

The Cryosphere Discuss., referee comment RC1  
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## Comment on tc-2021-185

Anonymous Referee #1

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Referee comment on "Kara and Barents sea ice thickness estimation based on CryoSat-2 radar altimeter and Sentinel-1 dual-polarized synthetic aperture radar" by Juha Karvonen et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-185-RC1>, 2021

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The study "Arctic Sea Ice Thickness Estimation Based on CryoSat-2 Radar Altimeter and Sentinel-1 Dual-Polarized SAR" by Karvonen et al. puts forth a method to interpolate and extrapolate CryoSat-2 sea ice thickness data over the Kara and Barents Seas using segmented Sentinel-1 SAR data. Currently, daily-scale sea ice thickness data from altimetry include only sparse, footprint-sized swaths that are less useful for timely purposes such as maritime navigation. This method aims to increase the utility of daily CryoSat-2 thickness data by mapping the CryoSat-2 thicknesses to coincident Sentinel-1 SAR segments to create high resolution sea ice thickness maps over a given area.

This manuscript defines the proposed algorithm, explains the inter-/extrapolation of the CryoSat-2 data to neighboring SAR segments, and compares the results to different sea ice thickness products, namely the AARI ice charts and two different sea ice reanalyses. An initial high bias was found in the data, so the CS2/S1 sea ice thicknesses were remapped using another reanalysis to reduce the positive bias. While the comparisons show low-to-moderate agreement of mean thickness values over the study area, the authors put forth some discussion on potential difficulties in the comparisons as well as potential future avenues for improvement of the algorithm, so that it may be useful for future altimetry missions.

Overall, the technique presented here is novel and potentially useful, and the manuscript shows some interesting results. However, I feel the results could be strengthened by incorporating some of the comments below. Additionally, there are many grammatical errors that need to be addressed, pointed out in the technical corrections. Provided that these (at times substantial) revisions are made, I would recommend this manuscript for publication in The Cryosphere.

## General Comments:

GC1: While the introduction does a good job to provide background on different retrieval techniques and limitations, I feel that it would benefit from:

- Including some applicable references that are missing (see specific comments below)
- Focusing the topics a bit more. The last paragraph, for example, talks a lot about the relationship between roughness and backscatter, which is not a topic covered in the rest of the paper.
- Providing an outline at the end that ties the introduction to the rest of the paper and outlines what will be presented in the manuscript

GC2: I feel it would be useful for this study to provide some assessment of the retrieved thickness maps with other remotely-sensed datasets. I understand the desire to compare with independent daily-resolution products (which are lacking from altimetry), however, the reanalyses simply don't capture the observed thickness distribution with enough confidence to draw robust conclusions about the performance of this algorithm.

To me, the merged CryoSat-2/SMOS product (Ricker et al. 2017; <https://doi.org/10.5194/tc-11-1607-2017>) would be a useful comparison, as the same input data are used (CryoSat-2) so the authors can test how well these data are mapped to S1 compared to the SMOS product. I understand this is a weekly product, however, I think it would be as (if not more) useful than e.g. the AARI ice charts (which are also a weekly product at a lower spatial resolution) by providing sub-polygon thickness information.

GC3: While this paper provides plenty of metrics comparing the study-area-mean thickness between the CS2/S1 and the reanalyses, I feel it does not adequately compare the retrieved spatial pattern of thickness over the study area. While the average values show moderate agreement in some cases, there is a clear discrepancy in the spatial patterns that warrants discussion. Difference maps, for example, would help to show

which areas (and therefore which ice types or texture features) show better or worse agreement. Such analysis could also help to better inform the need for remapping, as the bias may not be uniform over the region.

GC4: Parts of this study region have been found to have large snow loads that cause negative ice freeboards and wet/slushy snow-ice interfaces (Rösel et al., JGR Oceans, <https://doi.org/10.1002/2017JC012865>). While this paper mentions impacts of wet snow surfaces brought on by warm surface air temperatures, I would be curious to see the impacts of wet snow (and potentially slush) near the snow-ice interface mentioned in this manuscript.

GC5: Overall, the manuscript could use a closer proofread, as there are many issues with abbreviations, parentheses in citations, and grammar that partially distract from the science. Many of these are pointed out in the specific and technical comments below.

Specific Comments:

Title: While the title is descriptive and technically accurate, the method is only applied to a small area of Arctic sea ice and not tested over the whole Arctic. Therefore, I feel it may be best to modify the title and specify that the study focuses on the Kara/Barents Seas.

Lines 9-18: Appropriate references are missing from this first paragraph.

Line 9/introduction section: I appreciate stating the goal of the paper at beginning of this section, but feel that the introduction would benefit from also including an overview/structure of the paper at the end of this section.

Line 36: A reference to the limited accuracy of CS-2 over thin ice would be useful to include.

Line 37-38: This sentence beginning with 'although' is not grammatically correct and should be revised.

Line 55: I would suggest not using the variable 'F' here, since no equation is given and since it is (more appropriately) used for 'freeboard' later in the paper. Either no variable is necessary, or, if a different one is used, make clear what it represents (i.e. is it the standard deviation of roughness? Or just the average large-scale surface roughness?)

Line 56-57: A reference should be provided here.

Line 91, Figure 3: It says that  $T_{am}$  is plotted in figure 3, however, the axis labels in figure 3 show  $T_a$ . The figure should be changed to  $T_{am}$ .

Lines 97-122: These paragraphs contain many grammar issues/incomplete sentences and should be carefully read-through and revised.

Line 127: 250m is smaller than most studies cite as the nominal along-track footprint of CryoSat-2. Somewhere around 305m is more commonly accepted (from Scagliola 2013; [https://earth.esa.int/documents/10174/125271/CryoSat\\_Footprints\\_TN\\_v1.1.pdf](https://earth.esa.int/documents/10174/125271/CryoSat_Footprints_TN_v1.1.pdf))

Line 140: Is there a reason that Baseline-C data used in this study? Baseline-D CS-2 data were released in July 2020 (Meloni et al. 2020, *The Cryosphere*, <https://doi.org/10.5194/tc-14-1889-2020>) and should be used throughout this work. I would recommend redoing the analysis shown in this manuscript with Baseline-D data, or providing a valid explanation as to why Baseline-C data need to be used.

Line 193: Similar to the above general point, it doesn't seem entirely necessary to me to compare to both of these reanalysis products, especially if there is a known underestimation from TOPAZ4. In my opinion, it would be more useful to include an altimetry-based product (like the CS2/SMOS product) as opposed to multiple different reanalysis products.

Lines 257 – 262: I believe this paragraph is meant to provide an overview of the algorithm that is described in later paragraphs, however, it's slightly confusing as written. I would suggest adding some text that makes it clear that this is a general overview of the process and that each step is discussed in more detail in the following paragraphs. (It could also be combined with the next paragraph, as the block diagram is a nice way to provide an overview).

Section 4: While I feel the methodology is thorough, this section could benefit from a better organization (similar to the above point). I would recommend beginning section 4 with a general overview of the methodology (similar to lines 257-262 and figure 6), and have subsequent subsections to further explain each step in the process.

Line 327-329: I think the larger issue in using this many features would be the potential for overfitting as opposed to computational time. It may be useful to include some analysis/explanation that provides confidence that overfitting is not occurring given the number of features used.

Line 347-366: I understand the rationale for remapping to reduce the thickness overestimation (though would recommend showing some results of the 2016 training data overestimation that warranted the remapping) however, there is still a fairly large mean bias compared to AARI and Topaz4 after remapping, which could signal a difficulty in the method beyond the noted underestimation of SIT from model reanalysis (line 352). As noted above, a spatial comparison to another remotely-sensed (e.g. CS-2) dataset could be useful in assessing performance and provide more (or less) justification for remapping.

Figure 4: The land is fairly tough to see in the HV polarization. Since the authors mention that a land-mask is applied to the mosaics, I would recommend changing the color of the land or somehow differentiating it from the darker areas of sea ice.

Technical Corrections:

Line 5: Period should be a comma after ORAS5.

Line 9, elsewhere: 'CS2' is used here as the abbreviation for CryoSat-2, while in the abstract and later on in the paper 'CS-2' is used. This abbreviation should be kept consistent throughout.

Line 10: SIT is defined in previous sentence, and does not need to be written out here.

Lines 21, 50, 53, 136, 140, 144, 170, 179, others: Some references are put in parentheses when they should not be. I recommend checking them over and removing parentheses where applicable.

Line 31: Remove redundant 'the'

Line 36: 'uncertainty' should be 'certainty' in this case

Line 38: 'TIR' should be defined first.

Lines 89 and 91: References to figures should have 'Fig' and not just numbers.

Line 98: Change 'there is' to 'there are'

Line 111: 'CS-1' should be 'CS-2'

Line 112 : Change 'is' to 'are'

Line 116: Change 'very close' to 'is very close'

Line 131: Remove 'from' at end of sentence.



Line 145: S-1 is already defined above and does not need to be done again here.

Line 147: SENTINEL should not be written in all capital letters.

Line 150: The abbreviations used should probably be defined at first mention (AARI, ORA5, etc.)

Line 219-221: This sentence doesn't make sense grammatically and should be revised.

Line 254: Does MS = meanshift?

Lines 272-286: I wonder if this list would make more sense as a table?

Line 316, 382, others: The abbreviation 'w.r.t' should be written out.

Line 531: Melt-down is misspelled

Tables: Units should be added to tables that show biases or differences.

Table 6 caption: Remove redundant 'averages' .

Figure 8: I assume the caption should read 2017 and not 2021.

Figure 10: This color scale should not be used if values  $> 50\text{cm}$  are just going to be set to a single color. It should be adjusted to match what is shown in the plot.