Comment on tc-2021-204
Anonymous Referee #3

Referee comment on "A fine-scale digital elevation model of Antarctica derived from ICESat-2" by Xiaoyi Shen et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-204-RC3, 2021

General comments

Shen et al. describe a new DEM of Antarctica generated from 1 year of IceSat-2 data, using a model fit and blended resolution approach similar to Slater et al., 2018. The DEM is compared to airborne laser altimeter data to assess its accuracy, and compared to other DEMs derived from both radar and laser altimetry, radar interferometry and stereo-photogrammetry. The paper is generally well written and structured, although the readability of most figures could be improved, in terms of resolution and the choice of colour blind friendly colour scales. A new DEM exploiting the high accuracy and density of IceSat-2 data is a welcome product and is worthy of publication. However, I have some concerns relating to the description of the DEM resolution, model fit, and the comparison to both OIB data and other DEMs which I would appreciate if the authors could address.

DEM resolution and posting – the authors claim the modal resolution of the DEM is 250 m, but to me this does not seem correct. While the DEM is posted at 250 m, the most commonly used model fit is 1 km, and the majority of the DEM comprises of 500 m and 1 km model fits resampled to 250 m.

Model fit method – I have some concerns with the authors choice of using the model fit method, which I would appreciate if they could address:

- A linear component in time is more appropriate for longer time series, not one year of data where this parameter will be poorly constrained. How effective is this model at separating temporal elevation changes with just one year of data?
- Fitting a model to IceSat-2 data, which is both high accuracy and high density, to me
this is degrading the spatial sampling provided by this dataset, which should resolve finer scale features not observed by e.g. a larger radar footprint. The paper would benefit from the author’s adding a bit of text to justify why this approach is best for IceSat-2.

Comparison to OIB – the authors restrict the OIB comparison of their DEM due to temporal differences between the two datasets. This severely limits the amount of OIB data available for comparison. To my mind, temporal differences in elevation between the two datasets will only be worth considering in regions where elevation trends are high due to either ice dynamics or surface mass balance anomalies - why haven’t the authors used OIB data from other time periods in the interior of the ice sheet for example, where the surface height will be stable over time? This would allow more OIB data to be used and the DEM accuracy to be more robustly assessed.

Comparison to other DEMs – because the authors have not used a common OIB dataset to compare against the other DEMs, this limits their ability to claim their DEM is the most accurate (please note I am not doubting this is the case). Using a common dataset, or adjusting for temporal changes in elevation would allow for a more robust comparison.

Specific comments

L10 – I guess this should be ‘thirty years’, not ‘thirty decades’?

L51 – it is more that spatial and temporal variations in Ku band penetration depth are difficult to account for; I’d suggest re-wording this sentence to better reflect that

L74 – how are ‘good quality’ data defined – or on which criteria are poor quality data thrown out?

L87 – Not sure what the authors are referring to here by ‘seasonal elevation changes’? Seasonal elevation changes in Antarctica are only really specific to the Peninsula.

Fig 1 – suggest making the axis labels larger as they’re difficult to make out

Table 1 – this table is misleading as all the altimeter derived DEMs do have timestamps. Helm et al (2014) is derived from one cycle of data and Bamber et al (2009) correct for elevation changes between acquisition period of the two datasets. Instead of saying ‘unclear’, it would be more appropriate to state the acquisition periods of the datasets
used. I’m also unclear on what is meant by ‘Pan-Antarctica’, as Bamber et al (2009) includes the ice shelves also?

L128 ‘gaps’ not ‘gasps’

L141/Figure 2 – why have the authors chosen to show data density at 1 km, and not the posting of the DEM (250 m)?

L163 – I’m confused by the author’s claim that the modal resolution of the DEM is 250 m in the abstract – if most spatial coverage is provided by 1 km model fits then is that not the modal resolution?

L171 – I would suggest rewording as I feel this sentence is misleading – the DEM is posted at a resolution of 250 m, but the resolution is not 250 m as the most commonly used model fit is 1 km. This should be addressed elsewhere in the text (particularly the abstract) to make this clear to the reader.

Fig 3 – I’m surprised to see such large differences (up to ~ 300 m) between the three different resolutions? This could mean that the model fit is not working as intended; if the authors could investigate further into e.g. the spatial distribution of these differences that may help understand what’s happening.

Fig 4 – I’m not sure if the colour scale is playing tricks on me but it seems that the uncertainty is larger for the much of the ice shelves than it is for the ice sheet margins? Could the authors please explain why this is the case? The ice shelves are flat so the uncertainty should be lower here I think?

Fig 7 – I find this figure hard to read, improved resolution and particularly the colour scale used in panel c would improve the readability of this figure

L246 – remove ‘obviously’

Fig 8 – Suggest using a colour blind friendly colour scale here

Table 5 – I realise the authors have done this because the DEMs have different timestamps, but this is not a fair comparison as different subsets of OIB data are used for
each DEM, so it’s not possible to compare between the two. As mentioned previously, I don’t see the need for the authors to restrict OIB data in time in areas of low elevation change, so that could be a way to perform a more fair comparison. It may also be possible to e.g. correct for longer term elevation change between the two datasets using contemporaneous elevation trends.

Table 6 – I noticed the number of grid compared grid cells are different here – does this Table use different subsets of OIB data also?

Best wishes,