Response to the reviewer 1 comments on the manuscript TC-2021-185,

Thank You for reviewing our manuscript and for the constructive comments, We have tried to take the comments into account where applicable and updated the manuscript accordingly. In the following are our detailed response to the comments (in blue).

As two reviewers suggest, we decided to use the baseline-D data and recalculate everything. We'll also provide two versions of the algorithm: one trained to be compatible with the modeled SIT and another with the CS-2 SIT. Recomputation of everything and updating all the tables and remaking most of the figures will require a significant amount of work and time, so we'll request the editors enough time to perform this work in addition to our other duties.

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

We have included the references suggested by the reviewer. We think the paragraph on the relationship of SAR backscatter and ice thickness with an overview of the related research is important background information and indicating that C-band SAR backscattering and texture features derived from it will require some complementary data to get reasonable SIT estimates, especially for the thicker (e.g. compared to the Baltic Sea ice) Arctic sea ice.
We have added a few concluding sentences at the end of the paragraph to indicate the relation of this study to the existing SAR based SIT retrieval.
We have also included a paragraph providing an outline at the end of the introduction.

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.

We ha also provided comparison to CS2SMOS SIT in the revised version.

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.

We have included difference maps both for the single day case and average difference maps for the test data set of 2017.

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.

We have not studied this particularly, we included this topic in the discussion section with the reference. The most probable time for existence of wet snow on sea ice is the melt-down period which is mainly outside of the study period (because CS2 SIT is not provided for May-September). This would be an interesting topic for further studies.

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.

Our co-author Eero Rinne who has lived in UK for many year and with excellent English skills has proof-read and edited the manuscript accordingly. If seen necessary by the editor we'll let a proofreading service to check the final version before publishing it.

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.

We have changed "Arctic" to "Kara and Barents Sea" in the title.

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

References included.

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.

Added.

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

Reference included.

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

Sentence corrected.

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?)

We have now used only "average surface roughness" here.

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

The reference is the same as in the previous sentence i.e. Similä et al. 2010, added the reference also here for clarity.

Line 91, Figure 3: It says that T is plotted in figure 3, however, the axis labels in figure 3 show T . The figure should be changed to T .

We now use only Ta which is the daily average air temperature.

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

Have been read and revised by our co-author Eero Rinne.

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)

We have updated text here and included the suggested reference.

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.

The reason behind Baseline-C is that Cryosat-2 analysis was made already in 2019. Addressing the comments of the all of the three reviewers will require substantial re-processing of data, and we shall change to using baseline-D or even baseline-E, if that will be available. We'll start the new analysis in Dec 2021 -Jan 2022 and to recompute everything and rewrite many parts of the manuscript (results, tables, many of the figures) we need enough time to be able to do this in addition to our other duties.

Line193: 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.

We included comparison to CS2SMOS SIT. We also left comparison to Topaz SIT in the manuscript as it may be of interest for some readers using Topaz4 which is the operational ice model in Copernicus CMEMS.

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).

We have changed the structure of Section 4.

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.

We have provided a short overview in the beginning of Section 4.

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.

Because absolute SIT and feature differences are fit and the training data set is quite large and representative we can assume that there will not be overfitting, at least not in significant amount. Also the linear approach is not especially sensitive to overfitting with a large data set. The non-negative LSQ fit also gives relatively small weights for the less significant feature differences. We now also provide a plot of the feature difference linear combination vs. the SIT difference. If the algorithm will be further developed towards operational use then multi-year training data to guarantee a representative training data set will be 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.

We have included comparisons to CS2SMOS SIT.

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.

The land mask tone has been changed to white to be able to better distinguish between the dark open sea/ smooth level ice areas and land.

Technical Corrections:

Line 5: Period should be a comma after ORAS5.

Corrected.

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.

Now using CS2 throughout the text.

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

Corrected.

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.

The parentheses are provided by latex using TC style files.

Line 31: Remove redundant ‘the’
Removed.

Line 36: ‘uncertainty’ should be ‘certainty’ in this case

Corrected.

Line 38: ‘TIR’ should be defined first.

Now defined.

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

Corrected.

Line 98: Change ‘there is’ to ‘there are’

Changed.

Line 111: ‘CS-1’ should be ‘CS-2’

Corrected.

Line 112 : Change ‘is’ to ‘are’

Changed.

Line 116: Change ‘very close’ to ‘is very close’

“is” added.

Line 131: Remove ‘from’ at end of sentence.

“from” removed.

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

Corrected.

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

Used S-1 also here as it has already been defined earlier.

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

Have been defined now.

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

Changed.

Line 254: Does MS = meanshift?

Yes, now explained when first mentioned in the previous sentence.

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

We think a list is clear. Could also be a table in the final version if considered better by the CS editor.

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

Done.

Line 531: Melt-down is misspelled

Corrected.

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

All units are cm. Units added.

Table 6 caption: Remove redundant ‘averages’ .

Removed.

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

Yes, corrected.

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

There are only three SIT categories as described in the corresponding section. Using the same scale as in other figures shows how the thickest MODIS SIT is distinguished from the thinner MODIS SIT. Yhios is the main purpose of this figure because thin ice classes are not very well distinguished by CS2 anyway.

Thank You!
Authors of the manuscript