Authors response to the comments of referee #1

We thank referee #1 for the thorough and insightful review of the manuscript. The reviewer criticized our description of the methodology, the missing information concerning our error estimates, the validation by kinematic GNSS profiles only and the lack of 'numbers to back up most of the statements made'.

We have put much effort in rewriting the respective sections and think that this contributed to a much clearer presentation of the methodology and the results now. In order to describe the methodology in more detail, we have added significantly more details in the supplementary. We think that this is a good compromise between keeping the manuscript itself relatively short for the majority of the readers but having all technical details available for everybody who is interested. Error estimates have always been part of our processing but we agree, they have to be given their space in the manuscript as well. Therefore, we added respectively short descriptions in the manuscript as well as further details in the supplement.

Also the validation with IceBridge has been included now. However, we would like to stress that we are comparing the absolute elevation differences between two epochs. The magnitude of the difference is not important for this validation. Nevertheless, we agree that IceBridge contributes significantly more validation data in areas with complex topographies. Hence, this validation indeed provided important information. We included this validation but added a slope dependency to account for the topography effects.

To provide more 'quantitative facts' we added several correlation coefficients and key numbers. However, one of the major benefits of this work is the high temporal sampling of processes. Single numbers as linear trends, or even acceleration rates, can not always describe the underlying processes adequately.

Besides addressing these key issues, a major change in the revised version is that we have converted the volume changes into mass changes. With respect to some of the specific comments below, this indeed makes the comparison with the external data sets more meaningful, compared to the earlier version of the manuscript. We have decided for a rather straightforward and robust density mask approach, allowing us to still compare the SMBs as independent data set.

In the following we will respond the specific comments one by one.

Comment 1: Abstract – The abstract is quite long, but not well written. About half of the text is spent discussing the percent coverage for the 25 year and 40 year epochs, however these numbers aren't key scientific results so would be better placed in the data or methods section. Additionally, the coverage stats are poorly defined here, for example is this the percent coverage that of the raw data, plane fit output at whatever grid resolution used, or the extrapolated interpolated result. Without being specific the coverage stats are open to misinterpretation.
The abstract has been completely redesigned, being more focused. Besides the abstract, more details on our definition of 'observed' cells have been added in Sect. 3.3.3 and 5.2.

Comment 2: Abstract - The title of the paper states that this paper is about surface elevation change, but there are no key surface elevation change numbers stated in the abstract. Why not, if this is the main purpose of the paper?
The main focus of this paper are the time series of surface elevation change from multi-mission altimetry. We show that surface elevation changes are much more complex than just a single rate or even an acceleration. These variation are shown on a spatial and a temporal scale in the results section. In our revised version, we added a key value for the total mass change of the AIS since 1992 but also discuss the importance of the time series.

Comment 3: Abstract – The second paragraph of the abstract lacks any quantitative facts. For example, Pg1 L14 states that surface elevation change shows 'high coincidence' with precipitation anomalies and gravimetry; Pg1 L16 states that there is a 'high level' of agreement; and Pg1 L18 states that 'Geosat coincides very well with. . .'. The authors should replace these generic adjectives with quantitative statistics to back up their statements.
Respective correlation coefficients have been included in the document, but the respective sentence is no longer part of the abstract.

Comment 4: Abstract – Pg1 L15 – 'Satellite gravimetry' is the technique, but the authors have presumably compared their elevation results against a derived product, such as mass loss. Edit wording to be precise.
We agree, but in the revised version, this is no longer part of the abstract.

Comment 5: Abstract – Pg1 L18 – The Seasat and Geosat altimeters operated at radar frequencies and will consequently penetrate some depth into the snowpack, its therefore not a given that the elevation trend from these satellites should correlate with precipitation anomalies in snowfall. The penetration depth is spatially and temporally variable, influenced by snow density and moisture content. As the radar return originates from a scattering horizon within the snowpack, correlation between precipitation anomalies and rates of elevation change can't prove that the elevation change trends are 'reliable', so the satellite based elevation trends must be verified with a comparable elevation change dataset.
The penetration depth variations have been accounted for by the backscatter correction in Eq. (2) (except for Seasat, where the time period is too short). The offset corrections align the scattering horizons of the different missions. Section D in our revised supplement shows that the trend-corrected anomalies of the FDM and of the SEC differ by 0.12±0.21 cm for Geosat and 0.26±0.32 cm for Seasat (including one year without observations). Considering that the model is prone to some uncertainties as well, we think that this agreement is remarkable.

Comment 6: P1 L21 – Sentence wording not correct English, edit required. The wording throughout this paragraph is poor.
The introduction has been completely rewritten.

Comment 7: P1 L22 – Does the author mean sequentially rather than concurrently?
The introduction has been completely rewritten.

Comment 8: P2 L6 – Edit wording to say precisely what is meant. A long time series would not have prevented Wingham observing negative elevation rates in Dronning Maud Land compared with Flaments positive result for the same area, because as stated the observational time period is different. 'Help reduce the influence of such events' is factually incorrect.
The introduction has been completely rewritten.

Comment 9: P2 L10 – Edit wording. Mission calibration doesn't become 'more important', it is maybe more challenging though.
Changed.

Comment 10: P2, Fig1 – Add separate colorbar for map of spatial coverage as currently hard to interpret. Just the circle out- line that corresponds to bar color, but really hard to see in pole hole.
Changed.

Comment 11: P3 L1 – Edit wording to be more precise. As the raw data from both satellites was acquired in the same pulse limited imaging mode, the use of the word 'mode' to describe a processing choice could be misinterpreted. Additionally I think the two modes authors are talking about are ocean and ice retrackers, however there are actually 3 retrackers available for these missions (ice-1 and ice-2 are separate).
There seems to be a misunderstanding. The measurements of ERS have been switched between 'ice' and 'ocean' mode. See Paolo et al. (2016): 'To improve performance over the ice sheets, ERS-1 and ERS-2 operated in both a standard 'ocean mode' and a specialized 'ice mode', with mode switching based on an ocean-ice mask. For ice mode, the 64-bin range window (the segment of return echo that is recorded) was four times wider than for ocean mode (116.48 m vs 29.12 m), increasing the chances of capturing return signals over rough topographic surfaces.' We changed the wording to give a more self-contained explanation.

Comment 12: P3 L3 – Paolo et al presents results over flat ice shelves with zero slope, whereas this paper presents results over an ice sheet, where the most rapidly changing regions are found in the most steeply sloping terrain. The logic that Paolo used to justify including data from different imaging modes therefore may not apply to this paper. The authors should use data from a single retracker which has been shown to be more reliable, or quantitatively justify why including less reliable data improves the quality of the end elevation change result. E.g. via coverage, temporal extent, or reduced error maybe. It follows that a separate error estimate should be provided for the

elevation change result derived from different quality input datasets.
In this comment 'observation mode' and 'retracker' seems to be mixed up again. The retracker is a post-processing step, applied to the observed waveform. In contrast, one of the main differences between the observation modes is the onboard sampling of this waveform (see answer to comment 11). This question obviously refers to the observation modes. Both modes have been treated as independent data sets. For ERS-1 and ERS-2, we used individual a priori uncertainty estimates in the repeat altimetry processing (Eq. 2), estimated individual mission parameters and applied individual offsets. For each epoch where both modes exist, the monthly mean values have their individual error estimate. Hence, in the PLRA averaging step (Sect. 3.3.1), which combines the data, a poor quality of any mode would be reflected by a high RMS and, hence, a low weight in the average. We see no reason why we should remove one observation mode completely. A further discussion to the respective modes has been added in C.2.

Comment 13: P3 L4 – Specify the size of the bias between both modes, for both satellites.
This is done later in Sect. 3.3.1. The bias can be seen at Fig. S3.

Comment 14: P3 L16 – The Helm et al DEM is the ice surface during the first 3 years of CryoSat, however this surface has changed significantly in many regions throughout the 40-year study period. The ice surface of the DEM should evolve temporally to reflect the known elevation change, otherwise the slope correction will not be correct. This effect will be significant in regions such as WAIS which have shown to thin at a maximum rates of up to 9 m/yr. Have the authors done this, or if not what is the error on the slope correction that will result from not temporally evolving the ice surface for 40 years?
The absolute elevation of the topography has only a negligible influence on the location of the POCA. The POCA mainly depends on the relative topography, that is, the variations of topography w.r.t. the mean, over the footprint. If any changes of this relative topography occur, their rates are significantly lower than the rates of the mean absolute topography change (at least in regions that can be observed by pulse limited radar altimetry at all). Hence, we agree that such an effect exists, but its influence on the location of the POCA is negligible.

Comment 15: P3 L29 – Can the authors quantitatively state how much less sensitive to noise the OCOG retracker is compared to other options, and does this affect all satellites the same way given that the spatial resolution and imaging modes are different. Some retrackers will perform better over different terrain types (sloping or flat), therefore it would be helpful for more details to be provided about the region, time period, and satellites for which this analysis was performed.
Details on the comparison between functional fit and threshold retrackers have been added to Tab.1. The noise of different retrackers has been discussed in Schröder et al. (2017) as well, to which we refer at the respective locations here. Concerning the comparison between a waveform maximum and a OCOG threshold retracker, Bamber (1994) explain that the single 'maximum bin' is significantly more affected by noise as the squared mean over all bins. However, we did not apply this option, so we cannot provide any numbers.

Comment 16: P4 L3 – If the CryoSat retracking doesn't exactly replicate the methods in Helm et al, the full details should be detailed in this paper.
The retracking of the CryoSat-2 SARIn data was the method of Helm et al. (2014). We changed the wording to make this more clear.

Comment 17: P4 L13 – It is well known (as the authors later state) that anisotropy exists between ascending and descending tracks of radar altimetry data, and indeed many elevation change papers include a term for this in the plane fit solution to exclude any bias from this. When calculating data precision from ascending and descending tracks have the authors performed such a correction, and if so could further detail be provided on its size per mission.
We did not apply such a correction, but showed how the A-D bias is reduced by our low threshold retracker. Consequently, the precision from ascending-descending crossover differences includes the effect of the A-D bias as well. Table 1 and Fig. S2 show how the precision (including the effect of the A-D bias) is improved for each mission.

Comment 18: P5 L5 – Edit text to state quantitative statistics about the absolute or percent improvement following their slope correction. Frustrating that 'superior performance' and 'similar improvements' used when a number would be more persuasive.

Numbers added and text modified.

Comment 19: P5 L17 – Figure S1 does show a reduction in the anisotropy effect, but the signal is clearly still present in the data. Additionally, smoothing a result with an artefact in doesn't remove the affected data, so this error will clearly have an effect on the end result. If as the authors state recent previously published studies have designed and successfully implemented an anisotropy correction, this paper should add this step to avoid unnecessary error. If the authors chose not to do this, I would ask that the quantify what the affect of not applying the correction is to prove its not discernable.
Firstly, the mentioned studies use data which have been retracked using a functional fit retracker, where the effect of the A-D bias if much larger (see Fig. S2). Secondly, we do not simply smooth data with a systematic offset. When averaging ascending with descending data (which are both affected by the A-D bias, but with opposite signs), the result will not contain a bias any more. The bias will only affect the resulting uncertainty estimate (see answer to comment 17). We average ascending and descending tracks (with typical cross-track distances of less than 20 km) over 60 km. Hence, we usually average 3x3 ascending and 3x3 descending tracks (over 3 months). This perfect constellation will not always be true but, however, also the alternative, the application of a A-D bias correction might contain some issues. As this discussion belongs to the repeat track processing, we have moved it to C.1 and discussed this point there.

Comment 20: P6 L25 – If there are differences in the processing methods used for different missions as stated, this should be fully specified in the supplementary material. Use of full parameter names as found in the mission meta data will ensure that the methods and results presented are repeatable.
These 'parameters' refer to the parameter fit (Eq. 1). We have rewritten the whole paragraph.

Comment :21 P8 L1 – Spelling
Obsolete due to edits.

Comment 22: P8 L1 – Specify the thresholds and variables against which data is filtered out during the elevation change iterative processing. Are the same values used for all missions?
More details added, moved to suppl. C.1.

Comment 23: P8 L7 – Provide some detail on how the backscatter penetration correction is calculated and applied. E.g. over what epoch?
The backscatter penetration correction is applied according to Eq. (2). For each repeat cell and each mission (except Seasat and ICESat) a parameter dBS was estimated. By not including dBS in Eq. (3), the resulting time series are backscatter corrected. This has been explained in more detail in Sect. 3.2 and a further discussion was added to C.1.

Comment 24: P8 L18 – State threshold used to determine outliers.
Each processing step is described in much more detail now in the supplement.

Comment 25: P9 Fig4 – Red colors in this plot do not print well so can't easily differentiate missions. Change color scale used.
Done.

Comment 26: P9 L3 – The authors are simultaneously arguing that for all of the more recent missions a spatially variable offset correction must be applied as the offset is spatially variable, while stating that for Seaseat and Geosat the offset correction must be a constant because some of the difference could may be real elevation change. If the later is true, why does this not also hold for the more recent missions? The fact that a spatially variable correction can't be or hasn't been calculated doesn't remove the justification for why its needed.
For the recent missions we use overlapping epochs. The differences, used to calibrate these missions, refer to the same time (within one month), hence, real elevation changes do not play a significant role here. We believe that the true offsets between Seasat/Geosat and Envisat are spatially variable, just as the offsets between ERS-1/ERS-2/CryoSat-2 LRM and Envisat. However, in contrast to ERS-1/ERS-2/CryoSat-2 LRM, we are not able to estimate the spatially variable offsets for Seasat and Geosat over their region of coverage. This is because Seasat and Geosat have no temporal overlap to the later missions, so that actual elevation changes between the mission times are an additional source of error in the offset estimation. Therefore, our final estimate of the Seasat and Geosat offset is constant in space. The assessed spatial variability of the offsets is in turn included

in the uncertainty estimate and makes the estimate much more uncertain than for ERS-1/ERS-2/CryoSat-2 LRM. It is legitimate to adapt the offset estimation to what is possible, as long as the uncertainties are adapted accordingly. We clarified this in our explanation of the Geosat/Seasat offset estimation.

Comment 27: P10 L15 – The extrapolation and interpolation steps are not sufficient. It doesn't account for the spatially variable pattern of thinning, which increases towards the coast and is larger on fast flowing ice streams, and the method is not fully described. What is the maximum distance over which gaps are filled? In areas such as the ice sheet edge or on the Antarctic peninsula, which receive exceptionally poor coverage in earlier missions, how are these larger gaps filled? Equally, in order to state EAIS, WAIS, and continent wide thinning rates, the pole hole needs filling.
We explicitly do not perform an extrapolation to unobserved regions (not at the margins and not in the polar gap as well). This was achieved due to the criterion of different sectors around the cell, which need to contain data. Only cells which are surrounded by observations were filled. During the revision of the manuscript, we modified this criterion to be even more strict. In our final grid, now, we calculate a value only for 10x10 km cells that are within a beam-limited radar footprint of repeat altimetry results. A more detailed description has been added to Sect. 3.3.3 and C.4.

The 'Results' and the 'Discussion' section has been completely rewritten, so many of the following comments have been considered but do not apply directly to the revised version.

Comment 28: P10 L31 – The signal in the 1978 to 1992 map is extremely noisy with lots of variation over short spatial scales. Although the authors assert that 'coherent signal' can be obtained from these missions, to me it looks like the differences are as great if not greater than the similarities between the later data. Generate a difference map, or statistics, to quantitatively demonstrate that the results form the early missions are 'coherent', or similar to those from later missions.
We agree that this point should have been explained in more detail. Differences to the results over later periods logically arise due to interannual variations. However, as the whole section has been redesigned, this does not apply to the revised version any more.

Comment 29: P10 L32 – The authors attribute elevation change across the ice sheet to ice dynamics without providing evidence in support of this. Elevation change can be caused by dynamic ice thinning, a snowfall anomaly, change in the scattering horizon, or measurement error, so all of these factors will influence the result not just dynamic thinning alone. Which specific regions are attributed to dynamic change? If based on previous publications, please provide relevant citations. The authors should quantify how much of the elevation change is dynamic, vs all of these other factors, in order to demonstrate that it's the largest contributor.
In the revised version of this section, this very important remark has been taken into consideration carefully.

Comment 30: P10 L33 – State what's classed as a short time scale, annual/ sub-decadal/ other?
This does not apply directly to the revised version any more but has been taken into consideration in the wording.

Comment 31: P11 L1 – Again maps of elevation change are not evidence of change in dynamic thinning without additional supporting data. The authors need to quantify and rule out the influence of snowfall variations, change in scattering horizon, and error, and a corresponding change in ice speed should also be observed. Without this elevation change due to long term, decadal fluctuations in snowfall, may be mischaracterized as dynamic change. The authors should also state which regions they are referring to.
The passage has been rewritten.

Comment 32: P11 Fig5a – Add distance markers to the flow line on one of the maps, hard to tie 5b to locations along it. For example, what distance is the limit of Seasat and Geoset data?
With regard to this and the following comment, this figure has been replaced (now Fig. 9). Both comments would have been very difficult to apply while still keeping the figures readable.

Comment 33: P11 Fig5b – Add error bars to this plot.
See answer above. Figure 9 now contains error bars.

Comment 34: P12 Fig6 – Provide a spatially variable error map for each of these epochs. So far the results have been presented without any error method described, or measurements included in the plots.
Respective maps have been included in the supplement.

Comment 35: P12 L4 – Edit text. The discussions aren't controversial, there is just are just different approaches each of which have advantages and limitations.
Changed.

Comment 36: P13 Fig7 – Add error bars to all lines on this plot.
We added error bars to our altimetry results. Error bars for all data would be hard to identify in the plot.

Comment 37: P13 Fig7 – Clarify how the % coverage has been calculated, and add coverage labeling to an axis. For Antarctica and the LPZ, I don't see how it can be 100during the 1990's when the pole hole is not observed. Additionally, in the methods its stated that the raw data was originally gridded at 1km resolution, which will result in data gaps of several kilometers between tracks before the CryoSat's precessing orbit comes online. So again I struggle to see how such complete observational coverage is achieved, unless extrapolated and interpolated data is classed as an observation, which of course it isn't. Could the authors clarify?

The gridding is described in more detail now. The percentage of coverage serves as ancillary information for the interpretation, only. We think a label is not necessary to see when the coverage was almost 100% and when it was only 80%. Instead, we want to keep the plot itself as large as possible.
Concerning the polar gap: The caption says 'of the Antarctic Ice Sheet north of 81.5°S', which means *excluding the polar gap.*
Concerning the distinction between 'observed' and 'unobserved' we would like to stress that the majority of the 'raw' data in fact has beam-limited footprints of 20 km. We process the data at each kilometer but within overlapping circles of 2 km in the parameter fit. We calculate a value for our final gridded result (with a resolution of 10 km) only if the closest data is less than 20 km away (in the TCD version, the applied criteria to decide, whether we calculate a value or not was different, but the effect was similar). Hence, as we do not extrapolate to regions which are not close (in terms of a footprint) to data, we call our final result 'observed'.

Comment 38: P14 Fig8 - Add error bars to all lines on this plot.
See response above.

Comment 39: P14 Fig8 - Add % coverage axis label to plot. Same comment applies about how the coverage calculation is done.
See response above.

Comment 40: P14 Fig8 – Add a table in the SOM with the areas of the drainage basin sub-regions used to generate this plot.
Done.

Comment 41: P15 Fig9 – I don't understand the figure caption, please rewrite more clearly.
Removed during revision.

Comment 42: P16 L1 to 14 – These results sections are very poorly written as no actual results are described! The authors just state what some of the figures show, and leave the reader to do all the hard work of reading off numbers and key statistics, comparing this with numbers they have read in previous studies. Rewrite the results section to present some actual results. I don't think a single elevation change number has been presented in the text yet, despite that being the title of the paper!
Completely rewritten.

Comment 43: P16 L9 to 13 – The authors have described what this plot is, but haven't explained why it matters or what the key scientific result is. Either remove figure 9 or explain why its an important addition.
This is obsolete now.

Comment 44: P16 F10 – Label y axis of b, presumably count. Edit figure caption to state time period data validated over.
The validation has been completely revised, this is obsolete.

Comment 45: P16 L15 – The validation performed in this paper is completely insufficient, and I would argue it leaves the result presented essentially unvalidated. Use of only 19 GNSS profiles, in a region of no known change, over a limited time period and spatial extent, and on unchallenging flat terrain, does not inform the reader about the validity of these results. At a minimum the authors must use a more comprehensive independent dataset, e.g. ice bridge.
We agree, that a comparison with IceBridge could be interesting and included it.
Nevertheless, we do not think, that the previous validation leaves the results 'essentially unvalidated'. The elevation change is not important here. Our validation analyzes if both data sets see the same elevation change between two epochs. It doesn't matter if this is 1 cm or 20 m. The temporal coverage of IceBridge (2002-2016) is practicably the same as for the GNSS profiles (2001-2015). However, we agree that the coverage of more challenging terrain by IceBridge is also interesting. For this reason, we now validate with both data sets and made our validation now slope dependent.

Comment 46: P17 L2 – This 'validation' cannot be interpreted as an error. A formal error budget based on the altimetry data itself must be documented and added to the plots in this paper. Validation and error estimation are separate things.
Changed.

Comment 47: P17 L14 – The authors don't need to limit their validation data to in situ measurements, much more spatially and temporally extensive airborne data is available and this should be used.
From a satellite point of view, we consider also airborne data as in situ. However, the sentence this comment refers to is about 'the earlier missions'. We would be happy about any suggestions on pre-2000 validation data with more than 'a limited time period and spatial extent'.

Comment 48: P17 L21 – State the number, don't leave the reader to guess how much elevation change you have measured! Presumably it is different for the peninsula and east Antarctica, so again please present your result.
Numbers and some more details added.

Comment 49: P17 L22 – Add figure number.
Location of the reference changed.

Comment 50: P17 L23 – Add figure number, or label somewhere. Location of ice streams mentioned hasn't been identified on any plots in this paper. State quantitatively how your numbers compare with this thinning rates presented by Rignot (2006), the time period is different so there should be something new to say.
The location of the glaciers have been marked in Fig. 10b now. The Rignot (2006) paper uses the input-output method. They map the ice velocity and use these values to obtain ice mass balances. Quantitative numbers for thinning are not given there. Anyways, we added our maximum thinning rates for the respective glaciers to allow for such a comparison in future.

Comment 51: P18 L2 to 6 – Use statistics to show the agreement, or disagreement between the elevation change and precipitation anomaly. State with numbers what 'significant difference' is that allows ice dynamics to be determined. State how far inland the thinning was in earlier decades vs how far inland it reaches now.
This passage is largely edited. We now set the plots to be compared (Fig. 14 now) side by side. We also added a whole section concerning the comparison between SEC from a firn model and from our altimetry data (Sect. 4.2). Correlations for the basin mass time series can be found in F.3. The inland thinning was well reported by Konrad et al. (2016), which is cited here.

Comment 52: P18 L14 – State the threshold used to determine a strong snowfall anomaly. It looks like it varies just as much at different times around other regions in Antarctica.
Obsolete in the revised version.

Comment 53: P18 L15 – what distance away from the grounding line were Seasat and Geosat *typically able to observe.*

This strongly depends on the topography, the state of the tracking window loop and the orbit direction. It is now discussed concerning Fig. 9 and can also be seen in Fig. S3.

Comment 54: P18 L20 – The 12 m/yr thinning suggested by Li et al was due to grounding line retreat between '96 and 2013, however the 12 m thinning present in this paper is for 1985 – 2010. Given that the measurements presented in this paper start approximately a decade earlier, if as this paper sats the glacier was already thinning in the 80's then the magnitude and rate are not in agreement with Li et al. Please clarify.

This section has been completely rewritten.

Comment 55: P18 L 23 – The authors need to take more care before attributing elevation change to dynamic ice mass loss. There are many signals present in their continent wide maps that may well not be attributed to dynamic ice loss. There is not consensus in the published literature that all Antarctic peninsula elevation change is dominated by ice dynamics, and the authors themselves later attribute a different elevation change signal on the peninsula to precipitation anomalies without providing any more or less evidence that a different process could be responsible (P18 L30). The authors must present quantitative evidence to support their claims either way.

The wording has been changed to be more precise.

Comment 56: P18 L30 – GRACE data cannot disentangle whether elevation change is caused by snowfall anomaly or ice dynamics, as ice mass is lost in both instances. Only velocity data can demonstrate whether ice was exported from the catchment at an increased rate, proving ice dynamics. Both dynamic thinning and snowfall anomalies result in mass loss, but gravimetry mass loss measurements don't show which of these two different processes might be the cause.

This was a misunderstanding. We were arguing that we see an anomaly and that ERA-Interim sees the same, hence it is very likely a snowfall anomaly. GRACE just confirms the anomaly, not the origin. We changed the wording to be more precise.

Comment 57: P18 L35 – Quantify very well.

Obsolete.

Comment 58: P19 L10 – In Fig S7c I can't see any 2002 step in the precipitation time series so its not clear to me that there is good agreement. Again please provide quantitative stats to back this up, rather than just making unsupported qualitative statements. Cite Lenaerts et al 2013 with respect to the 2009 and 2011 precipitation anomaly results as not a new result from this paper.

Edited.

Comment 59: P19 L16 – While this appears to be true for 2008/10, there is a more significant accumulation gain in the 1990's that is not visible in the elevation change result at all. This is in part because the authors are comparing different things, snow mass anomaly, vs elevation change. Direct comparison not possible unless elevation change converted to mass change.

The volume to mass conversion has been included in the revised manuscript. The respective difference in the 1990's, however, is still present. The correlation in Fig. 8a shows that there are regions where the interannual variation of the FDM and of the altimetry do not agree very well. A more detailed analysis what causes this specific disagreement would be very interesting but is beyond the scope of this paper.

Comment 60: P20 Table2 – There is negligible data coverage outside of East Antarctica prior to 1992, so not valid to include an Antarctic wide number for the '78 to 2017 period in row 3. Remove this number as misleading.

Done.

Comment 61: P21 Conclusions – There are no key results from this paper presented in the conclusions. Add a few key numbers.

Done.

Comment 62: SOM A.1 – State threshold used to determine if noise is too high.

This is a flag, contained in the data from GSFC.

We have listed all the flags and criteria for data editing in a descriptive way (which also applies to the following data). For some of the datasets (as from GSFC), the data does not contain fixed names. The documentation does only contain a description to the parameters. The ERS data comes with auxiliary files containing additional flags, not included in the binary data files. Furthermore, the ERS data contains outlier in the time tags ('time jumps') as reported by the RA L2 Validation Report. Some of them are flagged but we found several outliers in timing also in the remaining data. All those details are very technical and cannot simply be listed as 'flags and thresholds'. We think the commonly used descriptive text is sufficient as it is done by a range of other publications (Smith et al., 2009; Pritchard et al., 2012; Fricker and Padman, 2012; Sørensen et al., 2015; Paolo et al., 2016) (while many others don't mention data editing at all).

Comment 63: SOM A.1 – State the start and end date for each satellite dataset used.
Very good point. Table added.

Comment 64: SOM A.2 – State which retracker the elevation measurements were derived from.
Our own, see Sect. 2.1.

Comment 65: SOM A.2 – State the specific name of the metadata flag used to filter out data, and if a threshold was used, state the number that this was set at.
See above.

Comment 66: SOM A.2 – Adjust Figure 1 to reflect the actual time period of ERS-1 data used. (same applies for all missions)
Done.

Comment 67: SOM A.3 – State which retracker the elevation measurements were derived from.
Our own, see Sect. 2.1.

Comment 68: SOM A.3 – State specifically which measurement confidence flags were used, and again if a threshold was used, state the number that this was set at.
See above.

Comment 69: SOM A.4 – State specifically which measurement confidence flags were used, and again if a threshold was used, state the number that this was set at.
See above.

Comment 70: SOM A.5 – State which LRM retracker the elevation measurements were derived from.
See above.

Comment 71: SOM A.5 – State specifically which measurement confidence flags were used to filter data, and again if a threshold was used, state the number that this was set at.
See above.

Comment 72: SOM B – Edit title and section text to be more specific as its unclear specifically what the authors have reprocessed? Is it that the elevation measurements have been retracked? Read as a stand alone section I don't know what
The section has been edited accordingly.

Comment 73: SOM E S6 and S7 - Add error bars to all lines on this plot. Add % coverage axis label to plot. Same comment applies about how the coverage calculation is done.
Error bars added to altimetry. Concerning the %-label, we refer to our answer to comment 37.

References

Bamber, J.: Ice Sheet Altimeter Processing Scheme, Int. J. Remote Sensing, 14, 925–938, 1994.

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