

## Referee report for “A simple model for daily basin-wide thermodynamic sea ice thickness growth retrieval” by James Anheuser et al.

The authors have made a significant improvement to the manuscript addressing my comments. Especially, I appreciate the authors’ hard work on the inclusion of sea ice drift in their methodology. The presented methodology and results in this manuscript are a novel and valuable addition to the sea ice community, but the manuscript still needs some clarifications. I, therefore, recommend the paper for publication following minor revisions.

### Comments

Title: Although the key result of this study is the daily basin-wide sea ice thickness record, the new title does not include anything about sea ice thickness. I think it is better to show that this paper is about sea ice thickness in the title. How about something like “A simple thermodynamic model for simulating daily basin-wide sea ice thickness using satellite passive microwave measurements”?

L2: I think it is not appropriate to indicate the algorithm of Lee and Sohn (2015), which was developed 7 years ago, as a “recently” developed algorithm.

L11: The word “equally” overstates the result of this paper. I suggest using “comparable” instead. Moreover, the authors should state clearly which quantities are comparable between SLICE and PIOMAS.

L24-L33: I think the literature review is still weak compared to other sections. The novelty and significance of this study can be highlighted based on the solid literature review. There are various sea ice thickness retrieval methods besides the methods using space-based altimetry only. For example, there is an algorithm for thin sea ice, simultaneous estimation of snow and sea ice thickness by combining satellite altimeter and radiometer measurements (Zhou et al., 2018; Shi et al., 2020), and the simultaneous estimation using two satellite altimeters at different frequencies (Kwok et al., 2020). Or if this study focuses on retrieving the sea ice growth rate, then relevant studies should be introduced.

L49-L50: The methodology also requires good initial guesses for sea ice thickness as well as passive microwave observations.

L65: It *as* an → It *is* an?

L87-L106: I think these two paragraphs can be shortened and moved into the introduction section.

L112: Where did you get the CS2SMOS data? Please provide data availability information.

L123: Also please provide data availability information for the QuickLook product.

L129: Two acoustic rangefinder sounders positioned above and below the ice can measure sea ice thickness if there is no snow on sea ice. How can sea ice thickness be measured with snow presence?

L185: I think the sentence “In Lee et al. (2018) ... in 1987” is not necessary.

L205: Explain why there are no upwelling and surface-reflected downwelling atmospheric TBs

in equations (2) and (3). I also want to confirm that the bias correction for estimated  $T_{si}$  is not applied in the revised manuscript.

L209: horizontal, vertical → horizontally, vertically

Figure 2: I found Figure 2 is not mentioned in the manuscript. Please mention it at an appropriate place.

L332: What density value is used for MYI? I can see value for FYI only. Authors may refer to the most recent research by Jutila et al. (2021) and Lee et al. (2021) on the sea ice density issue.

L347: It is a little bit strange that Figure A1 appears after Figure A2.

L365: I see that SLICE sea ice thickness is generally greater than buoy sea ice thickness and the difference between them increases with time. It is better to make some discussion/explanation on these results.

Figure 3: Please explain the meaning of color (red and blue) in the figure caption. Y-axis has only two ticks, which is not quite informative. It would be better to make the figure more informative.

L395: The word “improved” may not be a good choice because CS2SMOS is not a ground truth measurement (snow depth, sea ice densities, etc. are assumed). It should be better to say such as “shows better consistency”. Besides, I think the reason why SLICE is closer to CS2SMOS than PIOMAS is that SLICE uses CS2SMOS sea ice thickness value for its initial condition. Therefore, it becomes logical circulation if more consistency with CS2SMOS means “improvement”.

Figure 5: I suggest exchanging the x- and y-axis. It is generally easier to read plots with the reference variable on the x-axis.

L405: I suggest removing the word “new”.

L419-422: I don't think these sentences agree with the result shown in Figure 3. The difference between SLICE and buoy sea ice thickness increases with time even though the initial value is the same.

L427: These assumptions are reasonable because of what?

Figures 6 and 7: Is there a reason that K21 is not included in Figures 6 and 7, disturbing the consistency of the paper?

Figure 8: How did you calculate sea ice volume? Did you multiply sea ice concentration, grid area, and sea ice thickness? Is the 95% sea ice concentration criterion also applied to SC2SMOS and PIOMAS sea ice volume? Were three volumes calculated and compared based on the same area/criteria?

L443-444: Sea ice motion product used in the SLICE method also includes near-surface wind vectors from atmospheric reanalysis.

L479-481: Again, this conclusion can only be made upon a solid literature review. Please look for more state of art sea ice thickness observation methods.

## References

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