

# Review Answers

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Dear Editors, dear Reviewer,

we are very grateful for your detailed corrections and annotations. We are confident that your suggestions helped to considerably improve our manuscript. We tried to respond to all comments in the best possible way. The comments are sorted by page-line, which refer to the original, uncorrected document.

5

## Reviewer 3

### General Comments:

10

**R3C1:** 1-3: replace “estimates” with “monthly snapshots”

**Answer:** Done.

**R3C2:** 1-7: replace “;” with “,”

15 **Answer:** Done.

**R3C3:** 1-9: replace “GRACE missions” with “GRACE and the GRACE-FO mission”

**Answer:** Done.

20 **R3C4:** 1-10: maybe add “at significantly lower spatial resolution” behind “with respect to GRACE”

**Answer:** The reviewer is right. We should mention the lower resolution of Swarm in the abstract. We decided to mention it in 1-10 at “...substitute missing monthly solutions with Swarm results of significantly lower resolution”.

**R3C5:** 1-25: replace “GRACE missions” with “GRACE and the GRACE-FO mission”

25 **Answer:** Done.

**R3C6:** 2-3: At this point a general reference of the Swarm mission should be given.

**Answer:** Done. Added “(Friis-Christensen et al., 2008)”

**R3C7:** 2-10: Accelerometers provide more information than just air drag.

5 **Answer:** Changed “... for deriving the drag force” to “... for deriving the non-gravitational accelerations”.

**R3C8:** 2-15, 2-16: This is Swarm B, not Swarm C

**Answer:** Done.

10 **R3C9:** 2-18: The abbreviation GRACE should be introduced at the first occurrence.

**Answer:** The reviewer is right. It was already introduced in the abstract.

**R3C10:** 2-25: add “although Jäggi et al. (2009) showed with real GRACE GPS-derived baselines that the benefit will probably be small.”

15 Jäggi, A., G. Beutler, L. Prange, R. Dach, L. Mervart (2009): Assessment of GPS-only observables for Gravity Field Recovery from GRACE. In Observing our Changing Earth, edited by M. Sideris, 133, pp. 113-123.

**Answer:** Done (we added a new sentence: However, Jägge et al. (2009)...).

**R3C11:** 2-27: The right expression for AIUB is “Astronomical Institute of the University of Bern”.

20 **Answer:** Done.

**R3C12:** 2-33: add “gravity” in “monthly Swarm *gravity* solutions”.

**Answer:** Done.

25 **R3C13:** 2-34: add “pressure” in “drag, solar radiation *pressure* and Earth radiation pressure”.

**Answer:** Done.

**R3C14:** 3-Table 1: “van den IJssel”, not “van Den IJssel”

**Answer:** Done.

30

**R3C15:** 3-1: Replace “... Swarm satellites which is found to be important to improve the gravity results.” with “... Swarm satellites. This has been found to be important to improve the gravity field results”.

**Answer:** Done.

**R3C16:** 3-11: It is correct that the quaternions are referring to the mentioned transformation. But I guess they are eventually needed in the processing to relate the satellite reference frame to the inertial frame?.

**Answer:** We added “During the processing, the satellite reference frame needs to be referred to the inertial frame; this is achieved by multiplying the rotation matrix derived from the star camera data with the Earth rotation matrix (Petit and Luzum, 2010)”.  
5

**R3C17:** 3-15: If allowed by ESA, the explicit provision of the Swarm macro-model information in the form of a table would be most valuable for the reader and should be given in the manuscript. But I fully understand that ESA has to give permission for this..

**Answer:** As the macro model is pretty long and it was provided by ESA for our research, we would prefer if interested persons would directly contact ESA (Christian Siemes).  
10

**R3C18:** 3-18: GOCO05c is complete up to 720.

**Answer:** The reviewer is right. We wanted to say, that we used the model only until d/o 360. We changed the sentence to “... we used the GOCO05c model (Pail et al., 2016) up to degree 360 as a mean background field”..  
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**R3C19:** 3-20: “except for the atmospheric tides which were chosen such as to be aligned with ITSG Graz solutions”: Maybe state explicitly what the difference wrt the GRACE RL05 is.

**Answer:** We believe that the references that are now included should be sufficient  
(Dahle, C., Flechtner, F., Gruber, C., König, D., König, R., Michalak, G., and Neumayer, K.-H.: GFZ GRACE Level-2 Processing Standards Document for Level-2 Product Release 0005, Tech. rep., Deutsches GeoForschungsZentrum, <https://doi.org/10.2312/GFZ.b112020>, 2012.  
van Dam, T. and Ray, R.: S1 and S2 Atmospheric Tide Loading Effects for Geodetic Applications. Data set/model accessed 2018-01-18, <http://geophy.uni.lu/ggfc-atmosphere/tide-loading-calculator.html>, Updated October 2010.).  
25

**R3C20:** 4-Table 2: add references to products

**Answer:** Done.

**R3C21:** 4-Table 2: EOT11a: Specify up to which degree and order this model is used?

**Answer:** We used data from <https://doi.pangaea.de/10.1594/PANGAEA.834232>. These are gridded  $0.125^\circ \times 0.125^\circ$  data with a real and an imaginary part for each tidal constant in units of cm. We converted these data to spherical harmonic coefficients for the main astronomical tides M2, S2, N2, K2, 2N2, O1, K1, P1 and Q1, the long-period tides Mm, Mf, Om1, Om2, Sa, Ssa, Mtm and Msqm, as well as the non-linear constituent M4. The radiational tide S1 is not included here, because it is already considered in the AOD1B RL05 dealiasing product. The processing is consistent for GRACE and Swarm.  
35

**R3C22:** 4-5 to 4-7: “In Vielberg et al. (subm.) we compare NRLMSISE-00 to GRACE-derived thermospheric density and derive an empirical correction for this model; this has not yet been applied here.”: The statement does not seem to be relevant for this publication. It should be removed.

**Answer:** Vielberg et al. is submitted to the same special issue and we feel a misunderstanding may be possible. Therefore, we  
5 prefer to clarify the use of the model here.

**R3C23:** 4-11: add “ITG-” to “...results will be compared to the *ITG*-GRACE solutions.”.

**Answer:** Added “ITSG-”.

10 **R3C24:** 4-15: add “ITG-” to “...results are compared to the *ITG*-GRACE solutions.”.

**Answer:** Added “ITSG-”.

**R3C25:** 5-3: replace “have stronger problems” with “are affected by serious issues”.

**Answer:** Done.

15

**R3C26:** 5-4: Same remark as above (R3C22:“The statement does not seem to be relevant for this publication. It should be removed”): “In the light of recent improvements of empirical thermosphere models (Vielberg et al., subm.)”: The statement should rather be eliminated since the original thermosphere models are used in this study.

**Answer:** See R3C22.

20

**R3C27:** 5-Equation 1: Maybe this formula can just be cited in words. It is quite obvious.

**Answer:** We decided to keep this formula in the paper, because it gives a brief overview and is related to Eq. 2, 3 and 4.

**R3C28:** 5-Equation 2: What about lift forces? Are they included? Is a horizontal wind model used? If yes, which model is  
25 used?

**Answer:** Lift forces are included. We added an equation to clarify this. A horizontal wind model is not yet implemented, but this is planned for further research.

**R3C29:** 5-14: What is  $A_{ref}$  in this context. Isn't it the surface of one plate of the macro model and the total effect is the sum  
30 over all individual plates?

**Answer:** Cited from Doornbos (2011): “Note that the value for the reference area  $A_{ref}$  that appears in this equation and similar ones below, should be an agreed value for the entire space vehicle. Its value is not related to the dimensions or orientation of the single panel. It appears in these equations just to make the force coefficient dimensionless.” We chose  $A_{ref}$  to be the area of the front panel. The value does not matter, because as the reviewer points out, the drag coefficient is computed for each plate. In  
35 this computation, the factor  $A_i/A_{ref}$  occurs, so that  $A_{ref}$  cancels out in the end and the area of each panel is considered with

$A_i$ . We have clarified this in the text with “ $A_{ref}$  is a reference area that cancels out in the computation of  $C_d$  (more precisely in the computation of  $C_{D,i,j}$  and  $C_{L,i,j}$ , which will be introduced later), where the ratio of the area of each plate to  $A_{ref}$  is taken into account.”. We decided to refer to Sentman (1961) in the paper instead of Doornbos (2011), because this is the original source.

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**R3C30:** 5-15: add “relative to the atmosphere”

**Answer:** Done.

**R3C31:** 5-16: Give the most important keywords how the drag coefficient is computed. Especially how the underlying accommodation coefficient is chosen should be explicitly mentioned.

10 **Answer:** We have included an equation for  $C_d$  as well as more detailed descriptions.

**R3C32:** 5-19: replace “walls” with “surface”.

**Answer:** Done.

15

**R3C33:** 5-12: “Equation 3 accumulates SRP for each of the N plates ...”: A similar expression/statement is expected in the previous paragraph on atmospheric drag.

**Answer:** See R3C31.

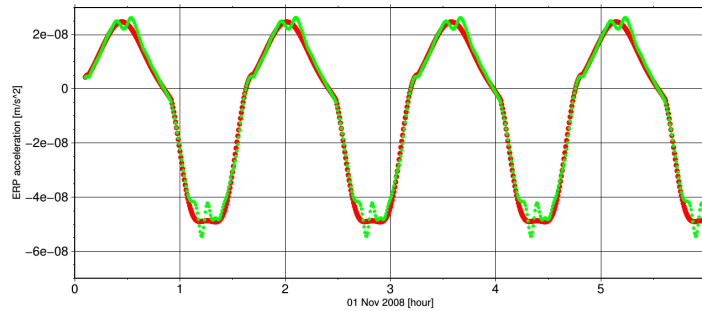
20 **R3C34:** 6-3, 6-4: “Different from the conventional implementation (Knocke et al., 1988), we expanded these data into a low-degree spherical harmonic representation to account for longitudinal variations” Can the differences to the conventional implementation be quantified? Is it relevant?

**Answer:** Representing the CERES data with low-degree spherical harmonics accounts for longitudinal variations, which is not possible with the approach from Knocke. This can be seen in the Figure below, which is taken from Vielberg et al. (subm.). It shows the ERP acceleration in the along-track direction of the instrument-fixed frame of GRACE A on November 1st 2008. The approach with spherical harmonics (green) is able to represent more variations than the approach from Knocke (red). This is most important in equatorial regions (minima and maxima in the plot).

**R3C35:** 6-7: Kinematic positions are not original observations but derived from the original GPS observations in a kinematic point positioning, which also provides covariance information of the estimated positions. Has this information been taken into account for the gravity field computation (at least epoch-wise)?

30 **Answer:** The reviewer is right, this sentence was misleading. We were talking about positions as “observations” in a least-squares adjustment. We deleted the word “observed”. We did not use any covariance information (neither temporal, nor 3x3 for the estimated positions) as this information is not provided for the official ESA orbits. By now, we do have the 3x3 information from personal contact with van den IJssel and plan to include it for further studies. Furthermore, our colleagues from

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**Figure 1.** ERP acceleration from Knocke (red) and ERP acceleration from spherical harmonic approach (green).

IfE Hanover will provide us with kinematic orbits, including 3x3 covariance information.

**R3C36:** 6-Equation 7: The short-arc approach allows in principle for a very simple quality analysis by comparing the overlap of the positions at the arc boundaries. Since I have never seen such an assessment for short-arc solutions, I am wondering whether it would be worth to be included? A statistics for the different solutions (mean, standard deviation) in the radial, along-track, and cross-track directions would be sufficient.

**Answer:** It is an interesting idea. The reviewer is right in the sense that the fitted boundary positions provide a POD solution simultaneously to the gravity recovery, whose quality is assessed by overlap analysis. However, we refrain from this test here due to three reasons: (1) The paper is on gravity, predominately, not on POD. It would be difficult to say what a good quality threshold would be. (2) Arcs are shorter than in common POD. It would be difficult to compare results. (3) Since the boundary values are reduced from the normal equation systems, it is not trivial to retrieve them without rebuilding and recomputing all of the solutions again.

**R3C37:** 6-16: “y contains all arc-related parameters, which can be eliminated during the estimation.”: I guess this means pre-eliminated, i.e. removed from the normal equation system but keeping the impact on the global parameters? Please confirm.

**Answer:** The reviewer is right. We added “... which can be eliminated from the normal equation system” to clarify.

**R3C38:** 6-16: replace “initial and final” with “start and end”.

**Answer:** Done.

20

**R3C39:** 6-21: replace “additionally estimated” with “parametrized”

**Answer:** We reformulated this sentence according to R2C30: “As we aim at a long and stable time series, we additionally parameterize a set of trends and (semi)annual harmonic amplitudes to the constant part for each Stokes coefficient in a single adjustment”.

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**R3C40:** 7-7: In the case of the short-arc approach, the force modelling errors are to a large extent prevented by the large number of new boundary values estimated at the beginning of each new arc. The allowed jumps between the short arcs act like empirical parameters that may absorb force model errors to a large extent.

**Answer:** The reviewer is right. However, we show in our study that the gravity field is improved when we introduce additional parameters that absorb errors in the modelled non-gravitational accelerations. This paragraph has been rewritten as proposed by Reviewer 2.

**R3C41:** 7-7: “sampling problems”: what does this mean?

**Answer:** This sentence has been reformulated: “Due to the presence of errors, e.g. caused by uncertainties in the density model or errors in the macro model, ...”.

**R3C42:** 7-8: “it is common to introduce additional parameters”: Depending on the chosen arc-length, the parametrization introduces more empirical parameters than other approaches. Is the “accelerometer bias” really needed if accelerometer data are not used?

**Answer:** The reviewer is right. The number of the empirical parameters depends on the chosen arc length. Figure 8 shows the influence of additional bias and scale factors on the ocean mass time series. For most of our best monthly solutions (considering the ocean mass time series) an additional bias has always been co-estimated (as can be seen in Figure 11).

**R3C43:** 7-11: “in that case a polynomial of slightly higher degree could be appropriate”: Is this statement based on experiments actually performed? Otherwise it can probably be omitted.

**Answer:** The reviewer is right. We decided to omit this statement. Intuitively, it would make more sense to increase the degree of the polynomial, because we want to allow for changes over the whole time period (compared to additional parameters per arc). Our tests (Figure 8, 10 and 12) do however not show, that a global polynomial of degree 4 is significantly better than a constant global parameter.

**R3C44:** 7-14: replace “controls” with “affects”

**Answer:** Done.

**R3C45:** 7-26: replace “intention” with “application”

**Answer:** Done.

**R3C46:** 8-Figure 1: Maybe a reference should be given from where the boundaries of the river basins are taken.

**Answer:** We added the reference in the caption of Fig.1: “The boundaries are taken from the Food and Agriculture Organization of the United Nations (FAO)”.

**R3C47:** 8-Table 3: “estimated until 40”: How was this threshold selected. The figure 10 in Dahle et al. (2017) shows that Swarm monthly solutions pass the signal curve in difference degree amplitude plots at about degree 60-70. How would the results presented in this paper change if Swarm would have the chance to contribute more?

**Answer:** The reviewer is right in stating, that the time variable signal from Swarm passes the static field curve at degree 60-70 when looking at the degree amplitudes. We do however believe, that this does not mean, that the Swarm time-variable signal is reliable until d/o 60-70. We think that one should rather look at the crossing point of error (GRACE-Swarm) and signal (GRACE). This is what we did in Figure 3 (dotted gray vs. red). This shows that the Swarm signal is only reliable for low degrees (in this particular plot even only until d/o 10, but in general until about 10-14). For this reason, we chose to evaluate our monthly gravity fields until d/o 10/12/14 (see Table 4). We estimated our gravity fields until higher d/o (20 and 40) because we believe that even though the signal is not reliable in the higher degrees, it might absorb errors in the lower degrees.

**R3C48:** Table 3: “trend per arc (pA1)”: Why is this needed? With the large number of boundary conditions in the short-arc approach (and the explicit modeling of non-gravitational accelerations) I would have expected that a bias should be sufficient.

**Answer:** As can be seen in Figure 10, our best trend/annual/semiannual solutions were computed with a bias consisting of a constant value and a trend per arc. However, only a constant value is leads to results that are almost as good. One possible explanation for the additional trend parameter might be the relation between the large number of observations (data from 37 months) to the relatively small number of parameters.

**R3C49:** 8-2: “Swarm alone”: Since a high-quality static gravity field model (GOCO05S) based on GRACE and GOCE data has been used, the presented solutions are not Swarm-only solutions. For completeness this should be mentioned at some point in the manuscript. To address in a strict sense what Swarm alone can do, one would have to replace the static field with a long-term solution stemming from Swarm as done, e.g. by Jäggi et al. (2016).

**Answer:** We added “relative to a reference (here GOCO05c)”. The reviewer is right in a strict sense. However, our interest is in the time-variability rather than in the static field and no time-variable GRACE (or GOCE) field has been used.

**R3C50:** 8-14: “In a next step, we substitute all degree 1 coefficients to correct for geocenter motion”: What does this mean? Are they estimated while stating that they cannot be estimated? Please clarify.

**Answer:** We did not estimate degree 1 coefficients, however, this was not clear from the discussion paper. To clarify, we replaced “substitute” with “add” and we added the sentence “We estimate the spherical harmonic coefficients from degree 2 onward.” in 7-7.

**R3C51:** 8-6: “buffer zone”: Specify how large this buffer zone is.

**Answer:** This sentence was confusing. It was meant in a way that we chose an ocean mask excluding (i.e. without) a coastal buffer zone. We restructured the sentence to: “We employed an ocean mask that includes the Arctic ocean and does not have a



coastal buffer zone.”

**R3C52:** 8-6: Is the effect of the reduced dealiasing product somehow restored for the presented ocean mass, or does the results just represent the "residual effect". Maybe a short comment should be given at the end of this paragraph.

5 **Answer:** The reduced dealiasing product has not been added back, because we wanted to concentrate on our results. However, this does not have any influence on the trends, because the GAD product has a trend of zero for the ocean basin. We added Figure 14 (b) and (c) (in the new manuscript), which show ocean mass from GRACE and Swarm with the GAD product added back. We included the paragraph “So far, ocean mass has been shown without adding back the GAD product from the German  
10 and the GAD product has a trend of zero for the ocean basin. Here, for better interpretation, we show ocean mass from GRACE for 2004 to 2013 (Fig. 14 (b)) and ocean mass from Swarm for 2014 to 2016 (Fig. 14 (c)) with the GAD product added back.”.

**R3C53:** 8-8: replace “otherwise” with “differently”.

**Answer:** Done.

15

**R3C54:** 9-Table 4: “static part until 20/40/60”: Similar remark as for Table 3. A static solution should have sensitivities up to much higher degrees. What happens if the maximum degree is significantly increased, e.g. up to degree 90?

**Answer:** See R3C47. We found that the quality of our time variable solution with trend, annual and semiannual signal does not very much depend on the static part, but rather on how the time-variable part is parameterized.

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**R3C55:** 9-5: “leads to our best solution”: Did you also compute a solution with non-gravitational accelerations not modelled but still a constant bias per arc estimated? This would show whether the non-gravitational modeling is indeed needed and would show how much you gain by the explicit modeling. I am asking because the experience from GOCE concerning the use of non-gravitational force modeling, where even highly-precise accelerometer data could be used, was rather limited for  
25 monthly or bi-monthly gravity field solutions (apart from degree 2). See figure 2 in

Jäggi, A., H. Bock, U. Meyer, G. Beutler, J. van den IJssel; 2014: GOCE: assessment of GPS-only gravity field determination. Journal of Geodesy, vol. 89(1), pp. 33-48

**Answer:** The solution with non-gravitational accelerations not modelled but still a constant bias per arc estimated is shown in Figure 5 (gray line). This is now clarified in the caption: “The only difference in IGG(not mod.) is that non-gravitational  
30 accelerations were not modelled, but a constant value per arc was still co-estimated.”.

**R3C56:** 9-6: “Table 6”: I am wondering whether part of the differences is caused by the different resolutions up to different maximum degrees in the individual monthly solutions. In order to decide whether this is relevant you could check for with monthly solutions from IGG resolved up to e.g. d/o 50 or 60 whether the picture is significantly changing or not. If yes, the  
35 different cut-off degrees might be responsible for the different noise behaviours.

**Answer:** We used the same cut-of degree (d/o 12) for all individual monthly solutions. This is now clarified in the caption: “The results are based on the time series of Fig. 2”.

5 **R3C57:** 9-7: “after the GNSS receiver update”: The Swarm GPS data are heavily affected by ionosphere disturbances (that were partly reduced after the GNSS receiver updates). As a consequence severe systematic errors are visible in monthly solutions along the geomagnetic equator (depending on ionosphere conditions). This aspect is not mentioned in the paper. Was it found to be not relevant?

**Answer:** We did not investigate this effect in this study. It probably only has a minor effect on our ocean mass tim series, because we look at the average over the whole ocean. This is however an interesting point that we will work on in the future.

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**R3C58:** 9-7: The updates of the tracking loop settings and their impact on the gravity field solutions are not mentioned in this manuscript. Are they not relevant for the derivation of ocean mass. See table 1 in Dahle et al. (2017).

Dahle, C., D. Arnold, A. Jäggi (2017): Impact of tracking loop settings of the Swarm GPS receiver on gravity field recovery. Advances in Space Research, 59(12), 2843-2854

15 **Answer:** We have not yet investigated the impact of tracking loop settings on the derivation of ocean mass. This is certainly foreseen for the future. We referred to the paper that the reviewer mentioned “The impact of tracking loop updates on gravity field recovery is discussed in Dahle et al. (2017).”

20 **R3C59:** 9-18: “our IGG solution for May 2016”: Which IGG solution is shown here? The best solution based on 30 min arc-length with modelled accelerations and a bias estimated per arc in all directions? I guess the solid lines in Fig. 3 (GRACE, IGG) represent differences wrt to a static field. Please explicitly state wrt to which model the differences are hown.

**Answer:** Yes, “IGG solution” always refers to the solution mentioned in Table 3, as mentioned in 8-8: “If not stated otherwise, we used the parameterization in Table 3 for monthly ocean mass...”. We included “with respect to our reference field GOCO05c”.

25

**R3C60:** 9-20: maybe add a statement like: “... due to the much lower precision of the GPS data compared to the GRACE inter-satellite K-Band ranging”.

**Answer:** Done.

30 **R3C61:** 9-21: “The formal errors (dotted black line) appear to be too optimistic, as they are always lower than the difference between GRACE and Swarm.” Maybe a statement could be given how to address this in future work.

**Answer:** Added “This will be addressed in the future by including realistic covariance information of the kinematic orbits.” Our colleagues from IfE Hanover are working on this topic.

**R3C62:** 10-Table 5: For AIUB solutions have been derived based on original and screened kinematic orbits. Which solution has been used to compute the ocean mass time series?

**Answer:** The results are based on the screened kinematic orbits. We have added this information in Table 5.

5 **R3C63:** 10-Table 5: Is this the max d/o of the solutions provided or the max d/o of the solutions used?

**Answer:** This is the maximum d/o provided, we clarified this in the caption. For the comparisons we used d/o 12. We corrected a mistake: Swarm ASU solutions are available until d/o 40 (not 50).

10 **R3C64:** 11-Table 7: How was GRACE treated to derive the numbers provided in the table? Cut-off at the same degree than for the Swarm solutions?

**Answer:** We used the same cut-of degree (d/o 12) for all individual monthly solutions. This is now clarified in the caption: “The results are based on the time series of Fig. 2”..

15 **R3C65:** 11-18: Are the gaps in the GRACE series also introduced in the Swarm series to estimate most comparable trends and amplitudes, or is the amount of data points different for the GRACE and the Swarm series?

**Answer:** We did not consider the monthly gaps for the Swarm trends, amplitudes and phases. This is now also included indicated by values in brackets.

**R3C66:** 12-Figure 4: Is this in the along-track direction of the local orbital frame or in the instrument-fixed frame?

20 **Answer:** This is the along-track direction of the instrument-fixed frame.

**R3C67:** 12-Figure 4: As mentioned in a previous comment, it would also be interesting to have a solution with non-gravitational accelerations not modelled but a constant bias per arc still estimated? Only one different setting in the processing would then be addressed and it would become clear whether the improvements are really due to the non-gravitational force modeling (or rather due to the estimation of additional empirical parameters per arc).

**Answer:** See R3C55.

**R3C68:** 12-4, 12-5: Please clearly declare whether the modelled accelerations are shown without the estimated scale or bias parameters.

30 **Answer:** Figure 4 shows the modelled accelerations without any bias or scale factors. We have now clarified this in the caption: “The black curve shows the ACC3CAL\_2\_ product from Siemes et al. (2016), while the red curve shows our modelled non-gravitational accelerations (without applying any bias or scale factors)”. A bias has however been co-estimated in the gravity field estimation process for Figure 5 (IGG (mod.)). We tried to mention this in 8-8: “If not stated otherwise, we used the parameterization in Table 3 for monthly ocean mass or ocean mass from a direct estimation of trend, annual and semiannual

signal terms is shown.”, but we now emphasized it as explained in R3C55, R3C59, R3C67, R3C73.

**R3C69:** 12-7: “and it also improves the trend estimate as can be seen in Table 8”: How can this be seen? Was there a solution with trend estimation when not using accelerometer data? Maybe I just missed it?

5 **Answer:** Yes, we talk about the ocean mass trend here. It can be seen in Table 8. The trend from GRACE is 3.5 mm/yr. Swarm with modelled non-gravitational accelerations (parameterization as in Table 4 (monthly)) has a trend of 3.3 mm/yr and the solution without modelled non-gravitational accelerations (still same parameterization, also with a constant bias) has a trend of 4.0 mm/yr.

10 **R3C70:** 13-Table 8: “same parameterization as IGG”: According to Table 4 the parametrization of the bias is different. Please clarify.

**Answer:** No, for both solutions a constant bias per arc was co-estimated, so the caption of Table 8 is correct. I think the reason for the misunderstanding and the comments R3C55, R3C67 and R3C73 is Table 4. It should be read in the following way: we tested arc lengths of 30 minutes, 45 minutes and 60 minutes (column 1). We tested non-gravitational accelerations either  
15 modelled or not (column 2),... Table 4 should not be read row-wise. A solution consists of one choice in each column. We tried to clarify this with a new caption: “Parameterizations that have been tested in this study. This table should not be read row-wise. It lists all possible choices for each heading. One solution can consist of any combination of the entries, for example: a monthly solution with an arc length of 60 minutes, modelled non-gravitational accelerations, a constant global bias, no scaling factor, max. d/o estimated: 40, evaluated until d/o 10.”.

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**R3C71:** 14-6: Could the authors give some explanation why the 30 min case is worst for the solution based on trends, annual and semi-annual signals, and essentially the opposite for the monthly solutions?

**Answer:** We are not sure about the reasons. It could be related to the ratio of the number of observations to the number of unknown parameters. Both differ extremely for the two cases.

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**R3C72:** 16-18: “have  $RMSE/RMS < 1$ ”: Maybe one should say that there exist some solution types with  $RMSE/RMS < 1$ . Especially for Ganges there seem to be many solution types with a ratio larger than 1.

**Answer:** This is right, we changed this to “For Greenland and Ganges mass estimates there exist some CTAS solutions with  $VAR/RMSE > 1$ ”. We use the variance instead of the RMS as suggested in R1C15.

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**R3C73:** 16-18: “modelling non-gravitational accelerations provides better results than not modelling them”: As mentioned before, I would like to see that this is not only due to the estimation of additional empirical bias parameters.

**Answer:** See R3C55.

**R3C74:** 16-19: Delete “Nevertheless”.

**Answer:** Done.

**R3C75:** 20-8: maybe it should be recapitulated up to which degree the Swarm solutions are solved for in this study.

- 5 **Answer:** As explained before, we believe that the time-variable part of the Swarm solutions is only reliable until about d/o 10-15. This is we we chose d/o 10, 12 and 14 for investigations. We did estimate our gravity fields until degrees 20,40 (and 60 for trend, annual and semiannual solutions) because higher degrees can absorb errors in the lower degrees.

**R3C76:** 20-12: replace “degree about” with “about degree”.

- 10 **Answer:** Done.