

Interactive comment on “A new Digital Elevation Model of Antarctica derived from CryoSat-2 altimetry” by Thomas Slater et al.

Anonymous Referee #3

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Review of “A new Digital Elevation Model of Antarctica derived from CryoSat-2 altimetry” by Thomas Slater et al., submitted to The Cryosphere Discussions.

Summary: The authors present a Digital Elevation Model (DEM) of Antarctica derived from CryoSat-2 measurements acquired in the period July 2010 – July 2016. The DEM is generated from spatio-temporal fitting to elevation measurements in individual grid cells of varying size (1, 2 and 5km²). The accuracy of the DEM is evaluated through comparison to contemporary surface measurements from NASA’s Operation IceBridge campaigns.

General comment: The manuscript reads well and is easy to follow. The creation of a new DEM might not represent a major scientific step forward, but the authors argue nicely why updated and high-quality DEMs are useful for a range of applications. The

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methods applied are sound and robust, and the authors find that the DEM has improved accuracy compared to previous altimetry-based DEMs of Antarctica, making this work worth publishing. I have listed below some specific recommendation that I think the authors should address to improve the manuscript.

Specific comments:

Sect 2.1.

p.3, l. 8. Both references here are from before the launch of CS-2. It would be nice to back up this statement of the performance of the OCOG with literature that actually is based on CS-2 data, if possible.

p.3, l. 9-11. It is unclear to me if the authors use the L2 SARIn product from ESA or whether they perform the retracking themselves. In the case of the latter (which I suspect is the case) some more detail about the retracking algorithm should be added, in the same way as they describe the OCOG.

Sect. 2.2.

p.3, Eq.1. The model includes a t-term taking into account any trend in surface elevation in the grid cell. But some regions must also include an acceleration (t² term). How will it affect the z value in a grid cell if an acceleration is present but not modelled?

p.3, l. 22. What is your definition of “unrealistic estimates”?

p.3, l. 29-30. The method results in 60% of the 1km grid cells on the ice sheet and 75% on the ice shelves having z values. Is this due to lack of measurements in the remaining grid cells? What is the minimum number of data that you require to provide a z-value in a grid cell? And do you have a requirement on the time span that must be covered by available measurements?

Also, have the authors performed relocation (due to topography) of the LRM data, and if so, how is this done? This should be clarified.

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Sect. 2.4.

The approach for the DEM evaluation is well described and I think that the choice of separating the evaluation in 'observed' and 'interpolated' DEN grid cells is good. This makes the analysis and results very transparent.

Sect 3.

I think that some of the information provided in this section actually fits better under Sect 2.4 (DEM evaluation), e.g. what statistics are being derived, how to account for elevation changes etc.

p. 6, l. 16 Isn't it the case that when using Eq. 1 to find the elevation (z) in a grid cell, this elevation actually corresponds to the elevation at $t=0$, meaning 2010 (or whatever the start time of measurements in the grid cell is)? Did you ensure the effective time stamp to be July 2013 by defining this to be $t=0$?

Sect 3.1.

p. 6, l. 15-17 Yes, but this is only the case if the time stamp is actually July 2013 (see my previous comment).

p. 6, l. 31-33. Why didn't you correct for the temporal elevation changes here?

Figures.

Figure 6. The colour scale is not optimal. First thing is that all the differences shown all look blue/green, making it difficult to see if there are any local differences. Also, the colours of the max and min values in the colour scale look similar (to me at least).

Figure 7. Same comment as for figure 6.

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