Authors response to the comments of A. Shepherd.

We would like to thank A. Shepherd for the very helpful and insightful comments. We have made a major revision of the manuscript, added a detailed description of our uncertainty estimates, a validation with IceBridge data and converted our final results from volume to mass.

Comment 1: Title. The title is misleading; a minority (25%) of the data set spans 4 decades. It should be modified to explain this or address the majority data set
The title has been modified so it doesn't imply four decades for the 'whole' Ice Sheet any more.

Comment 2: Error budget. The authors use the variance of single cycle crossover differences as a measure of error, and conclude that the reduced variance offered by their preferred retracker indicates a de-facto improvement in error. This is misleading, as their conclusion is entirely related to their choice of error metric and is therefore subjective. To conclude an improvement the authors should evaluate each retracker against independent observations of greater and known precision.
We did this by validating our retracked data as in Schröder et al. (2017). This shows similar improvements. However, as explained in detail in the manuscript, this is a measure for accuracy, not precision. Such a validation imposes systematic errors due to the different sampling of topography, which has to be considered when absolute elevations are important. With respect to elevation change detection, we chose the precision (or 'repeatability') as a measure for uncertainty. This is discussed in Sect. 2.3.

Comment 3: Methods. The authors discuss that a variety of approaches have been used to derive continental scale elevation change measurements, leading to apparently large differences in solutions, and yet they present only one solution. The reader is unable to assess whether the presented solution is optimal. The authors should show how the choice of power correction, firn correction, retracker, elevation change solver, spatial and temporal sampling, spatial and temporal interpolation, and mission cross calibration, influence the final product.
A description for our uncertainty estimates has been added which assesses the uncertainty of the respective data. However, we would like to stress that the method of repeat track parameter fit is well established. The choices we made are based one the results of previous publications as cited at the respective places in the manuscript.

Comment 4: Validation. Great efforts have been made by others to acquire independent elevation change measurements in Antarctica, for example NASA Icebrige. The authors should make use of these measurements to evaluate their satellite product, and their estimated error budget, in support of their claims that it offers improved accuracy and is optimal.
The validation with IceBridge has been included.

Comment 5: Comparison to GRACE and ERA. I don't understand why the authors have compared altimeter volume changes to mass changes and precipitation anomalies derived from GRACE and from ERA Interim. These are not equivalent, and so a side-by-side comparison has no meaning. There is potential value in contrasting these measurements, if they are each worked up to a common unit such as mass, but that requires more work.
The volume-to-mass conversion has been included in the revised version. Instead of ERA, we now use RACMO and the respective FDM.

References

Schröder, L., Richter, A., Fedorov, D., Eberlein, L., Brovkov, E., Popov, S., Knöfel, C., Horwath, M., Dietrich, R., Matveev, A., Scheinert, M., and Lukin, V.: Validation of satellite altimetry by kinematic GNSS in central East Antarctica, The Cryosphere, 11, 1111–1130, https://doi.org/10.5194/tc-11-1111-2017, 2017.