We sincerely thank the reviewer for their thoughtful and constructive comments, and appreciate the reviewer's positive assessment of our study and the recognition of its novelty and potential contribution to the development of satellite-based sea ice age and thickness products. The encouraging feedback is very motivating for us.

At the moment, the system does not yet allow us to upload the revised manuscript. We will submit the revised version as soon as the system becomes available.

General comments:

This study by Kimura and Hasumi introduces a new approach to estimate Arctic sea ice thickness by reconstructing its thermodynamic growth history from satellite-derived motion and concentration data. Virtual ice particles were tracked backward in time using AMSR-E and AMSR2 observations, and surface heat budget calculations were applied along their drift paths to model daily growth, which was then scaled against ULS measurements. The authors demonstrate that satellite-based ice age/backtrajectories in combination with a thermodynamic model can be used to reliably capture sea ice thickness (and annual and interannual variability).

At present, there are very few sea ice age and thickness products available, so the attempt to address this gap in this innovative and new way is highly welcome. The authors make use of a somewhat less commonly applied motion dataset, which renders the resulting ice age product independent of existing products, a very important aspect for any future cross-validation efforts of sea ice age datasets. The method is described with sufficient detail, and overall the paper is very well written and structured. I recommend publication, though I would encourage the authors to expand the validation section somewhat further and make data publicly available.

We have carefully considered all the comments and suggestions, and we provide detailed responses and corresponding revisions in the following sections.

Here are my two more general comments:

I believe that the validation is currently limited to the Beaufort Sea, which raises the question of whether the admittedly very good results can reasonably be transferred to the entire Arctic. I would encourage the authors to invest some additional effort here. There are a number of alternative data products available, in particular airborne measurements in the central Arctic, which are by no means inhomogeneous but rather represent local conditions very well as well as additional ULS datasets from other Arctic regions (e.g., Belter et al.). I believe the additional effort required would be relatively modest and would help to better justify the chosen correction factor. Furthermore, I would recommend that the authors consider dispensing with the correction factor altogether and instead frame this as an error inherent in the data. Such an error characterization should ideally also be included in the abstract, at least with a few key figures.

The correction factor of 0.25 used in this study is not meant to correct a computational error,

but rather reflects a conceptual adjustment inherent to our method.
In our approach, the surface heat budget is computed under the assumption of open-water conditions throughout the ice lifetime. Under identical thermal forcing, ice growth naturally slows as the ice thickens. Therefore, the cumulative ice thickness derived directly from the heat budget would overestimate actual thickness if interpreted literally. Importantly, this cumulative thickness is not itself an estimate of ice thickness, but serves as an indicator of the potential growth along the ice trajectory.
This potential growth indicator implicitly incorporates both thermodynamic and mechanical contributions. While the thermodynamic component alone would produce a gradually slowing growth rate, the increasing role of mechanical thickening in thicker ice leads to a combined effect that is roughly linear over time. This explains why the observed mean ice thickness can be successfully estimated from the cumulative growth. We have added a clearer explanation of this concept in the revised manuscript to avoid potential misunderstandings.

Another point that I personally find very important is that the presented dataset has not yet been made available. The manuscript states that it will at some point be accessible via a website. However, I believe that the availability of such datasets is a fundamental prerequisite for publication, as otherwise reviewers have no opportunity to directly examine the presented results and data. I am not sure whether this is also an explicit requirement of *The Cryosphere*, but before publication it should be ensured that the dataset is accessible and well documented. In addition, the motion dataset underlying the ice age product should also be made available. At present, only a link to a main webpage is provided, but not to the actual dataset itself
Thank you very much for pointing out the importance of ensuring full accessibility and transparency of the datasets used and produced in this study. We completely agree that public availability are essential for reproducibility and for allowing reviewers and readers to examine the results in detail. We have now made all datasets used and generated in this study publicly available:
Ice motion data: The gridded daily ice motion data have been published on Zenodo (https://zenodo.org/records/17694536).
Ice age data (maximum and mean): Both the maximum and mean ice age data produced in this study are publicly accessible on Zenodo (https://doi.org/10.5281/zenodo.17743866). Maximum ice age is also provided through ADS for additional accessibility.
Sea ice thickness data: The daily mean sea ice thickness data generated in this study have likewise been published on Zenodo (https://zenodo.org/records/17685488). They are also scheduled for inclusion in the ADS system with full metadata before final publication.
We have revised the Data availability section accordingly to provide direct links to each dataset.

Minor issues:
Title: "Arctic themodynamic? Sea ice thickness?
Thank you for your comment. The title of our manuscript is Estimating Arctic sea ice thickness from satellite-based ice history. While our calculations are based on the

thermodynamic history, we do not estimate sea ice thickness solely from thermodynamic growth.

Line 19 and 29: References are somewhat outdated. The most recent one listed is some 11 years old 😊

We appreciate the suggestion. We have updated the references to include more recent studies.

Line 66: obtained from sea ice motion data derived from passive microwave….

We revised the sentence as follows to clarify the approach:

In this study, we propose a novel approach to deriving sea ice thickness using a trajectory-based framework (e.g., Korosov et al., 2018), based on sea ice motion data derived from passive microwave radiometer observations.

Line 69: along each trajectory using XY

We revised the sentence for clarity:

"Along each trajectory, the cumulative surface heat budget is used as a measure of ice growth potential, and …"

Line 77: thermodynamic model combined with

We incorporated the suggestion as follows:

"Through the development and validation of this method, which combines a growth-tracking model with passive microwave observations, we aim to enhance the monitoring capability of sea ice thickness from passive microwave observations and contribute to a deeper understanding of polar climate dynamics."

Line 98: Provide number (error)

We have added:

Kimura et al. (2013) demonstrated that, even after five months of particle tracking, the positional error relative to drifting buoys remained below 50 km, even in regions with the largest discrepancies.

Line 104-110: I'm not entirely sure here: Wouldn't it be important for the comparison later to derive the modal value of the ULS observations? At the moment, the mean is being used, but the mean includes deformed ice, which is then compared against a thermodynamic model.

We clarified that comparison with the daily mean is appropriate in this study, as the thermodynamic calculation estimates the mean ice thickness, which implicitly includes dynamic growth. The objective is to estimate daily mean ice thickness rather than purely thermodynamic thickness.

Line 119: …according to changes in sea ice extent: What is meant by this?

We have clarified the sentence as follows:

Particles were initialized at 10 km intervals within areas where sea ice concentration exceeded 15 %, with their daily positions adjusted to reflect changes in the spatial distribution of sea ice.

Fig 2: Is it possible to zoom in or enlarge the fig?
We have enlarged the figure for clarity. We consider further close-ups unnecessary.

Caption Fig. 2: Held stationary or stopped?
We simplified the caption to:
Light blue dots mark the positions where particles reached open water.

Line 130: Why limited to 4 years? From my own experience (and looking at the past 2 years) there is lots of ice around that is older than 4 years?
The choice of a 4-year backward tracking period is closely related to the temporal coverage of the available satellite data. For instance, if a 4-year tracking is required, the computation of ice age and thickness can only begin 4 years after the start of satellite observations. Since there is a data gap between AMSR-E and AMSR2, extending the tracking period would also extend this gap by the same duration, reducing the overall temporal coverage of the dataset.
Therefore, a shorter tracking period allows for a longer and more continuous record of derived products. We selected 4 years as a practical compromise, because the areal fraction of sea ice older than 4 years is generally below a few percent across most regions. Treating all ice older than 4 years as a single category introduces only a minor influence on the resulting ice thickness estimates.
We have added an explanation of this rationale to the revised manuscript.

Line 138: This confirms https://www.nature.com/articles/s41598-019-41456-y
Thank you for the suggestion. Upon closer inspection of the paper you recommended, we found that the results shown in Figure 2 are similar to our findings. Therefore, we have added a citation to this paper in the revised manuscript.

Fig. 3 plus caption: I really like this form of presentation, as it makes the effect that ice concentration has on the distribution of ice age at the end visible. Perhaps the different periods mentioned in the figure caption could be displayed as a legend.
Thank you very much for this helpful suggestion. We have added a legend to indicate the different periods in the figure.

Fig 4: Same as for Fig. 3. Please add a legend. Red is not displayed.
Similarly, we added a legend. The red color is not missing, but its presence is too limited to be visible.

Line 200 – 205: Very interesting. Fun to read.

Thank you for your encouraging comment!

Line 216: May be some more up to date reference would be nice
We cited a classic and widely recognized paper on sea ice thickness distribution here to acknowledge its historical significance.

Line 230: The phrase 'it may also implicitly capture' sounds rather vague to me, and it is unclear why, in addition to thermodynamic growth, dynamic growth would be considered here. I think, in order for the sentence to remain as it is, the effect would need to be shown and that part expanded considerably.
We have clarified the text to explain that the assumption of open-water conditions over the ice lifetime allows the calculation to account for the contribution of mechanically thickened ice. The rationale for how both thermodynamic and dynamic growth are implicitly represented is described in detail in the Discussion section. This revision addresses the previous vagueness.

Fig. 8: I would directly include error assumptions, significance level, etc. in Fig. 8. It would also be nice to indicate the observation period, for example through a color coding of the figure.
We added lines for scaling factors of 0.15, 0.25, and 0.35, and included additional validation data, resulting in four scatter plots. We opted not to color-code by observation period to maintain clarity.

Line 279: Smoothing is generally fine, but in this case I don't see the necessity or the rationale for choosing, for example, a two-week time window. I would either elaborate on this part or leave the sentence out entirely.
The sentence regarding smoothing has been removed, as suggested.

General comment on validation: At present, the validation is strongly limited to the Beaufort Sea, which naturally makes the transferability of the results to the rest of the Arctic somewhat difficult. I would generally recommend that the authors broaden the validation. From my perspective, ship-based observations can equally be used for validation, as well as data from numerous airborne campaigns, some of which are publicly available. In addition, in the Russian Arctic or in regions primarily dominated by FYI, there are moorings that can serve validation purposes (e.g. https://tc.copernicus.org/articles/14/2189/2020/). In particular, since a correction factor is applied that is then used across the entire Arctic, a more comprehensive validation is required. I therefore recommend expanding this aspect, either through airborne data (e.g. EM-Bird data) or through additional ULSs.
We have incorporated validation using moored ADCPs observations in the Laptev Sea and extensive airborne EM surveys. These additions provide a more spatially comprehensive assessment of the ice thickness estimates.

Line 285: Can you provide more detail on how it was derived? For Fig. 7 its kind of clear how this is done… but how comes Fig. 6 into play?
Another point concerns the temporal validity of such a correction factor. The authors derive ice age for a period that clearly exceeds the coverage of the validation dataset on which the correction factor was ultimately developed. I wonder whether it might not be better to omit the correction factor altogether and instead simply acknowledge that the chosen approach overestimates the actually observed values by about 25%.
We clarified in Section 4 that the thermodynamic calculations serve as an indicator of potential ice growth rather than providing absolute thickness values. We also explained how the 0.25 scaling factor was derived to match the observed mean thickness in the Beaufort Sea. While this factor was determined from a limited validation period, its use is intended to provide a practical adjustment for comparing the cumulative growth indicator with observed thicknesses, rather than implying a universal correction.

Line 315: I would avoid terms like "reasonably accurate" in particularly after applying a correction factor.
This phrase has been removed to avoid ambiguity after applying the correction factor.

Line 325: I disagree with this statemtent. In particular, EM data cover large areas that have remained unconsidered in the validation and are generally very homogeneous, apart from the data in Fram Strait. Since the data were apparently already considered for use, it would be very interesting to present this comparison here as well, as otherwise it might give the impression of a somewhat selective use of data.
We have added validation using EM observation data and removed the previous statement to avoid any impression of selective use of data.

Last chapter / Fig. 111/12: The quality of the agreement in Fig. 11/12 (Fram Strait) is somewhat surprising to me. I would have expected a considerably poorer comparison, and in the end the good performance here is quite convincing.
The authors may wish to elaborate further on the limitations of the low-resolution data (both here and elsewhere in the manuscript), in particular the strong underestimation of drift speeds in the Fram Strait that may prefent from resolving daily to subdaily variability.
Regarding structure, it might be worth considering integrating the Fram Strait validation into the preceding chapter, as the separate treatment in the final chapter feels somewhat unusual.
Following the reviewer's suggestion, the Fram Strait validation results have been moved to Section 4 and integrated with the additional ADCP and EM validation. Text has been revised for clarity and discussion of limitations, including underestimation of drift speeds.

Line 407: It is very encouraging to hear about these plans. I believe that providing an additional ice age and ice thickness product, which employs different approaches and methods

than those already existing, will represent a highly valuable contribution to the scientific community. Many thanks for this excellent work — it was a genuine pleasure to read the paper.
Thank you very much for your kind words. Your comment greatly encouraged us.

Data availability: Please provide a direct link to the motion dataset that is used in this study. Please ensure that the sea ice age, mean sea ice age and sea ice thickness is pubilcy available prior publication
We have now provided a direct link to the sea ice drift velocity dataset, made the numerical data publicly available, and released the full-period ice thickness data obtained in this study.

We have carefully considered all of your comments, along with feedback from Reviewer 2, to prepare this revised manuscript. We sincerely hope that the revisions meet your expectations and improve the clarity and quality of our work.