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Interactive comment on "Four decades of surface elevation change of the Antarctic Ice Sheet from multi-mission satellite altimetry" *by* Ludwig Schröder et al.

Anonymous Referee #2

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This study presents the first complete time series of Antarctic land ice elevation changes obtained by merging all radar and laser altimetry data since the late 1970s. Prior to merging the data from different missions, they reprocessed the radar altimetry data using an OCOG threshold retracker and POCA relocation method of the laser altimetry footprint. The merged data set is spatiotemporally interpolated to produce monthly ice sheet elevation changes with a 10 km resolution for 07/1978-12/2017. The results are presented in various ways, including volume change time series of major drainage basins and annual elevation difference grids and compared those with mass changes from GRACE and precipitation anomaly related mass changes from ERA-Interim.

I really wanted to like this manuscript. The study aimed to solve an ambitious goal by generating a four-decade-long time series of ice sheet changes in Antarctica. However, the authors did not have specific, well-defined objectives, beyond to “identify rapid changes associated, e.g., to snowfall events as well as long-term changes as, e.g. due to changing ice dynamics over nearly four decades”. Unfortunately, the manuscript failed to convince me about the successful merging the different altimetry data sets. Also, beyond presenting the results of the new reconstruction, it did not provide any new insight into the behavior of the Antarctic ice sheets. Moreover, the authors did not place their methodology and results in the context of previous work. Multidecadal time series were developed from different radar missions by other studies, such as Fricker et al., 2011; Paolo et al., 2015. While most previous results were limited on ice shelves, where the smooth, flat topography makes change detection easier, lessons learned from those studies should have been summarized and advantages of the new approach should have been presented.

Numerous assumptions and simplifications are employed during the processing. However, the assessment of their impact is missing as no error models, and error estimates are presented. Error estimates could have been derived from rigorous error propagation or by a comprehensive comparison between the multisensor satellite altimetry time series developed in this study and repeat observations for example from OIB airborne altimetry. The only validation example from repeat from repeat kinematic GNSS is over the smooth, high elevation region of the EAIS between 2001 and 2015. Thus, the derived error estimates do not apply for the rugged, undulating coastal regions, where more significant changes occur or for the missions before 2000 (Seasat, Geosat, ERS-1). The ice sheet thickness change reconstructed in the low precipitation zone (LPZ), defined as the area where the average annual precipitation is less than 20mm/yr water equivalent from ERA-Interim indicates a good agreement with the ERA-Interim and small seasonal changes after 2003 (Fig 7.e). However, very large average thickness changes, up to 0.1 m in half a year (!!), with a seasonal pattern suggest unremoved systematic errors before 2003. This large seasonal variation is clearly an artifact that should be investigated as it can shed light on the errors and their distribution of ERS-1 and ERS-2 altimetry.

The proposed approach includes a series of assumptions, most notably the assumption of linear temporal changes, that is violated over several rapidly changing regions. No attempt is made to assess the impact of these assumptions. The issue of non-linear change is relevant to several steps. (1) Reconstruction of elevation change time series. Equation (1-3) works well for linear temporal trends, for surfaces that can be approximated with planar surfaces within the search radius and when a linear relationship exists between the backscattered power and the corresponding elevation correction. In this case, res_i will provide a measure of random errors. In all other cases, e.g., non-linear temporal trend or non-planar surface shape or non-linear backscatter correction, res_i will include both measurement and modeling errors. Moreover, the non-linear component of the temporal change is determined from res_i. Therefore, the detection of outliers is not an easy task and should be solved in an adaptive fashion. The manuscript fails to explain this critical step in sufficient detail. (2) The step of merging the time series from different missions also assumes a linear trend, a linear trend of temporal elevation changes. It is not clear if the linear temporal trend is assumed to be the same for all missions, same for different subsets of missions (e.g., ENVISat, ICESat, CryoSat-2, page 9, lines 24-35, page 10, lines 1-2) or could be different for each mission.

An evident deficiency of the study is that the elevation change is not converted into mass change. Solutions for estimating mass changes from thickness changes have been applied by several authors (see, for example, Shepherd et al., 2012 and 2018, and references therein). Presenting the results as mass changes would enable quantitative comparison of the results with other studies, and thus validating them against those. The comparison of measured thickness/volume changes with modeled precipitation from ERA-Interim (expressed as mass) and mass changes from GRACE gravity have insufficient value because of the complexity of the thickness/mass change relationship.

As mentioned above, the comparison of the ice sheet thickness change time series with ERA interim precipitation anomalies has major caveats, due to the complicated relationship between the two geophysical parameters. However, the availability of the two detailed time series would easily lend itself to a statistical analysis of their relationship, ranging from regression analysis to more sophisticated investigations. Instead of pursuing a statistical analysis, the authors relied on visual comparison. The interpretation and discussion section lists some similarities between reconstructed thickness changes and precipitation anomalies in a somewhat ad hoc fashion. Most of these patterns have already been described by other authors and the manuscript fails to provide a new insight into the surface processes and ice dynamics acting on the Antarctic Ice Sheets.

The results presented as "grids of surface elevation change (SEC) with respect to 09/2010 for each month observed and at a 10 km spatial resolution" (page 10, line 10) and as "elevation changes from year to year" (page 16, line 1). These geophysical parameters do not appear to be carefully designed, they are not explained properly and have misleading names. For example, the SEC is ice thickness (GIA corrected elevation change) rather than ice surface elevation, relative to the reference time, which usually (e.g., for linear trend) changes its sign at the reference time. Thus, regions characterized by thinning have positive values before the reference time and negative values after the reference time (see, for example, Fig. 5.a). As for the elevation changes from year to year, they appear to be differences between average annual ice thickness values of consecutive years. Therefore, they could be called as annual ice thickness rate change. The difference between the ice thickness at the end of two consecutive the balance years might be a better parameter to estimate annual ice thickness change rates as often used by other studies.

The description and interpretation of the change patterns in section 5 (Interpretation and Discussion, pages 18-20) is very difficult to follow. A few simple change would help, such as including an index map that shows the drainage basins (with letters), the

LPZ, and the major geographic names over a background of thickness changes (e.g., Fig. 6.b) and labeling the panels in Fig. 8 and Fig. S6-S7 by the drainage basin/glacier names. The description of the changes should be better organized, both in space and time.

Detailed comments: Throughout the paper the reconstructed changes are described as ice sheet elevation changes. However, changes due to vertical crustal deformations (GIA) have been removed from the reconstructed elevation change rates (page 10, lines 32-34). Therefore, it would be more appropriate to call the parameter ice thickness change.

Abbreviations should be spelled out when they appear first, e.g., ESA, SARIn.

Page 2, lines 15-16: use release numbers instead of “most recent”.

Page 3, lines 10-11: add beam limited, i.e., approximately 20 km “beam limited” footprint; lines 14-18: more details are needed to explain on how to find the POCA; line 24: ICE-1 and ICE-2 methods need to be described; line 25: remarkably higher precision than what?

Page 4, line 22: spell out CFI retracker, include reference.

Pages 6, 7: it would work better to explain first why the planar surface approximations are different for the different missions, followed by the equations.

Page 7: outlier detection procedure should be explained in detail.

Page 9: explain the use and effect of the moving median filter.

Page 10, lines 2-4: provide more details on the spatiotemporal smoothing, why was it performed and how effective was it? Line 10: explain the definition of “each month observed”. Is there a minimum number of observations or spatial coverage? Line 15: how are the surface elevation change rates determined? Are these average rates determined by straight line fitting in temporal domain?

Page 16, lines 10-12: the error of the trend (slope of the linear fit) is not the standard deviation from the linear fit and can easily be estimated from the data.

Page 18, line 4-6: the long-term trends over Kamb Ice Stream and Totten Glacier have been detected earlier, for example by Zwally et al., 2015.

Page 18, lines 29-34: it is not clear what this statement refers to: "Around kilometer 600 where the profile bends into the main flowline of Totten Glacier, we see a significantly rising elevation. The profiles at different epochs reveal that this is not a continuous change but that there is a distinct jump in the early 2000s." Maybe a different representation and a more detailed explanation would help.

Table 1: $\sigma_{constant}$ is a misleading parameter name – σ_{flat} or $\sigma_{noslope}$ might be better. Figure caption should include the type of retracker used, i.e., 10%-threshold retracker from this study. Better yet, a comparison of the performance of the different retrackers (from Fig. 2, Fig. S2) could be compared in this table.

Figure 1. The southern extents of the different radar altimetry missions are not clearly presented in the left panel.

Figure 2. ICE-2 retracker is mentioned in this figure caption only, not in text. Needs more explanation.

Figure 4. Time axis labels should be fixed. Describe vertical axis. Should show the combined time series.

Figure 5. Define the yearly mean surface elevation change.

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