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## Comment on **essd-2021-287**

Veit Helm (Referee)

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Referee comment on "Elevation change of the Antarctic Ice Sheet: 1985 to 2020" by  
Johan Nilsson et al., Earth Syst. Sci. Data Discuss.,  
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Review: Elevation Change of the Antarctic Ice Sheet: 1985 to 2020  
Nilsson et. al.

The paper provides a new data set of elevation change of Antarctica based on analysis of satellite altimetry.

The authors put in a massive amount of work to provide this comprehensive dataset. Thanks to the authors.

The authors used for most of the time L2 data from earlier mission for their study. They applied a slope correction (similar to Schröder et.al.) and which is new, included ICESAT(2) data in their study. For CryoSat-2 they applied an independent processing using L1B waveform data.

A new approach to derive elevation change is the decoupling of time-variable and static topography.

Then both spatial and temporal patterns of changes in the scattering horizon are estimated and corrected for using waveform parameters and sigma in a separate step.

Combination of the different mission, which is very challenging was done using a least square approach which also differs from other studies.

Another new step is the amplitude normalisation to further reduce seasonal amplitudes explained as residuals of not fully corrected radar scattering effects.

Finally an extrapolation method is suggested to fill the pole hole south of 81.5°/88° to provide a complete dataset for Antarctica to be used e.g. for model input.

The new data set shows slightly higher accuracy than the TUD and CPOM dataset when

compared to ATM or the 2003-2019 ICESat(2) data set of Smith et.al.

To my opinion the paper as well as the data set is of high quality.

The dataset could be accessed through the given link.

I think the data set is significant, unique, useful, and complete and of value for the community and worth to be published.

Language and figures are excellent.

Methods applied are described in detail. Validation as well as comparison to similar datasets are thought-out and well explained.

Results and the development of elevation change of Antarctica over the last 3 decades are well presented.

I have some open question in regard of the uncertainty estimate - see below.

and some minor points:

1.

I was not comfortable with the data handling:

I have the feeling that the chunk dimension of the data in the netcdf file is not applied or is too small or applied to the wrong dimension (I'm not sure) . This makes it extremely difficult to read in the data in an acceptable amount of time.

E.g. using ncview takes ages to scroll through the layers. I also used IDL to readin the data and it took very long. Finally, I converted the data myself to be able to check the data quality in an reasonable amount of time. I used a chunk dimension of (x\_dim,1,1) when writing my own netcdf file. This improved the access time.

2. Error estimate

A very important point is your error estimation. To me its not clear if you used uncertainties estimated in 4.1 in the intergrated error estimation of 4.3. It also not clear what is provided in the product.

sigma\_x I guess are fixed numbers for each mission or mode. Can you provide a table?

Is  $\sigma_c$  spatially varying? I don't get how  $\sigma_m$  is computed, which variability is used? Please give more details.

Is  $\sigma_m$  computed for each grid cell?

In table 1  $\sigma_{rand}$  and slope is shown. How is this used in equation 5?

In section 4.3 you estimate  $\sigma_s$  and  $\sigma_r$  by comparing to the Smith product. This might make sense for the same time period, but is this valid for the period before 2003?

In equation 7  $\sigma_m$  is mentioned. Is this the same  $\sigma_m$  as in equation 5 and is this the rmse in your product?

E.g. As a user of your product I'm interested in Pine Island drainage basin for the period 2011 to 2015. How is the procedure to estimate the uncertainty for this given time period for this specific basin and for total Antarctica? Can this be done with the information given in the data product?

It's also not clear if interpolated, extrapolated and observed grid cells are handled in a different way. Furthermore, the uncertainty given for the pole hole looks pretty strange. The whole area is extrapolated but shows very large spatial differences in the RMSE, Can you explain why?

3.

In the text you mention that the bedmap2 elevation model was used. However, the data set itself provide a different elevation model. This is not consistent. Furthermore, I'm wondering why the old Bedmap2 data set is used instead of using the Tandem-X or REMA DEM's. They are much more accurate and provide reliable data in areas south of 86°.

4.

Why is it not possible to use Envisat data after the orbit change? Did you try with and without or weren't you able to get good dhdt estimates due to data coverage issues? Line 130: what exactly is a segmentation filter. How does it work? Please explain in more detail.

5.

To me its not clear how you handled the different ERS modes. How is the coverage of ICE and OCEAN modes? Do you have data in both modes for each month covering the whole ice sheet or separate months with ICE or OCEAN or specific areas with one mode for the whole period?

I miss in Fig.3 the ERS1/2 OCEAN mode.

6.

The combination of Enviat and ICESAT is not fully explained. At which point you combine these two products? The same for ERS1/2 ocean and ERS1/2 ice.

Do you combine it before the multi-mission cross calibration is applied? Maybe its worth to show two examples figures of certain grid cells how such a combination works (e.g. an grid cell with data gaps and without).

7.

How much does the normalisation of seasonal amplitudes change the trend estimates?

I'm not sure if this kind of normalization should be applied. The point is that you apply correction based on correlations with sigma, LE and TE for each mission. Those correlations make sense and reduce the saisonal amplitude. However, the normalization has no physical explanation. Maybe its' worth to check and show the seasonal amplitude of the CryoSat L2 product. If the amplitude is similar to your own processed product than the normalization is questionable.

Otherwise you have an argument that due to your low level threshold retracking the time varying signal penetration is strongly reduced. Maybe it's also worth to show in an Appendix for each mission the Antarctic wide reduction of saisonal amplitude. I think this can help to understand where the corrections have largest impact and where largest amplitudes are observed and if this is mission specific.

It's also not clear at which point this normalisation is applied - before or after the mission cross calibration?

If it is applied before, then you should change the order of the sections in the text.

8.

Line 450: Here the reference to the figure 2 is not correct. It should be Figure 6.

Line 517: You mention a correlation length of 100km, however Table 2 list different values. Which one was used?

9.

Line 608: Do you have any idea why your product is not closer to LA in WAIS? For EAIS and AP they are and this is what I would suppose, as you also used ICESAT in your approach?