Dear Reviewer, dear Editor,

We would like to thank you once again for your efforts to improve our manuscript! We have implemented the majority of the suggestions mentioned by the reviewers and responded to the most important comments in more detail in the attached point-to-point response.

Recently, a paper about ApRES-derived melt rates on the Fimbul Ice Shelf from Lindbäck et al. (2025) was published. Since we had previously included two similar studies in the introduction, we have now also included this study in the introduction and in Fig. 1.

Thank you for your time and expertise.

Kind regards,
Ole Zeising and co-authors

Authors point-to-point response on Referee Comment #2 to egusphere-2024-2109

L. 155–156: "To determine the melt rate, we smoothed the cumulative melt time-series by applying a 36h butterworth filter, and calculated the gradient in a ***7d moving window*** *which gives the 7d average melt rate."*

Reviewer: 7-d moving window doesn't actually provide a 7 day frequency cutoff, which is the more relevant quantity. Can you provide the frequency cutoff in the final analyzed melt rate time series? Also, it is unclear why you first apply 36 h low pass filter and then do moving average when both act as low pass filters. If you want 7 day cutoff why don't you just do 7 d butter worth right away?

Authors' response:
The melt rate is based on the average gradient within the 7-d moving window of the cumulative melt time series. The reviewer is right, that the 36h butter worth filter does not change much here, so we removed it and updated the melt rate values. Calculating the average gradient within a 7-d moving window attenuates periods shorter than ~16 days. We added this to the manuscript.

L. 176–177: "While this method likely overestimates sea-ice production due to simplifications such as ***neglecting ocean heat flux and solar radiation, it effectively captures temporal variability and seasonality.****"*

Reviewer: Do the neglect variables such as ocean heat flux not have temporal (seasonal) variability that could interfere with the results? Or is there some reason to assume these effects are small?

Authors' response:
It is common practice to neglect ocean heat flux for the heat balance method. This is because the information is very sparse or highly uncertain. In fact, this heat flux is likely negligible unless it is a sensitive heat polynya, such as the Maud Rise Polynya (see e.g. Lin et al. (2023), https://doi.org/10.1029/2022GL101859). During the main phases of sea ice production, solar radiation is almost zero and is therefore neglected in the model of Pease (1987).

L. 177–178: "Comparisons with other resolutions and reanalysis forcing reveal consistent patterns in variability but differences in magnitude by up to a factor of two."

Reviewer: Do you have a figure to show that?

Authors' response:
Direct comparisons of our results with previous studies are not possible due to differences in the regions and time periods considered. However, we generated a circumpolar dataset spanning 1992–2023 using SSM/I SIC data at 12.5 km resolution (Kaleschke, 2024). These results are comparable to previously reported values and broadly align with earlier estimates derived from the same sensor family (Janout and Kaleschke, 2024).

We further compared SIP estimates derived from SIC data at 12.5 km and 3.125 km resolutions, as well as from different reanalysis forcings (ERA5 and JRA55). Our findings indicate that while the seasonality and variability of the estimates are consistent, their magnitudes differ by up to a factor of two.

Janout, M., & Kaleschke, L. (2024). Gridded European circumpolar sea ice production fluxes (D1.4). Zenodo. https://doi.org/10.5281/zenodo.14192263

Kaleschke, L. (2024). EU project OCEAN:ICE Deliverable: D1.4 Gridded European circumpolar sea ice production fluxes [Data set]. Zenodo. https://doi.org/10.5281/zenodo.11652686

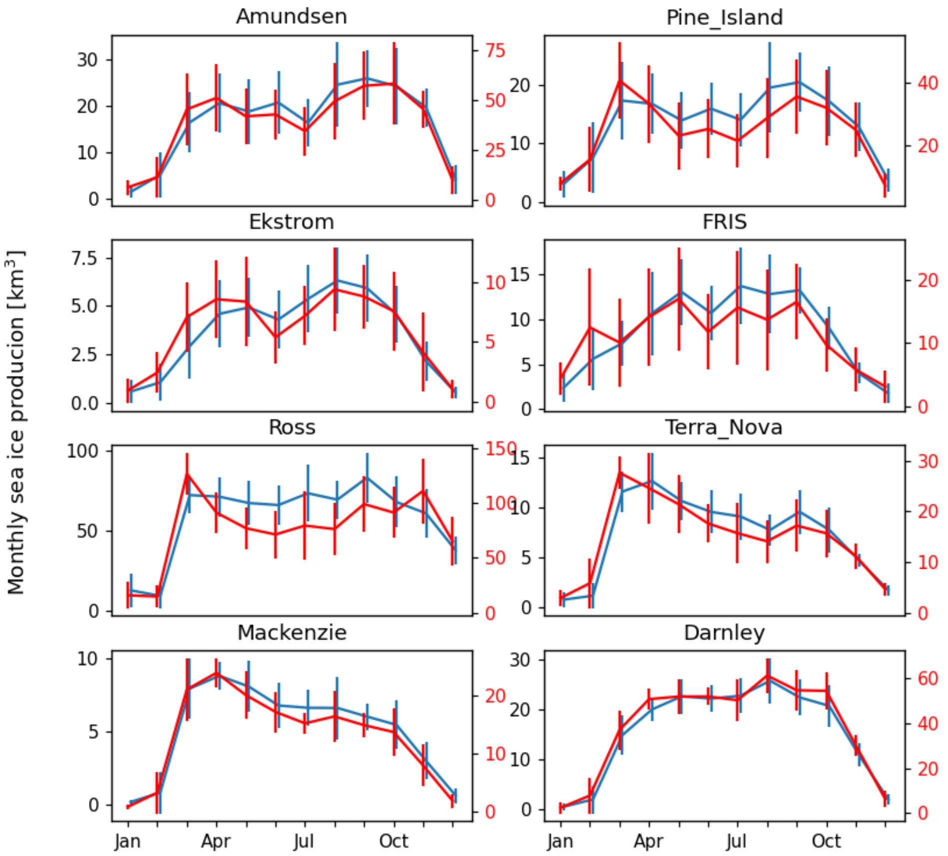

Fig.: Monthly sea ice production for selected polynyas from two data sources: (1) SSM/I 12.5 km ASI ice concentration with ERA5 wind and air temperature (blue), and (2) AMSR-2 3.125 km ASI ice concentration with JRA55 wind and air temperature. The solid line shows the 2012–2023 average, and error bars represent interannual standard deviation.

*L. 217–219: "**Omitting Coriolis' effect**, the system is parameterised along a one-dimensional flowline, described by four ordinary differential equations with prognostic variables for the plume thickness D, speed U, temperature T, and salinity S with details of the formulation being given in Jenkins (1991)."*

Reviewer: That doesn't seem to be an appropriate assumption for an ice shelf as wide as Ekstrom - what is the ratio of ice shelf with to rossby radius of deformation here?

Authors' response:
We acknowledge that the reviewer requests a more nuanced discussion of the applicability of the simple one-dimensional plume model. We addressed this in the manuscript. Here, the sentence provides an accurate description of the model formulation, which we prefer to leave it unchanged.

L. 222–223: "Given the confined configuration of the Ekström Ice Shelf ***along a quasi-one-dimensional flowline geometry****,* ***we assume that the idealised model adequately captures the basic features of the cavity circulation beneath this ice shelf.****"*

Reviewer:

- This might be the case for the ice flow, but not necessarily for the ocean flow - See, for example, numerous ocean modeling simulation results beneath a relatively small Pine Island Ice Shelf and the complexity of circulation there.
- I don't think you can make that argument based on scaling parameters (you can try or maybe you already have and if you succeeded, please provide the quantitative information in the paper). I understand the desire to use a simple model, but I think you need to acknowledge here in methods already that there isn't actually any basis for why it should capture the basic features of the 3D cavity circulation. You can still go ahead and use the simple model, but with being honest that the only motivation is its simplicity, and not actually the fact that it is justified or appropriate.

Authors' response:
We agree with the reviewer that the cavity circulation below Ekström Ice Shelf is likely influenced by rotational effects. In fact, we did neither state nor intend to claim that our idealized model captures the 3D cavity circulation (which seems to be the core criticism here and in other comments). While rising ice shelf water plumes indeed seem to manifest as (rotationally affected) near-geostrophic, topographically steered currents (Holland & Feltham, 2007), many three dimensional GCM simulations of (idealized and more complicated) of ice shelf cavities with similar confined, elongated flowline geometries such as the the Ekström Ice Shelf, show a systematic cavity wide circulation that can be conceptually be described as a quasi-two-dimensional overturning circulation, which manifests in three dimensions through topographically confined near-geostrophic currents – a recent example given by the MISOMIP simulations proposed by Asay-Davis et al. 2016 (https://doi.org/10.5194/gmd-9-2471-2016). Despite complexities at smaller scales, this overarching circulation is primarily driven by buoyancy forces due to water mass transformation from the ice-ocean interactions that are represented in the idealized one-dimensional plume model. Hence, we politely disagree with the reviewer, finding appropriate justification for the use of this model for representing the basic feature of and making it useful for studying the Ekström Ice Shelf cavity circulation, which provides additional motivation for its use beyond its convenient simplicity. To address the reviewer's concern, we revised the manuscript to clearly state the limitations of the model in the methods in addition to the discussion part:

"The Ekström Ice Shelf comprises a confined ice geometry along a quasi-one-dimensional flowline. The plume model approximates the averaged cavity circulation across the ice flow to provide an initial assessment of the dynamics of buoyant ISW that rises along the upward-sloping ice base. However, the three-dimensional circulation beneath the ice shelf is likely influenced by rotational effects. Consequently, the model results are only applicable to regions where the flow is constrained by topography (Jenkins, 1991). Further limitations of this approach will be addressed in the discussion."

*L. 235–236: "This end member was **linearly interpolated** to the observed water masses at 150 m (**constant to the surface**) derived from the PALAOA CTD time series, representing a late winter/autumn (Sept/Oct) WW extreme and a late summer/spring (Feb/Mar) AASW extreme."*

Reviewer:

- Why is linear vertical profile appropriate? Is that what the other available CTD profile suggests?
- What was constant and where?

Authors' response: A linear profile is consistent with the simplicity of our modeling approach that was pointed out by the reviewer (we specified this in the revised version of the manuscript). Existing profile data from Ekström and other ice shelves are ambiguous. In fact, we tried various profiles, e.g. prescribing AASW with a mid-depth pycnocline, or extrapolating the profile above the PALAOA CTD based on observed open ocean end-member values, only finding minor impact on the magnitude of our results, while the qualitative picture remains unchanged.
Updated sentence:
"This end member was linearly interpolated to the observed water masses at 150 m derived from the PALAOA CTD time series (and with temperature and salinity kept constant above to the surface), representing a simplified late winter/autumn (Sept/Oct) WW extreme and a late summer/spring (Feb/Mar) AASW extreme."

*L. 251–252: "The strongest melt event occurred in September 2020, when the melt rate increased from 0.21 to 1.81 m/a (**7-day average**)."*

Reviewer: Do you mean over 7 days duration? Else I don't understand what you mean

Authors' response:
We refer to the melt rate change between two consecutive 7-day bin-averaged melt rate estimates. Thus, the melt rate represents the average melt rate within 7 days. Within the 7-day window, even higher melt rate occurred at shorter times.

*L. 257: "Figure 2: Time series of 7-day average basal melt rate from autumn 2020 to spring 2023 (blue line) with **sub-weekly variability represented by the standard deviation (shaded area).**"*

Reviewer: Why don't you just show the melt rate times series before the 7 day averaging? It would make it clearer how abrupt is the onset of the melt rate events vs how much of it is smoothed out.

Authors' response:
With the approach we used to analyse the ApRES time series, we obtain a cumulative melt time series, from which we calculate the gradient within a 7-day window to get the melt rate. The cumulative melt rate is not an ideal signal. The 1-day average melt rate is to a certain extent affected by noise. Thus, we prefer showing the 7-day average melt rate.

L. 261–262: “When the sea-ice concentration reached a coverage of 90% in April and May, the growth rate initially declines, but increases again towards the end of winter.”

Reviewer: What is this dip in Jun/July caused by? lower winds or warmer temperatures? Perhaps including a plot of those two variables would be useful too, for completeness.

Authors' response:
The bi-modality of sea ice production is a common feature for other polynyas (see Fig. above). We suggest that this seasonality is related to the maximum ice concentration as shown in Fig. 3 of the manuscript, which insulates the ocean from the cold atmosphere and limits further growth. A further attribution of the underlying reason is difficult due to complex sea ice dynamics and interaction of the ocean and the atmosphere.

L. 268–270: “A cross-correlation analysis between sea-ice formation and melt rate, calculated for each season, revealed predominantly moderate (0.5 — 0.75) or low (0.25 — 0.5) correlation values ***for various lags of up to 26d****.”*

Reviewer: Do the lags have either sign, or do you always have?

Authors' response:
The lags are only positive (an increase in sea ice formation is followed by an increase in melt rate), which is described in the previous sentence.

L. 327–329: “However, a detailed comparison of the melt rates determined from ApRES measurements in this study with those estimated from satellite observations is difficult due to different observational periods.”

Reviewer: But can you still show the other satellite estimates, side by side, even if they are for different periods? It would be useful to have that anyway, to see whether there is some substantial change in character of the time series using different techniques or not. Finally, can your temperature time series may provide some indication of melt rate variability or of the stability of the conditions, which may be useful for tying the different time periods together?

Authors' response:
Previous comparisons of ApRES and satellite based estimated time-series have shown large differences due to the uncertainties in the satellite-based method (Vanková and Nicholls, 2022). Since non-validated time series based on satellite observations are not trustworthy so far, we do not want to compare the characteristics of the different time series observed at different times. Unfortunately, this means that we can’t use our time series to validate the satellite-based estimates as they don’t overlap.

L. 345–348: "In contrast to Fimbulisen and the Nivl Ice Shelf, the Ekström Ice Shelf also exhibits a more confined geometry and a deeper grounding line, which together promote the formation of ***coherent quasi-two-dimensional cavity overturning circulation that*** *is driven by the pressure-dependent formation of buoyant ISW."*

Reviewer: As mentioned above, justify using scaling arguments, or leave out

Authors' response:
Perhaps there is a misunderstanding, where the term "quasi-two-dimensional" did not intend to refer to the absence of rotational effects which the reviewer seems to refer to in their comment. The intention with this formulation was to describe the expectation of a relatively clean-cut estuarine-alike circulation, with (primarily) one inflow pathway along the sloping bottom and (primarily) one outflow pathway along the sloping ice base, which coherently spans the entire Ekström Ice Shelf cavity (primarily oriented along the ice flow path, but skewed by Coriolis). This is opposed to the situation at Fimbulisen, where observations (e.g. Nicholls et al. 2006) show the existence of multiple inflow pathways, and 3-D simulations (e.g. Hattermann et al. 2014) suggests a horizontal circulation pattern that is affected by a more complex bottom and ice shelf geometry with a highly asymmetric distribution of deep and shallow ice (and the later being also true for Nivilisen). To avoid ambiguity, we have dropped the term "quasi-two-dimensional" in our explanation in the revised version of the manuscript:
"In contrast to Fimbul and the Nivl Ice Shelf, the Ekström Ice Shelf also exhibits a more confined geometry and a deeper grounding line, which together promote the formation of a coherent cavity overturning circulation that is primarily driven by the pressure-dependent formation of buoyant ISW."

L. 393–394: "Running the plume model with various (idealised) geometries confirms the robustness of results about the influence of the cavity stratification in the simulated melt rates regardless of choice of the detailed flow path of the plume."

Reviewer: I thought the keel was focusing outflow because of rotational effects. Running plume model without rotation cannot account for geometric-rotational effects in any way.

Authors' response:
The assumption here is that (non-resolved) rotational effects focus the outflow along a near-geostrophic, topographically steered flowpath, as e.g. described in Holland & Feltham 2006, https://doi.org/10.1175/JPO2970.1), or seen in the 3-dimensional simulations of the Fimbulisen cavity (Fig. 8 in Hattermann et al. 2014, https://doi.org/10.1016/j.ocemod.2014.07.004). By prescribing the geometry along such a near-geostrophic flowpath, the geometrical-rotational effects are implicitly approximated in the one-dimensional plume model – and extreme localization of this concept is e.g. provided by Lazeroms et al. 2018 (https://doi.org/10.5194/tc-12-49-2018). Surely, the representation is incomplete, but at this point, we do not expect that a more sophisticated representation of the cavity circulation would make much difference to our main finding, which primarily relates the circulation strength inside the cavity to the ISW buoyancy relative to ambient water that enters the cavity, which, together with observational insights and inductive reasoning lead us to formulate a novel hypothesis for explaining observed melt rate variability below Ekström

Ice Shelf. We assume that a similar result could for instance have been obtained with a idealized model of the cavity overturning (e.g. following the approach of Walker & Holland 2003 (https://doi.org/10.1016/j.ocemod.2007.01.001)), or the box model formulation of Olbers and Hellmer (doi: 10.1007/s10236-009-0252-z), which are equally simplistic but conceptually diametral representations the cavity circulation compared to Jenkin's plume model. Using a more sophisticated 2D-plume model, or a fully resolved 3-D GCM simulation might be considered superior for more quantitative analyses, but those would face other challenges of e.g. providing more detailed accurate forcing and boundary conditions, including a well constrained three dimensional geometry of the cavity, as well as accurate representation of mixing processes inside the cavity (Gwyther et al. 2020, https://doi.org/10.1016/j.ocemod.2020.101569). Discussing all these aspects in detail may arguably be out-of-scope of this paper, which centers around (as was pointed out in previous reviews) a novel observational dataset of basal melt rates.

L. 399–401: "However, since these effects superimpose linearly on the simulated processes, we assume that the proposed impact of seasonal stratification on the plume dynamics is a robust result, while the detailed modulation of these dynamics remains subject for further studies."

Reviewer: There are definitely nonlinar tidal effects and currents in the system

Authors' response:
Agreed! We removed the term "linearily." A study that suggests how linear may be of leading order in some cases is shown in Jourdain et al. (2019), (https://doi.org/10.1016/j.ocemod.2018.11.001). Hence, we propose that the detailed modulation of these dynamics remains subject for further studies.