the reference SMB , this type of uncertainty is reduced. Thus, the cumulative $SMB_i = \int_{i_2}^i (\delta SMB_t) dt$ is more accurate than integrating the SMB estimate themselves.

In addition, we add the reason of applying the reference period in P9, L12 to L22. And we also refer to van den Broeke et al., 2009 and Sasgen et al., 2012.

General comments 2: “Being able to resolve some of these mascons (particularly 4a, 4b, 5a, and 5b) and separate between the interior and exterior is very unlikely with current GRACE errors and resolution. In order to get estimates that even seem reasonable, one would likely have to constrain GRACE to the point of it being inseparable from the input model.

Another problem is that the difference in the approximation can be due to more than just truncation and smoothing. The misfit results found in this analysis could be due to ringing, or not properly sampling the averaging area. For example, mascon 1a covers a vast area with the GRACE trend showing a non-uniform spatial pattern over the same region. Fitting a uniform layer to this mass pattern will result in leakage.”.

Response:

We agree with the first concern. However, it is still possible to find reasonable estimates for these mascons from GRACE data, when applying constraints. We like to note that the constraints are subject to the variance of the mass changes in the input model, not the actual modelled mass changes. Tests in our previous study (Xu et al., 2014), showed that the constrained results for the coastal regions (such as 4a and 5a) are less dependent on the constraints mostly dependent on the actual mass changes signals in the GRACE data.

Regarding the second concern, we note that we get similar results for mass balance in the north-west as other GRACE studies that use kernel function, for instance a non-uniform mass distribution (derived from ICESat data). Moreover, the comparison to the other GRACE solutions shows very similar mass changes rate in this region, please see the updated table 1 (P36). Still, the kernel function definitely influences the inversed solution, and we think the influences are contained in the approximation errors. Because we count the approximation errors as the difference between the simulation and the approximation, and the kernel function is one of the reasons causing the differences. In our study, we show that this type of differences is reduced by applying a correction.

General comments 3: *“There's still a few issues with grammar and sentence structure throughout the paper.”*

Response: We greatly appreciate all your comments on the language, it truly helps us to improve the manuscript. We have updated the content according to all your comments and have gone through the entire manuscript.

Line-by-line comments

Comment 1: *“Page 2, Line 17: ICESat instead of ICESat.”*

Changes: corrected here and throughout the paper.

Comment 2: *“Page 3, Line 5: runoff instead of run-off.”*

Changes: now changed to be runoff everywhere.

Comment 3: *“Page 3, Lines 9-11: There are some major dynamical changes happening in Southeast Greenland (e.g. Enderlin et al. (2014)). I couldn't find a comment about the mass loss being dominated by SMB in Noel et al. (2015). Is this to reference the RACMO2.3 model or work within the paper?”*

Response: It is a reference to the RACMO2.3 model used in this study.

Comment 4: *“Page 3, Lines 14-15: You can mention that you use altimetry for validation purposes.”*

Response: done, see P3, L13-L15.

Comment 5: *“Pages 3-4, Lines 21-22+Line 1: I'm not sure the use of the 1961-1990 reference period is to reduce the uncertainties, but to help provide interpretation of the current mass loss. As you mentioned in the next line, the reference period introduces uncertainties of its own. Also as you mention near the end of the manuscript, the use of the reference periods reduces the agreement between IOM and GRACE (Page 23 line 23-Page 24 line 1).”*

Response: In order to derive a IOM solution, one can either use or not use reference period (Eq. 5 and Eq. 6). Given the current SMB model accuracy, not using the reference period might mean that uncertainties accumulate over time, resulting in a large uncertainty at the later periods of the monthly Δ TMB time series. Also check our response to your comment 1. However, introducing the reference period also brings in additional uncertainties to the monthly Δ TMB. (see Table A2).

We think the downside of not using a reference is more critical, so in this study we applying the reference period. But we also believe the consequence is causing the difference to the GRACE solution in the northwest region.

Comment 6: *“Page 4, Lines 4-6: should be a comma instead of a period in front of which.”*

Changes: it has been corrected. (see P4, L6)

Comment 7: *“Page 4, Line 13: I wouldn't use the word \tackle”.*

Changes: we now use the word “*investigate*” instead of “*tackle*” (see P5, L8)

Comment 8: *“Page 5, Line 7: sub-regions instead of sub-region.”*

Changes: changed to *sub-regions* (see P5, L14)

Comment 9: *“Page 5, Lines 11-13: Wouters et al. (2008) is cited twice in this sentence, and you use the word also” twice. I would rewrite the sentence:*

Regional GrIS mass changes estimated with GRACE are influenced by mass changes from areas outside the ice sheet, such as from Ellesmere Island, Baffin Island, Iceland and Svalbard (EBIS) Wouters et al. 2008.”

Changes: the sentence has been improved according to your comment, thank you. (see P5, L18-L18).

Comment 10: *“Page 6, Line 10: In the end, we”*

Changes: we rewrite this sentence. (see P6, L11-L14)

Comment 11: *“Page 7, Line 5: I would remove “Note that” from this sentence.”*

Changes: done (see P7, L8)

Comment 12: *“Page 7, Line 6: I would remove “Note that” here as well.”*

Changes: done (see P7, L9)

Comment 13: *“Page 7, Lines 9-11. I would split this sentence to have one sentence about ice thickness ending after “radar data”, and the next sentence talking about surface velocities.”*

Changes: We split the long sentence into two, and the separation is after the “radar data”. (see P7, L14). Please see below:

In Enderlin-14, the ice thickness of 178 glaciers is estimated as the difference in ice surface elevations from repeat digital elevation models and bed elevations from NASA’s Operation IceBridge airborne ice-penetrating radar data. The ice surface velocity is obtained from tracking the movement of surface features visible in repeat Landsat 7 Enhanced Thematic Mapper Plus and Advanced Spaceborne Thermal and Reflectance Radiometer (ASTER) images. (P7 L12-L17)

Comment 14: *“Page 9, Lines 5-9: Still seems a bit strange to have a GRACE introduction within the IOM methods. Might also want to cite Wahr et al. (1998) after the thin layer assumption sentence. ”*

Response: We removed this sentence and add a new one as below:

Changes: Contrary to the GRACE data which represents total mass the SMB, D and TMB are estimates of rates of mass change (i.e., mass flux) in “Gt per month” or “Gt per year”. (see P9 L6-L7)

Comment 15: *“Pages 10-11, Lines 18-20 and Lines 1-2: these results suggest that the ice sheet (interior at least) is not in balance during this time period. Also Andersen et al. (2015) and Colgan et al. (2015) are based on different interior delineations.”*

Response: It is true that studies as you have listed above show an imbalance status of the GrIS interior during the reference period, and it is also mentioned in the text (see

P10, L19 – P11 L3). However, we learned that the estimates for each sub-basin during that period is not yet ready to use, after we communicated with Ian Joughin. Ian Joughin suggested us to make this assumption and consider the imbalance as uncertainty (see P11, L8-L11 and refer their study, i.e. Howat et al., (2011)

Comment 16: *“Page 11, Lines 4 and 6: should be δ components (i.e. SMB and D)? ”*

Changes: The missing δ componets are added to Eq. (7) and Eq. (8) on P11

Comment 17: *“Page 17, Lines 15-16: using the indices i for a different meaning than on Line 12 is very confusing.”*

Changes: we changed the indices to l , see P17, L17

Comment 18: *“Page 18, Line 21-22: “applying the relationship to” (to what?)”*

Changes: We rewrote this sentence to:

When the vector of observations \mathbf{y} becomes the GRACE observations, by applying the linear relationship to the corresponding approximation $\hat{\mathbf{x}}$, we can derive an improve regional GRACE solution. (P19, L3-5)

Comment 19: *“Page 21, lines 13-19: it is hard to meaningfully compare Greenland estimates over different time periods. The regressed trend and acceleration terms are highly dependent on the observation period.”*

Response: we extended table 1. Now we show solutions for three different time periods, i.e. 2003-2007, 2003-2010 and 2003-2012, see the table on P36 and the description on page (see P36)

Comment 20: *“Page 21, lines 20-22: Despite the lack of acceleration, the mass loss in SE Greenland has been pretty variable due to both variations in SMB and ice dynamics. Note the time series in Velicogna et al. (2014) (despite the large difference in averaging areas compared to these results).”*

Response: we update the content as following:

In the southeast region DS4, there is a deceleration of the mass change after 2007. The regional acceleration of mass loss becomes negligible ($\sim -0.1 \text{ Gt yr}^{-2}$). Different GRACE solutions for the same time period do not result in the same mass change rates. This suggests that a large approximation error, which is associated with different approximation approaches, is likely present in this region in the GRACE solutions. As shown in Fig. 5, if we consider the time period of 2003 – 2013, the regional mass change is reduced by 29% in this region after applying the correction introduced in sect 4.2. (see P22, L5 – L11)

Comment 21: *“Page 22, Lines 12-13: How did you extend the ICESat results to 2013? With an IceBridge solution?”*

Changes: We rewrite this part, please check the update below:

Another area where GRACE and IOM do not agree is the northwest (region DS8). We find that our GRACE solution shows -32 ± 6 Gt yr⁻¹ mass loss during 2003-2007 and -46 ± 5 Gt yr⁻¹ during 2003-2010. The ICESat solutions show similar mass loss rates in this region (see Table 1), while the IOM solution shows lower mass loss rates, i.e. -13 ± 3 Gt yr⁻¹ and -28 ± 6 Gt yr⁻¹ for the time period 2003-2007 and 2003-2010, respectively. (see P22,L19 – P23,L2)

Comment 22: *“Page 22-23, Lines 21-22 and 1-5: The leakage of mass could be more discrete than a positive in one mascon and a negative in another. Also note that ice discharge changes of the Canadian Archipelago marine terminating glaciers is largely unknown”.*

Response: Regarding the second point we find that estimates for Ellesmere Island are similar to those of Gardner et al., 2011 who employs the D estimates for this region. We changed the text as following:

We have reduced the approximation error in the GRACE solution for this region, although by a small amount (-2.3 Gt yr⁻¹). In order to assess the influence of the remaining approximation errors, we compare the GRACE and IOM solutions in the surrounding areas, i.e. DS1, DS7. It can be seen in Fig. 5 that mass change rates are consistent within the uncertainties between IOM and GRACE solutions. This suggests that the approximation errors become negligible in the GRACE solution given the uncertainties. The comparison for Ellesmere Island is more difficult because discharge is not included in our IOM solution. However, an IOM solution including the D estimates, by Gardner et al., 2011, showed that the mass changes rate of the glacier on this island is 37 ± 7 Gt yr⁻¹ between the years of 2004 and 2009. This agrees within uncertainty with our solution for the same time interval, i.e. -35 ± 7 Gt yr⁻¹ and -29 ± 3 Gt yr⁻¹ for IOM and GRACE solutions, respectively. Hence it is reasonable to believe the mass changes estimated from IOM and GRACE can agree with each other in this region. After comparing the GRACE and IOM solutions on all the neighbor regions of DS8, no significant differences between the IOM and GRACE solution are found, it suggests that the remaining approximation error is not the major reason causing the difference in DS 8. (P23L4-L17)

Gardner, A. S., G. Moholdt, B. Wouters, G. J. Wolken, D. O. Burgess, M. J. Sharp, J. G. Cogley, C. Braun, and C. Labine (2011), Sharply increased mass loss from glaciers and ice caps in the Canadian Arctic Archipelago, *Nature*, 473(7347), 357-360, doi:10.1038/nature10089, 2011.

Comment 23: *“Page 23, Lines 79: Kjeldsen et al. (2013) is a combined ICESat+Operation IceBridge estimate.”*

Changes: Now we say it is [“The ICESat and Operation IceBridge based mass change estimate”](#). (see P23 L20)

Comment 25: *“Page 24, Line 9: ICESat instead of ICESat.”*

Response: done.

Comment 26: *“Page 24, Line 12: ICESat instead of ICESAT.”*

Response: done.

Comment 27: *“Page 26, Line 2: Is this supposed to be pre-1996 discharge estimations?”*

Response: It should be indeed pre-1996, it is corrected in text. (see P26, L2)

Comment 28: *“Page 28, Line 6: 3 GIA models with 2 having a range of rheological parameters. Not 11 independent models.”*

Response: we make the changes as following:

We apply the GIA correction to the GRACE data using three GIA models with a total of 11 different parametrizations before estimating the associated regional mass changes in 20 GrIS and surrounding Arctic regions (see the mascon definition in Sect. 3). By comparing with one without applying GIA correction, we assume the differences are the regional GIA effects. In addition to Paulson-07 GIA model, we use a GIA model with lateral changes in viscosity and the ICE-5G loading history (van der Wal et al. 2013). (P28 L8-13)

Comment 29: *“Figure 4: There should be common X and Y axes for each subplot to help discern the actual correlation. Where are the plots for the interior?”*

Response: we mentioned the X and Y axes in the caption. And in the text we explained that for the interior region, the approximations and simulations are not correlated so we do not show those.

Comment 30: *“Figure 5: Listing the GRACE estimates before the approximation correction is applied would be helpful.”*

Response: the GRACE estimates before the correction is actually shown in this plot, with hollow blue square markers.

Comment 31: *“Table 1: Besides the Colgan and Sasgen results, none of these estimates are for the same time period. As Greenland loss is accelerating, it is difficult to discern how your results compare with the others using this table (i.e. the listed altimetry estimates are for at most 6 years, while your estimate is a 10 year study).”*

Response: we adjust the table in response to the earlier comment so that some of the numbers are for same time intervals, so solutions can be compared. We have also update the associated text, please check the changes to your comment 19.