Comment on essd-2021-425
Anonymous Referee #1

Referee comment on "A new digital elevation model (DEM) dataset of the entire Antarctic continent derived from ICESat-2" by Xiaoyi Shen et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-425-RC1, 2022

First of all, I would suggest a different paper title to reflect the scope of the ESSD, e.g. "A new digital elevation model (DEM) dataset of the entire Antarctica continent derived from ICESat-2"

This manuscript provides an Antarctic DEM data set based on NASA’s new generation of ICESat-2 altimeter. The authors applied the spatiotemporal fitting method, so the data set covers both the ice sheet and ice shelves. This is not the first manuscript to tackle the DEM data set for the Antarctic. Nevertheless, the authors have demonstrated their product and evaluated it using the OIB and GNSS data under various surface conditions. As far as I understood, the ICESat-2-derived Antarctic DEM is not available. Considering the high-resolution and accurate measurements of ICESat-2, I expect this dataset could be valuable for Antarctic glacier research. For this reason, I would like to see this paper to be published.

In general, this paper is well written, and the structure is clear and easy to follow. It is an interesting topic and is worth to be published as ICESat-2 provides elevation measurements in much higher spatial-temporal resolutions. One weak point I can tell is that the authors provide just a 1-year data set. On the other hand, think of the entire Antarctic domain, one year data set is already quite comprehensive, in particular, authors have claimed that they can provide annual data sets in a sustainable way meaning the data set can be accumulated on an annual base.

I think the conclusion is rather short, I would like to see recommendations that authors could point out, e.g., a number of potential applications applying this data set or the forthcoming new DEM data sets generated by the methodology authors have applied. This improvement would strengthen this data paper.
Some specific comments I hope authors may find useful:

Abstract: “Antarctic digital elevation models (DEMs) data sets are essential,,”; “human fieldwork”, is there any nonhuman fieldwork?

Introduction: P3, L80: Do you apply any other quality control criteria than what you have mentioned here?

P3, L84: “Although the signal energies of strong and weak beams are different, all six beams provide centimetre-scale elevation measurements, and the biases of two beams in one pair are less than 2 cm (Brunt et al., 2019) and 5 cm (Shen et al., 2021) for flat and steep surfaces. Thus, the effect of elevations estimated from weak beams is negligible” Not very clear text, please explain more in detail.

P4, L92: “Icessn” ?

P4, L95: What do you mean by ‘the effect of interannual changes’ here?

P6, L137: this model-fitting method has been used in other papers (e.g., Slater et al., 2018). They have produced multi-annual data, while in this paper you have made just one year of data. Can you point out the differentiation between your work and theirs, e.g., does the length of data processing matter?

Additionally, considering the higher coverage and spatial resolution of ICESat-2, applying a fitting model to ICESat-2 will resolve its finer observations which are not obtained by other satellite altimeters. Can you try to make use of all ICESat-2 data and apply the kriging interpolation directly, in this way you may obtain a more detailed and accurate elevation map, due to the higher resolution and accurate measurements of ICESat-2? The Authors should clearly state why this estimation method is suitable for ICESat-2 data.

P6, L144: I would like to see a figure for elevation change rate (a5), which can be used to evaluate the method performance. In addition, I have some concerns if one year of data is enough to estimate a reliable elevation change. Could you please provide the elevation change rate map (a5) to see if the method makes sense?
P8, L185-195: please explain clearly among those resolution numbers, what exactly number you have finally applied and why?

P9, L218: why do you use this method, why don’t you resample the OIB to the DEM data and calculate the difference and its statistics?

Table 3: your uncertainty map shows values of < 2m, in this Table an SD of 15 m can be found, which means that the uncertainty map may not represent this, can you explain this? Additionally, the predicted uncertainty (i.e., uncertainty map) with the actual uncertainty (comparison with OIB and GNSS data) should be compared and discussed.

Figure 7: I found some negative values in your DEM map in the boundary of ice shelves, can you explain them?

P13, L268: Can you prove more evidence here to clarify why ice sheet elevations are more accurate than those estimated for ice shelves.

Figure 9: I noticed that OIB elevations are near > 0 while your DEM has some elevations even less than -200 m, can you explain this?

Table 5: why the number of used OIB measurement points in this table is different from that in Table 4.

Table 5: here you compared the other DEM and your DEM to OIB data, as the same OIB data were used, but the timestamps of DEMs are different, do you consider this effect or how to reduce it?

P19, L385: Again, please provide an elevation change rate map to evaluate the elevation estimation performance.

Please make a revision of the manuscript accordingly, I recommend this manuscript be considered as an ESSD publication after a revision.

Regards,