Summary

This paper presents a new technique for understanding fractional sea ice coverage in the Arctic, by developing a Linear Ice Fraction (LIF) product from ICESat-2 ATL07 data. It's great to see the high-resolution capabilities of ICESat-2 being used for this novel application. The paper was well-structured and enjoyable to read, and I have just a few comments to address prior to publication.

We thank the reviewer for their thoughtful and constructive comments. We appreciate the time and effort they took to read and evaluate our manuscript. The suggestions and feedback have helped us clarify and improve the presentation and interpretation of our results. Below, we provide detailed responses to each comment and describe the corresponding changes made to the manuscript.

Comments

L6-7: The statement comparing winter and summer biases is a little misleading. Without the further context provided in the paper, it reads as if summer biases are consistency smaller, rather than skewed by the NT algorithm. It would be useful to highlight here that in most cases, summer biases are larger. See also my comments on Section 3.2.

Thank you for your comment- we addressed this in Section 3.2 and have added this section in the abstract:

"Despite the low mean bias in the summer, uncertainty increases in the summer due to complex surface conditions, leading to a wider spread in SIC biases than in winter."

L12: "…measurements of the sea ice surface **with PM data** to enhance…". IS2 LIF is still dependent on PM SIC data.

IS2 LIF is not directly dependent on PM data. Indeed the IS2 ATL07 product extent is confined by the PM SIC, as the calculation of LIF can only exist in regions where PM SIC exceeds 15%, ATL07 and the LIF do not use radiometric features similar to PM brightness temperatures and are calculated independently of passive microwave retrievals. Since our focus is on leads within highly concentrated ice, the 15% constraint does not affect our analysis.

L26: Quantify "narrow", because it's an important point for justifying why LIF are useful

Added "(ranging from one meter to hundreds of meters)"

L41: I disagree with the introduction of LIF being an independent measure of sea ice presence. The LIF is developed using IS2 data that rely on a PM concentration product to determine sea ice presence, so LIF is more complimentary than independent. Please make this clear through the paper.

Please see the response to the comment above. We have clarified this point in the revised text at the end of the description of the LIF calculation:

The ATL07 product is limited to regions with passive microwave SIC > 15\%, but LIF is derived from the ICESat-2 surface type classifications that rely solely on ICESat-2's photon cloud and not surface brightness temperatures and is thus independent of passive microwave inputs, especially given the high-concentration ice we consider here.

L50: “**Then,** using…”

Added “Then,”

Table 1: This might be an EGU issue, but the date formatting in the table wasn’t great to read

Reformatted the dates per egu style guidelines

Table 1, row 2, column 6: Remove “–“

Corrected with a previous comment on the data formatting

Table 1, row 5, column 6: Do you mean 450 and 430?

Yes, corrected.

L76: “…advanced **over the satellite period**…”

Added “over the satellite period”

L115: “instrument” > “instruments”

corrected

L120: “utilizing” > “utilizes”

corrected

L135: The OIB acronym hasn’t been defined

Now defined at the beginning of the paragraph

L142-143: What do the authors mean by "outliers", and why this becomes more of an issue when MPF is greater than 50%?

By “outliers,” we are referring to images where the very high MPF (MPF>50%) may not be representative of actual surface conditions- due to either small image footprints that are not representative of the whole PM footprint area or potential misclassifications in the image processing routine. This is only 2% of images. We have clarified this in the text.

We have edited the text in the manuscript:

In the summer, we restrict the analysis to images with MPF<= 50% to reduce the influence of potentially misclassified images that may produce unrepresentatively high melt pond fractions.

L149: Remove “(box)” ?

removed

L152: “these products” > “the PM products”

corrected

L153: Should the “(2)” say “(Figure 2a)” ?

Yes, thank you. Corrected.

L157-158: I couldn’t make much sense of this sentence. What do the authors mean by “strong similarity in patterns” ?

Changed to “NSIDC biases have a mean and range similar to the BT biases.”

Section 3.2: The results here are particularly interesting, and I’d like a bit more information on why PM products exhibit a positive SIC bias in summer, and why it’s larger than winter. In Section 1 the authors explain that melt ponds on the sea ice appear radiometrically similar to open water, so if anything I’d expect PM to underestimate SIC compared with imagery. It would be great to add some brief text relevant to this in the abstract and Section 1 too.

This is an important point that warrants further discussion. We have updated Section 3.2:

“We find that the NT product provides the lowest SIC estimates among the algorithms evaluated, with this negative bias more pronounced in summer than winter. This is consistent with findings from Kern et al., 2020, who showed that NT products tend to underestimate SIC in the Arctic during summer due to their high sensitivity to surface melt and use of fixed, hemispheric tie points that do not capture evolving surface conditions. In contrast, we found products using the BT, NT2, and NSIDC algorithms tend to overestimate SIC, with biases of 5%–10%, consistent with Kern et al., 2020. Kern et al., 2020 also identified the OSI SAF product as having the lowest absolute bias, which aligns with our findings (Fig. 2b and Table 2).

These varying biases reflect the challenges PM algorithms face in summer when complex surface conditions—such as widespread melt ponds, wet snow, and variable ice concentrations—distort the microwave signal. While melt ponds can cause underestimation when misclassified as open water, they can also lead to overestimation when their presence affects the determination of tie points. Algorithms like OSI SAF attempt to mitigate this by using daily-updated dynamic tie points, whereas NT and NT2 rely on static tie points that are not adapted to melt season variability. These contrasting sensitivities to melt processes contribute to both under- and overestimation of SIC and explain the wider spread in PM SIC error observed in summer (Fig. 2b) compared to winter (Fig. 2a).

L168: “Figure 2b **and Table 2**”

Added “and Table 2”

L172: The NT2 acronym hasn’t been defined

Now defined in the algorithms section

L172-173: Could the authors explain why they find this interesting? Because the changes to NT2 weren’t intended to account for ponding.

We agree that the NT2 algorithm was not designed to account for melt ponding. We did not intend to suggest otherwise but highlight that both the NT algorithms (original and NT2) show the greatest biases at high melt pond fractions. We believe this is worth noting, as it suggests even the new algorithm remains sensitive to surface melt conditions. We removed the word “interesting” to prevent this misunderstanding.

L182: An IS2 footprint of 10 m was stated in Section 1, and 11 m here

Changed to 11 m in section 1 and reference added.

L187-188: What is meant by “likely recorded”? And what impact would this have on the IS2 products?

This sentence was reworded for clarity: “Kwok et al. (2019b) found that IS2 can consistently resolve leads as narrow as 27 m, although due to the incidence angle of ICESat-2 relative to the orientation of the lead, finer scale cracks are likely still represented in IS2 sea ice products (Hell and Horvat, 2024).

General: I suggest each author has another readthrough and checks for clarity and accuracy in the text. I noticed some issues with grammar/typos/formatting (citations and symbols).

Thank you for the suggestion, the authors have read through the manuscript again.