

Earth Syst. Sci. Data Discuss., referee comment RC3  
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## Comment on **essd-2021-425**

Anonymous Referee #3

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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-RC3>, 2022

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This manuscript generates a 500 m resolution DEM of Antarctica based on the ICESat-2 data from November 2018 to November 2019 using a spatio-temporal fitting approach. The authors validated the DEM using IceBridge airborne altimetry data and GNSS ground measurements, and also compared it with six other published Antarctic DEMs. Although the results show that the accuracy of this DEM is very superior, I am doubtful about this result.

General Comments:

It is a good attempt to build an Antarctic DEM dataset based on ICESat-2 altimetry data, and Antarctic DEM is important for the study of Antarctic ice sheet changes. ICESat-2 satellite can indeed provide very high resolution and high accuracy ice sheet elevation data and has great potential to be a reliable data source for building Antarctic DEM. However, unfortunately, the dataset was not completely utilized by this manuscript.

Other specific comments:

First of all, there is no innovation in the method study as the spatio-temporal method was referred from Slater et al. (2018). On the one hand, although Slater et al. used this method to build a superior performance Antarctic DEM based on data from the radar altimetry satellite CryoSat-2, the authors' transposition of this method to the data processing of the laser altimetry satellite ICESat-2 may create unknown uncertainties. On the other hand, I consider that using the altimetry data with only a time span of 1 year cannot show the priority of the spatio-temporal fitting model, and can cause fitting errors due to the limited data density and spatial distribution. In fact, this is also reflected in the manuscript, where only 46% of the grids in the 500m resolution DEM claimed by the authors are directly generated by fitting sampling points within the 500m grids, with other

gaps either obtained by resampling the grids at low resolution or by kriging interpolation.

Secondly, I have serious doubts about the reliability of this Antarctic DEM dataset, and although the authors use some measured data to validate it, I do not think this validation method is reliable. Although these variations are neglected in areas with small elevation changes in the interior of the ice cap, I do not agree that it is reasonable to use OIB and GNSS data with large time differences to assess the DEM accuracy. In addition, it is not representative of the accuracy of the whole DEM, limited by the amount and distribution of the validation data. Of course, this is due to the limitation of obtaining large-scale field measurement data. However, it cannot be arbitrarily claimed that the accuracy of DEM under such validation conditions is better than the results of other scholars.

In addition, from the perspective of manuscript writing, this manuscript is well structured and the language is more fluent, but there are some places where the expression is not very clear and there are also a large number of obvious typographical errors. For example, Fig. 1b and Fig. 1c are not seen in Fig. 1, but they appear in lines 105 and 108, respectively; in line 305, the description of the comparison of other DEMs should be discussed in Section 4, which seems very confusing here; in Table 6, it should be 'Number of used GNSS measurement points' instead of 'Number of used OIB measurement points', etc.

In fact, I have also seen this manuscript in the discussion forum of *The Cryosphere* last year, and this does not seem to be too much changed from the previous manuscript. Moreover, another article by the authors using the same approach applied to Greenland has been published in *ESSD* (Fan et al. 2022), and the two manuscripts are similar in approach and writing style, and I do not think it is worth publishing a similar work again.

In conclusion, I think the manuscript has no innovations in the DEM generation method and the dataset is not reliable, and its validation data are not enough to support the authors' conclusion, so it is not recommended for publication.

Reference¼□

Fan, Y., Ke, C.-Q., and Shen, X.: A new Greenland digital elevation model derived from ICESat-2 during 2018–2019, *Earth Syst. Sci. Data*, 14, 781–794, <https://doi.org/10.5194/essd-14-781-2022>, 2022.

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Parrinello, T.: A new digital elevation model of Antarctica derived from CryoSat-2 altimetry, *The Cryosphere*, 12, 1551–1562, <https://doi.org/10.5194/tc-12-1551-2018>, 2018.