Review
EGUsphere
Manuscript Number: egusphere-2025-3286
Title: Estimating Arctic sea ice thickness from satellite-based ice history
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General comments:

Arctic sea ice thickness is one of the most crucial variable to monitor due to its direct link to sea ice volume and climate change impacts. However, despite its importance, measuring sea ice thickness remains a significant challenge, especially when compared to sea ice extent. To address this gap, the authors employ an innovative technique and investigate the following question: Can sea ice thickness be retrieved from its thermodynamical growth history using satellite-derived ice motion and concentration data?

In this work, the authors relied on backwards trajectories of virtual sea ice particles from ASMR-E and AMSR2 to determine the formation dates and the drift paths (for a maximum duration of 4 years). Then, using ERA5 atmospheric variables, they calculated the surface heat budget to estimate the growth and melt of each particle. Through this approach, the authors successfully reconstructed sea ice thickness and age distributions for each virtual particle.
This method enabled the retrieval of daily sea ice age distribution and thickness, from which monthly averaged values were derived. The comparison with Upward-Looking Sonars (ULS) in the Beaufort Gyre shows a strong agreement in temporal variability. To align the magnitude of ice growth/melt with observations, the authors determined and applied a scaling factor of 0.25, which was subsequently validated against ULS data from Fram Strait – a region representative of broader Arctic conditions. This study's limitations are thoroughly discussed, providing a comprehensive overview of its scope and constraints.

The paper is clear and well written, which makes it pleasing to read. The context and method are thoughtfully described. The results are clearly explained and discussed properly in the corresponding section. In my opinion, this is a great paper which could be improved by splitting the section 5 in two distinct sections: "Discussion" (limitations and last results) and "Summary" (short conclusions).

In the following pages, I address several points that requires the authors' attention and I hope they will help improving the present manuscript:

I appreciate the thoroughness of the study, but I suggest that the applicability of the method across the entire Arctic could be demonstrated in a more robust manner. Specifically, I question the authors' decision to evaluate only the 2016–2018 period, given that Sumata et al. provide 30 years of data. To strengthen the analysis, it would be valuable to extend Figure 7 to cover 2016–2022, ensuring consistency with the timeframe discussed.
Additionally, I recommend to compute metrics (RMSE, correlation) over the period 2007-2022 (over the thin and uniform regime in Sumata et al. 2022) to assess the method's broader applicability across the Arctic.
While I recognize the significant time investment required for such an analysis, presenting Figure 7 for 2016–2022 would provide compelling support for the study's conclusions. If readability becomes an issue, the extended figure could be included in the appendix or reserved for the review process.
To provide further context, I suggest adding a brief statement early in the paragraph noting that much of the Arctic sea ice is exported through the Fram Strait, thereby capturing a representative variety of ice conditions across the region.

I have checked the online visualization tool and find it to be a valuable resource for monitoring sea ice variables. However, I noticed instances of rapid fluctuations in sea ice thickness, including periods where thickness appears to increase sharply over just a few days (reaching ~4–5 m) and then decrease abruptly (down to ~2 m). One notable example occurs between 10–24 May 2021, particularly over the Canadian lakes.
Could the authors comment on the consistency and reliability of these sea ice thickness estimates, especially given the magnitude and speed of these changes? Additionally, the high variability in sea ice thickness —including peaks above 3 m—observed in Figure 7 may reflect these rapid growth and melt dynamics.

The study does not include a clear outline or plan at the end of Section 1. While not mandatory, providing an explicit plan for the reader could improve accessibility and help guide them through the manuscript. This is at the authors' discretion.
On a related structural note, Section 5 currently combines the summary, discussion, and additional results (e.g., Figure 11). For greater clarity, I suggest reorganizing this section to:

1. Separate the discussion into its own dedicated section (e.g., "Discussion"), where additional results could be explored in depth.
2. Condense the summary into a shorter, self-contained Section 6 ("Summary"), allowing readers who may only skim parts of the manuscript to quickly grasp the key takeaways.

This restructuring would enhance readability and better align with conventional scientific manuscript organization.

Specific comments:

L. 50: “hybrid approaches combining multiple satellite datasets have been developed.” The authors do not mention the hybrid approaches combining both model and satellite data to reduce the uncertainties compared to “standard” reanalysis datasets (i.e. PIOMAS). One such study is Edel et al. 2025. It is up to the authors to add a comment on this recent work.

L. 68: “...location and reconstruct its age”. I would argue that “...location to reconstruct its age” is easier to read.

L. 108-9: “These data were used both to develop the sea ice thickness estimation method and to evaluate its accuracy”. For me using the same data to develop and evaluate the method is inherently problematic. I would expect the data to be split in 2 parts, one used to develop the method while the other would be used for evaluation. It could be done by splitting the data in time (4 years for development, 2 for evaluation) or in space (use 2 ULS for development, 1 for evaluation).
Using another independent dataset would be more appropriate and rigorous, as using only one ULS in Fram Strait could be considered insufficient.
Another ULS at the North Pole existed from 2000 to 2008, and could be used to assess the applicability of your method earlier in your dataset (over 2007-2008): https://arcticdata.io/catalog/view/doi:10.5065/D6P84921 .

L. 111: “from 2016 to 2018 were used.” Why not more years? Please, see my main point above.

L. 163: “Ice formed during the first year (prior to 10 September 2018) declined rapidly in September 2018, and by 31 May 2021 accounted for only 0.2 % of the ice cover at that location. Ice formed between 10 September 2018 and 10 September 2019 accounted for approximately 10 % of the total area.” In my understanding, these results can be seen on the Fig. 4. If it is the case, please, indicate it properly.

Figure 5: In the caption, I would change “average age” to “area-fraction-weighted averaged age” for greater precision.

Figure 6: It would be more practical to add the colorbar (same as in Fig. 5) on one of the subplots to make this figure self-standing.

L. 275/Figure 8: Given that the scaling factor already ensures good agreement, Figure 8 may not be essential for supporting the results discussed in this section. I suggest either enhancing the figure to provide additional insights or considering its removal to streamline the presentation.
It would be insightful to assess how the correlation varies when applying scaling factors of 0.15 and 0.35 (arbitrarily chosen here). This analysis could provide valuable information about the sensitivity of Sea Ice Thickness (SIT) to the chosen scaling factor.

Figure 10: It would be more practical to add the colorbar (same as in Fig. 9) on one of the subplots to make this figure self-standing.

References:

Edel, L., Xie, J., Korosov, A., Brajard, J., & Bertino, L. (2025). Reconstruction of Arctic sea ice thickness (1992–2010) based on a hybrid machine learning and data assimilation approach. *The Cryosphere*, *19*(2), 731-752.