

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.

(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.

(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.

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).

3. Section 3 "Data"

(1) Please check equation 1. This equation is for laser altimeter not for radar altimeter.

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

(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?

(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.

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

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?

(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.

(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.

(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.

(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.

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)?

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

(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.

6. References and figures

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

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

Specific Comments:

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

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

3. Line 38: "TIR" should be defined first.

4. Line 49: "km" should not be used in italics.
5. Line 55: "Simila" → "Similä".
6. Line 55, 56 and 57: "F" should be used in italics.
7. Line 75: "10:th" → "10th"
8. Line 85: "55°" → "55°", "70°" → "70°".
9. Line 84: "coordinate system (CS):" → "coordinate system".
10. Line 89: "show in 2" changes to "show in Fig. 2".
11. Line 91: "show in 3" changes to "show in Fig. 3".
12. Line 99: "21 Apr" → "21th Apr".
13. Line 103: "first week of Apr" → "the first week of Apr".
14. Line 105: "Apr" → "Apr".
15. Line 111: "15-26 Mar" → "15th – 26th Mar".
16. Line 117: "25 Dec" → "25th Dec".
17. Line 121: "Dec" → "Dec".
18. Line 127: "earlier satellite radar altimeters" → "traditional pulse-limited altimeters".
19. Line 131: "F" has been defined in previous sentence.
20. Line 147: "SENTINEL-1" → "Sentinel-1".
21. Line 170: "(Afanasyeva et al., 2019) states" → "Afanasyeva et al. (2019) states".
22. Line 174: "km" should not be used in italics.
23. Line 194: "(Zuo et al., 2017, 2019)" changes to "(Zuo et al., 2017; Zuo et al., 2019)".
24. Line 232-237: "i" should be used in italics.
25. Line 250: what is the mean of "bpp"?
26. Line 254: "MS" meanshift?
27. Equation (3): Why is there a square on the log term? The define of Entropy should be $\sum(p \times \log(p))$.
28. Line 306: "h" has been defined as thickness in previous sentence.
29. Equation (6): what is the mean of "H"?
30. Line 308: "i, j, and h" should be used in italics.
31. Line 323: "Fig. 3" and "Fig. 4", I think the number of figure is wrong.
32. Line 325: "L1 difference" Does the L1 means L1 norm?
33. Line 335: "2016 CS-2 thickness" → "The 2016 CS-2 thickness".
34. Line 424: "60/80" → "60/80 cm".
35. Line 458: "Table 5"? I think the number is wrong.