

Interactive comment on “Sentinel-3 Delay-Doppler Altimetry over Antarctica” by Malcolm McMillan et al.

Anonymous Referee #2

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General comments

This manuscript assesses the quality of Sentinel-3 delay/Doppler radar altimetry over Antarctica through analysis of: precision at Vostok Subglacial Lake and Dome C; accuracy at Vostok, Dome C, Dronning Maud Land, and Wilkes Land; and elevation change across the continent. The authors find that Sentinel-3 altimetry achieves a precision of <0.1 m over flat topography, has a mean bias <0.2 m in the flat interior and <1.5 m in complex coastal terrain, and can reproduce known patterns in elevation-change rate at the continent scale. The paper represents the first use of Sentinel-3 altimetry data over an ice sheet, is generally well-written, and will be of significant value to the community as an introduction to the capabilities of this Sentinel mission, but there are three major weaknesses that should be addressed before publication:

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1. **Limited Analysis:** While the overall analysis presented in the manuscript is sound, it is also much more limited than I would expect from a rigorous treatment of altimetry uncertainty. The authors only assess precision in two flat, interior locations, yielding a “best-case” estimate, while opting not to perform a continent-wide crossover analysis, which would provide the readers with much better insight how and why precision varies. Crossover analysis is a standard technique in satellite altimetry error analysis and should be included in any rigorous assessment. In a similar vein, there are only four regions included in the accuracy analysis, when Operation IceBridge has surveyed across the continent. It is not clear why this assessment was so limited in scope, when including the rest of the Operation IceBridge airborne laser altimetry dataset would help illuminate the causes of error in the Sentinel-3 altimetry and provide the community with much more direction as to where and how to use this new dataset. Finally, the analysis of elevation-change rate is entirely qualitative; while I certainly understand that the processing chain is still in development, there should be some quantitative analysis of the result if it is to be included in the manuscript, such as a comparison to CryoSat-2 derived elevation-change rates over the same time window. At the very least, a map of “known” dh/dt from CryoSat-2 or ICESat should be shown in conjunction with the Sentinel-3 map to show the patterns are generally similar.
2. **Data and Method Detail:** There are quite a few pieces of information missing with respect to the data sources used and the methods applied. There is a long list of corrections applied to the altimetry data, but no citations that provide the details of the models. There is no data citation related to the ATM laser altimetry (though there is for the Riegl data), which prevents the reader from knowing what level of data processing was used or other important details for understanding cm-scale accuracy. It appears to be L2 data; why was L2 chosen over L1b (full-swath)? Are the same corrections with the same models applied to the airborne

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altimetry as to the satellite altimetry? What are the dates of the Operation IceBridge flights (which is needed understanding the potential impact of the dh/dt correction)? Last, the method for correcting space and time elevation differences for the satellite-airborne analysis requires some more detail: how is a 1 km DEM being used to correct for spatial separations of <100 m? What is the error associated with the dh/dt correction? What is the magnitude of these corrections (mm? cm? m?)? Also, at a basic level, are the results presented as $h_{sat} - h_{air}$ or $h_{air} - h_{sat}$? The former is typical, but the manuscript implies the latter (“IceBridge-Sentinel-3 measurement pair”); resolving this issue is fundamental to the analysis of why the bias exists (p.7, lines 7–9).

- Figure Presentation:** The figures could be greatly improved to make them easier to understand and to use journal space better. Cyan and yellow for groundtracks on Figures 1, 3, and 5 are hard to see, printed or on a screen. I am not sure why the maps on Figures 2, 3, and 4 are plotted as 3D perspective plots; this style makes it harder to understand the spatial context, particularly how the tracks align with easting and northing (which is important since this is how the transects are plotted). The axis and legend labels on all figures are unreadably small and the line widths are too small. I am not convinced that Figures 2c and 2e add to the discussion over a single histogram of uncertainty in 400 m along-track bins across the entire elevation range. The histograms and cumulative distribution functions in Figures 3b/3c and 3e/3f can likely be combined into a single panel. The authors should rethink how each of the figures is presented to make the best use of space and to ensure they are fully legible at production size.

Specific comments

- p.1, line 13: “the first step towards a new era” is a bit over the top. The Antarctic coast is already blanketed in *better* altimetry (that includes along-track de-

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- lay/Doppler processing). Please tone down these statements throughout (especially the use of “novel”).
- p.1, line 20: “accuracy decreases slightly” is an understatement, given most of the measurements are up-to an order of magnitude less accurate.
 - p.2, line 16: the rest of the details of what controls pulse-limited footprint should go in this parenthetical statement (i.e., satellite altitude, pulse length).
 - p.3, lines 5–7: Figure 1 should have the SARIn/LRM mode mask on it to show where Sentinel-3 altimetry will make the biggest improvement compared to existing datasets (i.e., where Sentinel-3 covers LRM areas). It also should show the latitudinal limit of Sentinel-3.
 - p.4, lines 12–14: The rest of the study *only* uses the TCOG retracker; I think adding some of the statistics from the other retrackers to Table 2 would be a fantastic addition and helpful for the community.
 - p.4, lines 23–24: I would like to see the % of data points removed by this filtering step.
 - p.4, line 27: This date range is insufficient given the dh/dt correction. Please provide exact flight dates, either here or in Table 2.
 - p.4, line 28: The along-track sampling of ATM L2 data is 0.25 s, which results in variable along-track distance spacing due to changes in aircraft velocity.
 - p.5, line 3: Why was the single closest measurement chosen, instead of some sort of average of all the points within the approximate SAR footprint? Given that surface roughness is later suggested as the cause of bias and that lidar can easily resolve surface roughness, it seems appropriate to try a less simplistic approach that can account for surface roughness.

- p.5, lines 8–10: A few words about how the distributions deviate from normal would be appreciated here. I am also somewhat worried about the use of median absolute deviation given that it is $>20x$ smaller than the standard deviation in coastal areas, demonstrating that outliers are a significant problem for Sentinel-3 altimetry. One path forward would be to suggest a filtering method for Sentinel-3 users could apply that would bring the two statistics closer to one-another.
- p.5, lines 3–4: What % of IceBridge records were removed by this filtering step?
- p.5, line 22: It should be noted that you are using 1σ as the definition of precision.
- p.6, lines 4–5: While it may be true, I do not think that the conclusion that the SAR waveform leading edge is insensitive to penetration is defensible given the analysis presented in the manuscript. Perhaps it is the Sentinel-3 sampling geometry that limits observed variability, or, even more simply, there was not much surface variability during the one-year period of observations. This statement should be removed, or alternatively there needs to be a thorough comparison between Sentinel-3 SAR and CryoSat-2 LRM mode using a similar retracker over the overlapping period to demonstrate that SAR processing (specifically) reduces sensitivity to penetration and subsurface scattering.
- p.6, lines 8–9: A “slope” correction, in its original meaning, is not applied in this processing chain; it is a “relocation” correction.
- p.6, lines 28–29: Given the importances of these outliers on the statistics, I think filtering strategies should be touched on in this manuscript. All the individual pieces are there to assert a solid filtering pipeline and demonstrate how it improves data statistics.
- p.6, line 34: Why is the bias related to the airborne campaign rather than perhaps a bad dh/dt correction?

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- p.6, lines 31–32: The method presented does not derive a surface slope (but rather a linear slope of elevation residuals), so where is this coming from?
- p.19: It should be clarified that the footprint diameter is the beam-limited footprint. I would like to see when the rest of the satellites are planned for launch in this table as well.

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