Review of Shen et al, TC 2021

This paper presents a new DEM of Antarctica derived from a year's worth of ICESat-2 data. It is a worthwhile endeavour and, with adequate care and attention to detail, an ICESat-2 DEM could provide a useful additional source of topographic information for Antarctica. There are, however, some misgivings with the approach taken here and lack of clarity about some of the methods.

General comments

In general the paper is well written but there are a significant number of instances of statements that are not accurate and/or misleading possibly because of the terminology used or possibly because of flaws in understanding or misconceptions. Specific details are given below but they start with the very first sentence of the abstract and continue from there and occur frequently.

If I understand the approach correctly, the authors produced a DEM at 250 km posting by resampling DEMs at 500 m and 1 km posting. First, the authors confuse resolution and posting (they are not the first to do this). It is misleading or incorrect to claim that a DEM with 26% observed coverage at 250 m posting has a resolution of 250 m as 74% of the grid points are interpolated. It has a 250 m posting and a resolution that is latitudinally dependent. More serious and worrying appears to the fact that there is a bias between elevations interpolated at the three resolutions which amounts to ~100 m at different latitudes (Fig 3b). According to this Fig. the mean elev at, say, 80 degs differs by ~150 m from 1 km to 250 m. if the interpolation has been done correctly what is the explanation for this huge difference and how is it possible to combine elevations at these three resolutions when they are so different? I must have missed something but this seems like a fundamental issue? It suggests the distribution of elevations is not Gaussian around the mean, which is entirely possible but the interpolation should account for this unless there is a flaw in the method used.

A second major reservation, which in addition contradicts the brief discussion of the results, is that the largest mean elevation difference with OIB data, standard dev and RMSD is found over the ice shelves. These are the flattest places in Antarctica and so it is worrying and extremely unexpected that the "worst" bias and random error is over the ice shelves. This makes no sense. Without some sound explanation for why this would be, it brings into question the methodology/approach used here. Combined with the strange bias between different resolutions this suggest some issues with the methodology.

Third. The DEM is presented and compared to six existing DEMs but there is no real attempt to discuss what value or use it might have compared to these, how and where it might be better/worse than one or other or why someone might want to use this DEM over, for example, the REMA DEM which has an RMSD that is half of the ICESat-2 one. It could be better for some applications but no attempt is made to consider what these might be. Was this DEM created because it could be or because it should be? The former has limited scientific value, the latter needs to have demonstrated scientific value.

Specific Comments

L9-10. "ice topography monitoring and ice mass change estimation". First, these two things are almost identical, the difference being a density, so to assert that they are somehow different is misleading. To get mass balance you have to measure an elevation change here. Second, the vast majority of altimetry-derived mass balance and "topography monitoring" approaches published do not use a DEM (partly because of biases in absolute elevation) but use elevation difference either at cross overs or along track.

L10 thirty decades = 300 years.

L24 “essential addition”. This is not demonstrated in the m/s. While the new DEM is an interesting addition to the 6 discussed, it is not the most accurate so to claim it is essential is unjustified.

L27-28. See first comment above. In addition, the two references cited do not use altimetry DEMs for ice motion tracking or mass balance.

L30 “monitoring the topography”. What do the authors mean? Do they mean measuring elevation change? See first comment.

L51-52. KU-band penetration into snowpack is unknown. This statement is misleading at best. There is plenty of literature on Ku band penetration into snow. Further, radar penetration can be corrected for either empirically or theoretically using a waveform fitting approach (see e.g. [*Davis*, 1996; 1997]).

L84 Icessn?

L87-88. This is not reducing “seasonal elevation changes” because any change that occurs in less than a year must be sub-annual, not multi-annual. Second, except for the pensinsula seasonal changes do not really exist over Antarctica, they are sub-annual which is not the same thing.

Table 1, p5. It states the the ICESat/ERS-1 DEM has an uncelar timestamp but in the paper there is a specific section the DEM time stamp, where it explains that the ICESat data are corrected for any significant dh/dt between 1995 and date of acquisition [*Bamber et al.*, 2009] so the time stamp is extremely clear and spelled out. It may not be perfectly corrected to 1995 but the Table is incorrect for this DEM and, consequently, makes me concerned about the veracity of claims made elsewhere in Table 1. Without carefully studying the methods used to generate each DEM the authors will not be able to support the claims made in Table 1.

L120 replace “seasonal” with sub annual here and elsewhere.

L130 neighboring. The preferred spelling convetion for TC is UK English not US. Please follow that convention.

L153. Strictly speaking the kriging variance is not, and cannot, be used to determine the interpolation error. It is related to the interpolation error but not equal to it.

L171. Nominal resolution. See above. It would be more appropriate to refer to 250 m posting with a resolution of 1 km for latitudes x-y and 250 m from a-b…

L248. Needs rewording.

L250 Here the authors claim the bias increases with slope or roughness but that is not borne out by Table 3. See General Comments.

L280-281. This is a sweeping generalisation that is incorrect. The accuracy of photogrammetric derived DEMs is a function of the resolution of the sensor and the accuracy of GCPs.

L321. “modal resolution” -> posting.

References

Bamber, J. L., Gomez Dans, J. L., and Griggs, J. A. (2009), A new 1 km digital elevation model of the Antarctic derived from combined satellite radar and laser data. Part I: Data and methods, *The Cryosphere 3*(2), 101-111.

Davis, C. H. (1996), Temporal change in the extinction coefficient of snow on the Greenland ice sheet from an analysis of seasat and geosat altimeter data, *IEEE Trans. Geosci. Remote Sensing*, *34*(5), 1066-1073.

Davis, C. H. (1997), A robust threshold retracking algorithm for measuring ice-sheet surface elevation change from satellite radar altimeters, *IEEE Trans. Geosci. Remote Sensing*, *35*(4), 974-979.