Response to the reviewer 3 comments on the manuscript TC-2021-185,

Thank You for reviewing our manuscript and for the constructive comments. We have tried to take all the reviewer 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.

Obtaining sea ice thickness in a width swath scale is a welcome topic in the sea ice community. This manuscript gives an interesting method to estimate sea ice thickness from CryoSat-2 and Sentinel-1 data. Results of the manuscript indicate the possibility of overcoming the drawback of sparse measurement of altimeters by combining the texture feature of SAR imagery. Although the authors present a novel idea to solve the problems of sea ice thickness estimation, the manuscript doesn't meet the standard for publication at present. Because there are too many grammatical errors, even some figures are missing. The authors should check whether the submitted manuscript is the final version. In addition, further improvement is needed in the structure and logic of the manuscript, especially in description of methodology. Another point I concerned about is that the estimated ice thickness still has a relatively large bias. I therefore strongly recommend that the authors revise the manuscript carefully.

General comments:
1. Title and section 1 "Introduction"
(1) The data, experiments and results used in this manuscript are conducted in the Kara and Barents Seas. So I think the title should clearly define the area of research rather than the Arctic.

We have revised the title and the introduction section.

(2) The paper would benefit from a better introduction. The authors should briefly introduce your research idea, the difference from previous work, and the advantages and disadvantages of the proposed method.

We have revised the introduction section.

(3) The state-of-art could be described more logically. You can start with the development of ice thickness estimates from radar/laser altimeters, then introduce the development of other sensors including SMOS, MODIS, and SAR, and followed by introducing the multi-sensors fusion method.

We have rearranged the introduction section describing the existing SIT estimation methods.

2. Section 2 "Study area and period"
It is very useful to give a detail information about weather conditions. But this information is not used to support analysis on the accuracy of sea ice thickness estimation (see Section 5).

The weather conditions included mainly tell the reader that the weather has been below zero during the study periods (with available CS2 SIT), except October – early November. During these periods there may be some effect of wet snow cover to the results, otherwise the snow cover in general was dry. Included a sentence on this in Section 5.

3. Section 3 "Data"
(1) Please check equation 1. This equation is for laser altimeter not for radar altimeter.

We have included an equation for radar altimeter assuming dry snow cover (transparent snow layer for the radar).

(2) For Cryosat-2 data, you used Baseline-C product. Why not use Baseline-D? The accuracy of the Baseline-D product is higher.

The study was made originally already in 2019 and then only Baseline-C data was available. We'll revise the manuscript using newer CS-2 SIT data, recalculate everything, produce new table and figures and rewriting the conclusions in the revised version, assuming TC editors will provide us enough time for this work.

(3) For snow depth and density, Warren 99 is used in this paper. The snow depth data of Warren 99 is very different from the actual snow thickness. So I doubt that the use of incorrect snow thickness data will cause a higher bias. Have you tried the snow thickness product of the microwave radiometer?

W99 snow depths are indeed less than optimal for our study area. However, at the time of processing the CS2 data (already in 2019) this was the best estimate available. For the revised manuscript, we consider using an improved CS2 product which uses an MWR based snow estimate over areas where W99 is known to produce unrealistic values.

(4) In this paper, the authors used the ice thickness data retrieved from CS-2 data to interpolate/extrapolate between CS-2 tracks. Since the interpolation and extrapolation are based on SAR backscattering and texture features, I think maybe ice freeboard is more suitable for interpolation than ice thickness. This is because (a) radar backscatter and ice freeboard are statistically related as Similä reported. (b) The snow layer can be considered as transparent under dry snow conditions for Sentinel-1 and CS-2, and CS-2 actually measures ice freeboard directly. (c) Ice freeboard can reduce the interference caused by incorrect snow thickness.

This study has been made using CS-2 SIT. This suggestion makes sense but we'll use SIT in this manuscript. We have included this topic in the discussion. SIT is the parameter of interest anyway. In the revised manuscript we'll use newer CS-2 Sit data which includes better snow depth information.

(5) Does the model reanalysis data include snow thickness? Ice freeboard can be calculated using equation (1) if snow thickness information is available.

We do not have modeled snow depth data. Included a sentence on this in discussion.

4. Section 4 "Methodology"

(1) For SAR data, should open water, lead, and polynya be identified and removed after segmentation? Will the patches containing water/lead/polynya affect the estimation of ice thickness?

Large open water areas are excluded based on the SIC estimation, excluding low SIC segments, made before the SIT estimation. Smaller open water patches or leads are included in the segments but we estimate that their contribution is minor.

(2) Lines 319-324: each segment is assigned by the medians of backscattering. Why not use the mean value, but the median? Intuitively, the average value is more compatible with the physical significance.

We have used median here because it is more robust w.r.t. outliers, especially with a restricted number of samples. If more samples were available then also e.g. mode value could be considered to characterize the segment SIT by only one value. Actually, in most cases there is not much difference between average and median. Either average or median may include error because the SAR segments have different shapes and the CS2 measurements are just along a measurement line within the segment. Average with samples over the whole segment in a fixed grid would make more sense, but still it may be something not measured at any grid point. Now the median presents a typical value measured within a segment. On the other hand if we have a nice symmetric single-modal (e.g. Gaussian) SIT distribution within a segment then average, median and mode would be the same. In the case of a restricted number of measurements median is a "safe" and reasonable selection.

(3) I feel confused about the description between Line 330 and Line 337. How can I get the segment difference function T before least squares fit? In my opinion, the coefficients ct, cd, and cs are calculated by least squares fit, therefore the segment difference function T should be given beforehand.

We have changed the order.

(4) At the beginning of section 4.3, it said that “Because SIT was significantly overestimated for the 2016 training data”. This sentence is very abrupt since the estimated ice thickness has never been provided before.

This sentence should be “… compared to modeled and AARI SIT.” Also added a sentence before this sentence.

(5) I think section 4.1 “S-1 preprocessing” could be moved to section 3. The overview of the method as described in Fig. 6 could be moved to the beginning of section 4, and each step could be explained in more detail.

Moved and included more details.

5. Section 5 “Results”

(1) In the paper, the data of 2016 for training and the data of 2017 for testing. Could the results be improved by using the data of two years for training (not all data)?

Not significantly. The results were quite similar when using 2017 for training and some 2016 images for testing. The conclusion is that the tendence to underestimate w.r.t. CS2 SIT or CS2SMOS SIT is inherent for the proposed algorithm. Included this topic in the discussion section.

(2) Line 384: why the SIT at 50 cm has zero bias against the CS-2 estimates for the ORAS5?

This is probably due to the overestimation of ORAS5 for SIT under 50 cm. For the ice thicker than 50 cm there is a positive bias. Added a sentence at this point.

(3) The main issue of this paper is that its results are not compared with reliable ice thickness data (e.g. OIB data). The model reanalysis data don’t provide ice thickness distribution and spatial pattern with high resolution. I think the authors can compare the retrieved ice thickness with ice type and texture of SAR images. I also hope to see that the change of the spatial pattern of retrieved ice thickness before and after remapping.

Unfortunately there is not much reference in-situ SIT data available over the study area. We included a comparisons to the CS2SMOS SIT, this comparison seems to show significant underestimation, however. IceSat measurements Reliable ice types from SAR are produced by visual interpretation of ice analysts and they are present in the AARI ice charts. We have not included a comparison to SAR texture and SIT. It could be included but in many ice areas SAR texture is not correlated with SIT and this kind of comparison could give incorrect information. In this study we only assume that similar SAR segments within a reasonable spatial and temporal distance exhibit similar ice with a similar SIT. The remapping only maps SIT to better correspond the model SIT (distribution) so the spatial pattern is not changed because the order of the SIT values is preserved in the mapping.

6. References and figures

(1) The format of references does not conform to journal requirements.

The reference format has been updated. It will be finalized in for the final version.

(2) All the captions of figures are too simple.

We have updated the figure captions.

Specific Comments:

1. The abstract is too simple. Some numerical conclusions are needed.

We have included some (numerical) conclusions.

2. Line 10: “SIT” has been defined in previous sentence.

Corrected.

3. Line 38: “TIR” should be defined first.

TIR defined.

4. Line 49: “km” should not be used in italics.

Corrected.

5. Line 55: “Simila” → “Similä”.

Corrected.

6. Line 55, 56 and 57: “F” should be used in italics.

F removed here, it is not necessary here.

7. Line 75: “10:th” → “10^{th}”

Corrected.

8. Line 85: “55o” → “55°”, “70o” → “70°”.

Corrected.

9. Line 84: “coordinate system (CS):” → “coordinate system”.

Corrected.

10. Line 89: “show in 2” changes to “show in Fig. 2”.

Corrected.

11. Line 91: “show in 3” changes to “show in Fig. 3”.

Corrected.

12. Line 99: “21 Apr” → “21th Apr.”.

Corrected.

13. Line 103: “first week of Apr” → “the first week of Apr.”.

Corrected.

14. Line 105: “Apr” → “Apr.”.

Corrected.

15. Line 111: “15-26 Mar” → “15th – 26th Mar.”.

Corrected.

16. Line 117: “25 Dec” → “25th Dec”.

Corrected.

17. Line 121: “Dec” → “Dec.”.

Corrected.

18. Line 127: “earlier satellite radar altimeters” → “traditional pulse-limited altimeters”.

Changed as suggested.

19. Line 131: “F” has been defined in previous sentence.

Corrected.

20. Line 147: “SENTINEL-1” → “Sentinel-1”.

Corrected. We use S-1 throughout the text for Sentinel-1 (after defining it in the introduction section).

21. Line 170: “(Afanasyeva et al., 2019) states” → “Afanasyeva et al. (2019) states”.

Corrected.

22. Line 174: “km” should not be used in italics.

Corrected.

23. Line 194: “(Zuo et al., 2017, 2019)” changes to “(Zuo et al., 2017; Zuo et al.,2019)”.

Corrected.

24. Line 232-237: “i” should be used in italics.

Corrected.

25. Line 250: what is the mean of “bpp”?

Bits per pixel, opened.

26. Line 254: “MS” meanshift?

MS is used for meanshift in the manuscript. Now it is explained when appearing for the first time.

27. Equation (3): Why is there a square on the log term? The define of Entropy should be sum(p×log(p))。

Corrected. Two is the basis of the logarithm. Not an exponential. The basis is now a subscript in the eq.

28. Line 306: “h” has been defined as thickness in previous sentence.

Changed h to d (distance).

29. Equation (6): what is the mean of “H”?

Changed H to N. N_h is the set of pixel with the mutual distance d within the study window and |N_h| is the number of these pixels.

30. Line 308: “i, j, and h” should be used in italics.

Corrected.

31. Line 323: “Fig. 3” and “Fig. 4”, I think the number of figure is wrong.

Corrected the figure reference.

32. Line 325: “L1 difference” Does the L1 means L1 norm?

L1 difference is the absolute difference, now explained in the text.

33. Line 335: “2016 CS-2 thickness” → “The 2016 CS-2 thickness”.

Corrected.

34. Line 424: “60/80” → “60/80 cm”.

Corrected.

35. Line 458: “Table 5”? I think the number is wrong

Yes, corrected. It is Table 8 in the updated version.

Thank You!
Authors of the manuscript