

The Cryosphere Discuss., referee comment RC3
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Comment on tc-2021-157

Anonymous Referee #2

Referee comment on "Antarctic snow-covered sea ice topography derivation from TanDEM-X using polarimetric SAR interferometry" by Lanqing Huang et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-157-RC3>, 2021

Overview

The manuscript *Antarctic snow-covered sea ice topography derivation from TanDEM-X using polarimetric SAR interferometry* by Huang et al. presents the development and validation of a new two-layer plus volume sea ice model with the aim to correct for the height bias associated with InSAR penetration into the snow pack. This model is able to represent the sea ice/snow stratigraphy and associated scattering, and, when simplified and inverted, allows for the estimation of the sea ice plus snow surface topography from TanDEM-X. This retrieval technique shows strong agreement to an Operation IceBridge optical (DMS) DEM that was collected contemporaneously as part of the OIB/TanDEM-X Coordinated Science Campaign.

This manuscript is well-written and thoroughly presents novel methods and results that could be useful to the broader sea ice community. I have a few relatively minor comments and suggestions that should be considered, found in the general and specific comments below.

General Comments

The main comments I have on the manuscript deal with (1) the height threshold used (2) X-band scattering/slush layers and (3) the snow depth parameter.

GC1: To me, it appears there is some mix-up with the height threshold used to keep model-error accuracy to within 25%. In section 4.4, it was stated that the *ice volume* ($z_1 - z_2$) needs to be thicker than $\sim 1.5\text{m}$ to achieve this accuracy. However, in later sections only ice+snow heights above the local sea surface (effectively the total

freeboard) above 1.5m are used. Doing so filters out ice volumes much thicker than 1.5m, since most of the ice volume is below the waterline.

I would suggest the authors confirm that the 1.5m threshold is indeed for the ice volume, and recommend that they filter the InSAR retrieved heights accordingly (which should result in a much lower height-above-sea-surface threshold).

GC2: (This is similar to that from reviewer 1) While the scattering impacts of a slush layer are briefly mentioned, I feel that their impact should either be discussed further or/and incorporated into the model in some way. A slush layer at the snow-ice interface would surely effect the radar return differently than if the snow-ice interface was smooth and dry. Also, some mention of the effects of surface roughness would be beneficial, as snow surface/interface roughness has been found to influence X-band backscatter (Nandan et al. 2016, *Remote Sens. Of Envir.*, <https://doi.org/10.1016/j.rse.2016.10.004>). Finally, while surface melt may not be present in this particular region or season, a wet snow surface could also influence the X-band backscatter (Dufour-Beauséjour et al. 2020, *The Cryosphere*, <https://doi.org/10.5194/tc-14-1595-2020>). This would need to be taken into account if applying this technique to other regions and/or seasons.

GC3: The paper states that the influence of snow depth on y_{mod_T} is not negligible, and that a priori data from external sources must be used in the simplified model. If I understand correctly, the passive-microwave-derived snow depth data used as the sole model parameter results in a single snow depth value (18 cm) for each pixel across the scene. While I understand that high-resolution snow depth data is generally not available, this single value is likely not representative of the actual spatial snow depth distribution (and perhaps not realistic for heights >1.5m, as a quick hydrostatic calculation of ice thickness with this snow depth yields abnormally thick ice). Therefore, I'm curious as to the impact of the snow depth parameter on the experimental results (beyond what is shown in the simulated results of figure 8), and if/how the retrieved heights would agree with the DMS DEM under e.g. spatially-varying snow depths.

Specific Comments

-Lines 23-25: I find this sentence slightly confusing as it's written, especially since Petty et

al. 2016 also mention the “close correspondence” between the predicted (surface height+square root relation) and OIB-measured thickness. Just noting the +/-2m difference makes it sound like a poor retrieval.

Lines 29-31: Since you mention that characterization of sea ice topography is an active area of research (line 28), I would suggest citing more recent studies using laser altimetry and photogrammetry (e.g. Farrell et al. 2020, <https://doi.org/10.1029/2020GL090708>; Li et al. 2019 <https://doi.org/10.3390/rs11070784>; and/or others).

Line 87: By previous work, do you mean Huang and Hajnsek (2021)? Or previous studies in general?

Line 91: Same as above comment. If previous work is referring to Huang and Hajnsek (2021), I would suggest writing that explicitly.

Line 179-180: How are water-surface points selected? And how many pixels/points are used in this scene? More information would be useful to ensure that these reference surface elevations are not biased due to e.g. newly frozen leads.

Line 199: How many segments are removed vs used due to mis-coregistration? A percentage of rejected or accepted segments would be useful here.

Figure 8: This figure should have subplots labeled (a-f) on the figure, since they are referenced as such in the text. I agree with reviewer #1 that it is not apparent how phase centers are derived from these figures.

Line 393: Similar to above points, what percentage of pixels are processed (i.e. heights above 1.5m) vs not? With a scene-average height of 1.27m along the DMS DEM (line 494), I suspect that a large portion has been removed.

Line 398: I assume 18cm is the average snow depth of the whole region, including ice <1.5m? If only samples >1.5m are selected for processing (line 394) I am curious how your results would look if you were able to use snow depth on just the ice with elevation >1.5m. While I know this information may not be available, using some type of spatially-varying snow depth assumption may help to constrain possible retrieved topographies.

Figure 13: It's fairly tough to see the DMS DEM in between grey lines in (b)-(d) and draw any conclusion about its agreement with the SAR data. I would recommend making the lines thinner or reducing the width of the zoomed sections, if possible, so that more of the DMS heights are shown. If inclined, a difference map (InSAR height – DMS height) would be useful to provide a more quantitative 2-D verification.

Figure 13 also: How are heights less than 1.5m calculated in this SAR image if not selected for processing with this model? Subplot (d) in particular appears to have regions of 0m height that I suspect are not entirely physical.

Line 456: I would suggest clarifying that h_{Model} in this case is the simplified model. While I understand it is in the "simplified model" section, to me the third row in Table 1 is the

Theoretical model row (as it is the third method). The fact that the RMSE ranges between 0.22 and 0.27 for both models further adds to the confusion.

Line 487: This line (particularly "larger baselines respectively larger k_z values") doesn't quite sound correct as written. Do you perhaps mean the possessive "baselines' "?

Line 499: Should be "25%-error accuracy" to be consistent with previous sections

Technical Corrections

Line 59: Icebridge -> IceBridge

Line 143: iceberg -> icebergs

Figure 1 caption: rectangular -> rectangle

Line 215: 'flat-earth removed' should be written as 'flat-earth-removed'

Line 254: Provide full names of TDF and TSX at first mention

Line 470: (grammar) well correct -> e.g. adequately/sufficiently/suitably correct

Line 501: comma after "For instance"