We appreciate the reviewers for investing their time and providing constructive comments on our manuscript. Overall, we revised the manuscript in accordance with their suggestions.

A brief overview of the changes is as follows:

Recalculation of the Index (Na/SO4): After a thorough re-evaluation of our data, we found that the majority of Na/SO_4 in the IND-36/9 core is of non-sea-salt (nssSO_4) origin. We agree with the reviewer that mechanistically justifying the Na/SO_4 ratio as a sea-ice fractionation proxy is difficult in this coastal setting, as marine biogenic signals likely dominate the sulfate budget. We have therefore recalculated the polynya index excluding the Na/SO_4 ratio. The refined index now integrates four independent and well-justified parameters: ssNa flux, $\delta^{18}O$, d-excess, and annual snow accumulation rates.

Improved Visibility of the 1964 Event: We revised the calculation methods to use rolling Z-scores rather than native proxy values and by removing the Na/SO_4 ratio, the 1964 polynya opening is now clearly evident in the new index as a synchronized peak across the remaining four proxies. This aligns our reconstruction more closely with historical Nimbus I satellite data and provides a more consistent validation of the index during the instrumental era.

Below, we explain the detailed changes we have made and present our reasoning for the suggestions we didn't follow. We hope these revisions are satisfactory and that the revised manuscript meets the journal's criteria. Our responses are tabbed and blue, following individual comments. The line numbers we refer to are those of the marked manuscript.

RC1: 'Comment on egusphere-2025-5820', Anonymous Referee #1, 12 Feb 2026

This paper attempts to construct, from ice core measurements, an index for the existence of the Maud Rise (or Weddell) polynya (MRP) over the last 200 years. The MRP was a major feature in the mid-1970s and a weaker feature at intervals since. It is clearly an important change in the ocean whose cause and effects are well worth investigating. Because there are only a few occurrences (and only one strong one) in the instrumental record, searching for evidence of past occurrences is a valuable way to approach this. The core used in the paper is well placed to "see" the MRP, and the dating is quite convincing, so the premise behind this work is a good one, and the paper should eventually be a useful publication. However, there are some major issues with the way it is presented and with the construction of the so-called polynya index that need to be addressed before the paper can be published.

We appreciate the reviewer's positive assessment of our study's premises. We have addressed all major concerns regarding index construction and the 1964 event by fundamentally restructuring our methodology. Specifically, we excluded the Na/SO_4 ratio because our re-evaluation confirmed that the majority of sulfate is of non-sea-salt origin. To improve robustness, we transitioned to a Cumulative Anomaly approach using 30-year rolling Z-scores rather than direct proxy values. This shift, combined with the removal of biogenic sulfate noise, has significantly enhanced the sensitivity of our reconstruction, making the 1964

> polynya opening clearly evident as a synchronized peak across our four primary proxies: ssNa flux, d-excess, and snow accumulation. These revisions, now detailed step-by-step in the updated manuscript, provide the mechanical rigor necessary for a reliable multi-century reconstruction of the MRP. We have responded to all suggestions in detail below.

The main concern over the polynya index is that the paper only explains to a limited extent how it is constructed and at least one component of it seems not to be well-justified. In Figure 7a we are shown 5 ice core parameters that are said to show links to periods of polynya activity, and section 3.3.1 tries to give a mechanistic reason why each should be associated with polynya presence.

The first issue concerns the use of Na/SO4: the authors have already justified that Na should be high in polynya years. But for this index (Na/SO4) to be a useful additional piece of information they need SO4 to be low in polynya years. The authors justify this on the basis of mirabilite precipitation on se ice, which would lower the SO4/Na ratio (ie raise the Na/SO4) ratio in sea salt aerosol. However by far the majority of SO4 in this core (based on the numbers given) must be non-sea-salt sulfate, produced from marine biogenic activity, something that to first order one might expect to be high in the somewhat open waters of the polynya. The authors therefore need to reconsider whether they can really justify (mechanistically) including Na/SO4 in their index; if they do they need a better justification.

> We appreciate the reviewer's insightful critique regarding the Na/SO_4 ratio. Upon further assessment of our ice core data, we confirm that a significant majority of the total sulfate in the IND-36/9 core is indeed of non-sea-salt origin. Given the proximity to the coast, these signals are heavily influenced by marine biogenic activity and ocean-related MSA products, which can obscure the specific sea-ice fractionation (mirabilite precipitation) signal. We agree that, mechanistically, justifying the Na/SO4 ratio as a reliable polynya proxy in this specific high-accumulation coastal setting is challenging, and the Na/SO4 peaks must be assumed spurious. We have now recalculated the polynya index by excluding the Na/SO_4 ratio. Our revised multi-proxy index now integrates four parameters: sea-salt sodium (ssNa) flux, $\delta^{18}O$, deuterium excess (d-excess), and annual snow accumulation rates. These four proxies are directly linked to the increased heat and moisture exchange and enhanced sea-salt production characteristic of open-ocean polynyas. Preliminary results with this refined index show that major events, such as the 1974 – 1976 Maud Rise Polynya, remain clearly identified, while the overall reconstruction is less susceptible to biogenic noise. The 1964 polynya event is also much better represented in the revised index. Additionally, the calculation methodology has been revised to use rolling Z-scores instead of native proxy values. Following the removal of the Na/SO_4 ratio, the 1964 polynya opening is now more clearly resolved in the updated index as a synchronized peak across the remaining four proxies. This revision improves the

> consistency of the reconstruction with historical Nimbus I satellite observations and provides a more robust validation of the index during the instrumental era.

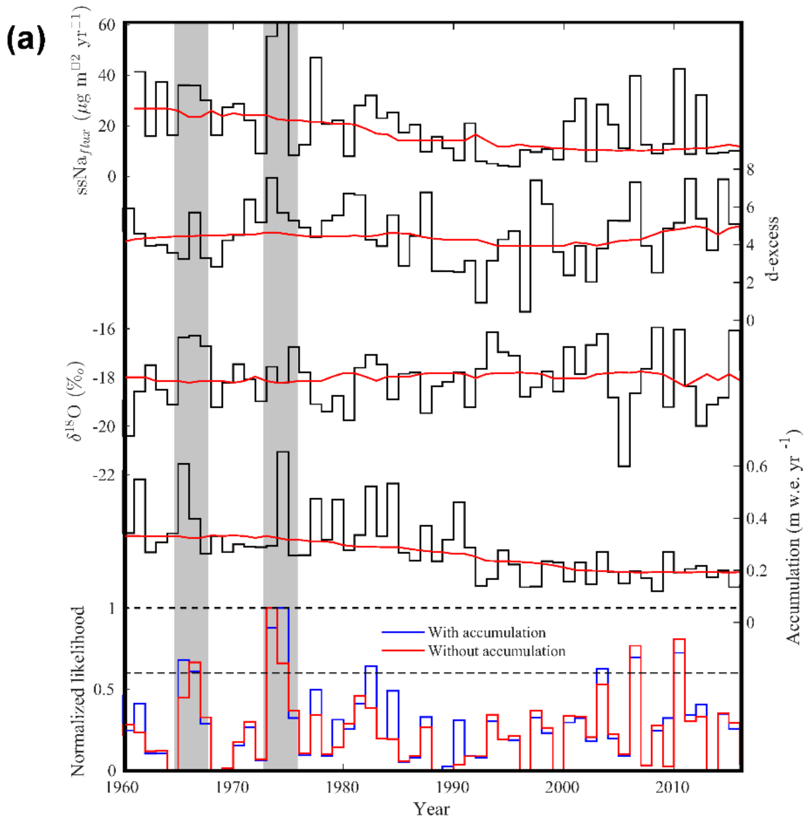
The second issue may just be presentational. How are the 5 components combined? The paper never explains this. Is each component normalised and the 5 components simply added together? This would be the simplest but assumes that all 5 components are equally good indicators of a polynya. An alternative would be to test different weights for a best fit to the satellite data. In any case this needs to be described and explained.

> We have now described the method in more detail in the main text. We also revised the calculation methods to use rolling z-scores rather than the native proxy values for index calculation, which improved the representation of the 1964 polynya opening in the indices. We have also decided to simplify the index calculation by removing the Monte Carlo simulations of uncertainty, as the proxy uncertainties are relatively insignificant to the final polynya index calculation. We use equal weights for all four proxies because we do not have sufficient control points to effectively quantify variable weights for each proxy. The revised text in the main manuscript is as follows (Line no. 387 – 407):
>
>> *To reconstruct past MRP activity, we developed a multi-proxy index using a cumulative-anomaly approach, calculated as the means of rolling z-scores. This allows us to isolate anomalies that are synchronized across proxies from long-term climate trends. We use four independent proxies: sea-salt sodium (ssNa) flux, δ18O, deuterium excess (d-excess), and annual snow accumulation rates for our polynya index calculation. Prior to index calculation, all proxies are scaled to 0 – 1 to remove bias toward proxies with greater variability. For each proxy, we first calculate a 30-year rolling z-score to identify annual departures from the local background state. This accounts for non-stationary signals in the 250-year record, such as the gradual depletion of water isotopes or recent decreases in snow accumulation rates. As explained in section 3.3.1, MRP openings are linked to positive anomalies in ssNa flux, δ18O, d-excess, and annual snow accumulation rates; therefore, we used only positive z-scores for the calculation. Negative z-scores were set to zero to prevent them from cancelling out physically significant peaks in other proxies. To test the dependence of the polynya index on snow accumulation, we calculate two versions of the index: one using all four selected proxies and another excluding annual snow accumulation rates. The polynya index is a mean of all individual rolling z-scores, which is then normalized so that the final values are such that the largest known polynya opening of 1974 – 76 is set to 1. A polynya year (a year when a polynya activity*

is recorded) is defined as a year in which the polynya index exceeds the likelihood threshold (0.6, 0.8, or 1). The polynya index is, therefore, an indicator of polynya occurrence from ice core observations and not a measure of the absolute extent or duration of the polynya events.

A final issue with the polynya index is hard to discuss because we are never shown a zoomed version of it for the satellite era (Fig 7b is for the longer period). However to my eye the index shows a good peak for 1974, but does not (despite what the text (line 380) and Fig 7a would suggest) give a peak reaching even the 0.6 threshold for the other known event in 1964. The authors need to confront this and make a better quantitative assessment of how well their composite index works over the satellite era before they discuss its extension to 1800.

We have now included a snapshot of the revised polynya index, focusing on the recent decades (1960 – present), for evaluating the index's performance. This is included as an extra panel in Figure 7a. The recalculated polynya index now clearly shows the 1964 event with a likelihood >0.6. This is also highlighted in the revised figure.



I have a number of presentational and discussion points on the manuscript, but the issues above are the ones that need addressing before the other areas are considered. Because the issue with

Na/SO4 may require a recalculation of the index, and the issue about 1964 may require a reduction in confidence about previous polynyas, I class this as major revision.

> We appreciate the reviewer's critical assessment regarding the robustness of the polynya index. We acknowledge that the Na/SO4 ratio and the representation of the 1964 event are central to the reliability of our reconstruction. In response to these concerns, we have revised our methodology and recalculated the index to ensure it is grounded in physically justifiable mechanisms. We believe these changes address the reviewer's concern regarding the confidence of our historical record. The fact that the 1964 event is now captured further strengthens the index's ability to detect shorter-duration or less intense events, reinforcing the justification for the polynya events identified in the earlier parts of the indices. The updated methodology, including a more detailed description of the cumulative-anomaly approach and the updated polynya activity history, is now provided in the revised manuscript. We believe these revisions provide the quantitative and mechanical rigor requested.

Detailed comments:

Line 45 and Figure 1. Please add to the caption a description of the polynya itself, which I assume is the grey area (but this is not stated). I expected this figure to show me sea ice concentration (or some similar index): please explain better what the MODIS image is showing.

> We have now updated the Figure 1 caption to better explain what the polynya looks like in the image and why we use the MODIS image. The updated figure caption reads as follows (Line no. 605 – 617):
>
> > ***Figure 1 | Study area and wind-trajectory patterns.*** *(a) Maud Rise Polynya during its largest recent opening on 25 September 2017, detected with Moderate Resolution Imaging Spectroradiometer (MODIS) on NASA's Terra satellite (from NASA Worldview, https://worldview.earthdata.nasa.gov/, last access: 15 September 2024). The polynya is visible as the dark, open-water feature amidst the surrounding white sea ice (b) MODIS Satellite imagery taken on the same day in a non-polynya year, 25 September 2022. The MODIS corrected reflectance imagery provides a true-color-like representation where the open ocean appears dark gray or black, while sea ice and clouds appear white or light gray. Images are overlaid with the endpoint frequency of back-trajectories ending at the Djupranen ice core site (green star) from June to October in 2017 and 2022, respectively (see methods). The Indian Maitri Station is marked by a yellow square. The*

> *colourmap is on a logarithmic scale. The inset shows the location of the main map. Grounding line and calving front are marked (Matsuoka et al., 2015).*

Line 75: this doesn't quite make sense. I assume you mean "they found that snowfall increased during the 2016-17 MRP opening and assumed that this had also occurred in past MRP opening periods"

> We agree that the original phrasing was unclear. We have revised the sentence to clarify that Goosse et al. (2021) observed the increased snowfall during the recent event and used that relationship as a basis for their historical reconstruction. The revised sentence reads as follows (Line no. 76 – 78):
>
> > *In these records, they observed increased snowfall during the 2016-17 MRP opening and assumed that similar anomalies occurred during the past MRP opening periods.*

Line 96. Section X. (2.5?)

> The placeholder "Section x" has been replaced with the correct reference to Section 2.5 (Line no. 99)

Line 101 "an ideal ice core site" typo.

> The typo "idea core site" has been corrected to "ideal ice core site" (Line no. 104)

Line 139-40 Mg2+, Ca2+, the "2" should be a superscript not a subscript!

> Corrected. All chemical ion notations have been updated to proper superscript formatting

Line 180. There are 2 Dey 2023 in the reference list. I think you mean the paper not the thesis. Use 2023 a and b?

> Yes, we refer to the paper, "Dey et al., 2023" here. The thesis is cited as "Dey, 2023" in the text, while the paper is cited as "Dey et al., 2023" following the Journal's referencing guidelines.

Fig 2. I assume the y-axis unit should be ug m-2 yr-1?

> Correct. The y-axis unit for $nssSO_4$ flux has been updated to μg m^{-2} yr^{-1} to reflect annual accumulation rates.

Line 211. It would be useful to show this box (lat and long range) on Fig 1. I think you do show it on Fig 5 (but please say so in the caption).

> We have added a black bounding box to Figure 1a to represent the "polynya-prone" study region. Figure 5 caption is also updated as per the reviewer's suggestion.

Line 218. Please explain this metric more clearly. Do you mean it is the count of days when any pixel in the box has sea ice less than 60% or do you mean that the average across the area is less than 60%. Only the former really seems possible but it needs to be said, also how big is each pixel (ie how large an area needs to have low sea ice)?

> The "Polynya Days" metric counts the number of days when the sea ice concentration minimum (any pixel within the polynya-prone region) falls below the 60% threshold. We have also clarified in the text that we use pixel-level data (approx. 25 x 25 km) and not average values. The paragraph is now restructured and reads as follows (Line no. 227 – 240):
>
>> *To quantify the polynya metrics, we use sea ice concentration data derived from passive microwave measurements using the NASA Team algorithm (Comiso and Nishio, 2008), processed at a spatial resolution of 25 x 25 km per pixel. The first metric, Polynya Days, represents the annual count of days (between June 1 and October 31), with a minimum sea ice concentration in the polynya-prone region below 60%. The second metric, Cumulative Polynya Area, measures the total extent of all polynya occurrences within a year (June 1 to October 31). This was calculated by summing the daily open water areas within the polynya prone region, defined as the cumulative sum of pixel area with sea ice concentration below the 60% threshold. The combination of these metrics allows us to characterise both the temporal persistence and spatial extent of polynyas, providing insights into their formation mechanisms and potential impact on regional oceanographic processes. The combination of these metrics allows us to characterise both the temporal persistence and spatial extent of polynyas, providing insights into their formation mechanisms and potential impact on regional oceanographic processes.*

Fig 4. If the cumulative polynya area is, as per text, a sum of daily polynya areas, then the right hand y axis should have units of km^2*days.

> We have updated the axis label to "km^2 days" to accurately reflect the cumulative daily summation used in the calculation

Line 256 and Fig 4. Can you give an estimate of what the 1974 event would show on this figure. I read 300,000 km2 persisting for a long period so I assume it would be way off scale on that red axis. I think the reader needs to have this explained.

> Thank you for the suggestion. The polynya did reach a maximum extent of 300,000 km^2, but this size was not consistent for the whole period of the polynya opening. To get an accurate extent of the polynya, we will have to analyse the satellite records from the older Nimbus satellite, which we believe it is beyond the scope of the paper and not something that is the main focus of the paper. However, we agree with the reviewer that the reader would appreciate a perspective of how massive the 1974 – 1976 polynya, therefore we add a representation of what only one day (during the maximum extent) of the 1974 – 1976 polynya would look like compared to the cumulative polynya area observed between 1979 – 2016. The updated figure and caption are shown below: (Line no. 630 – 635)

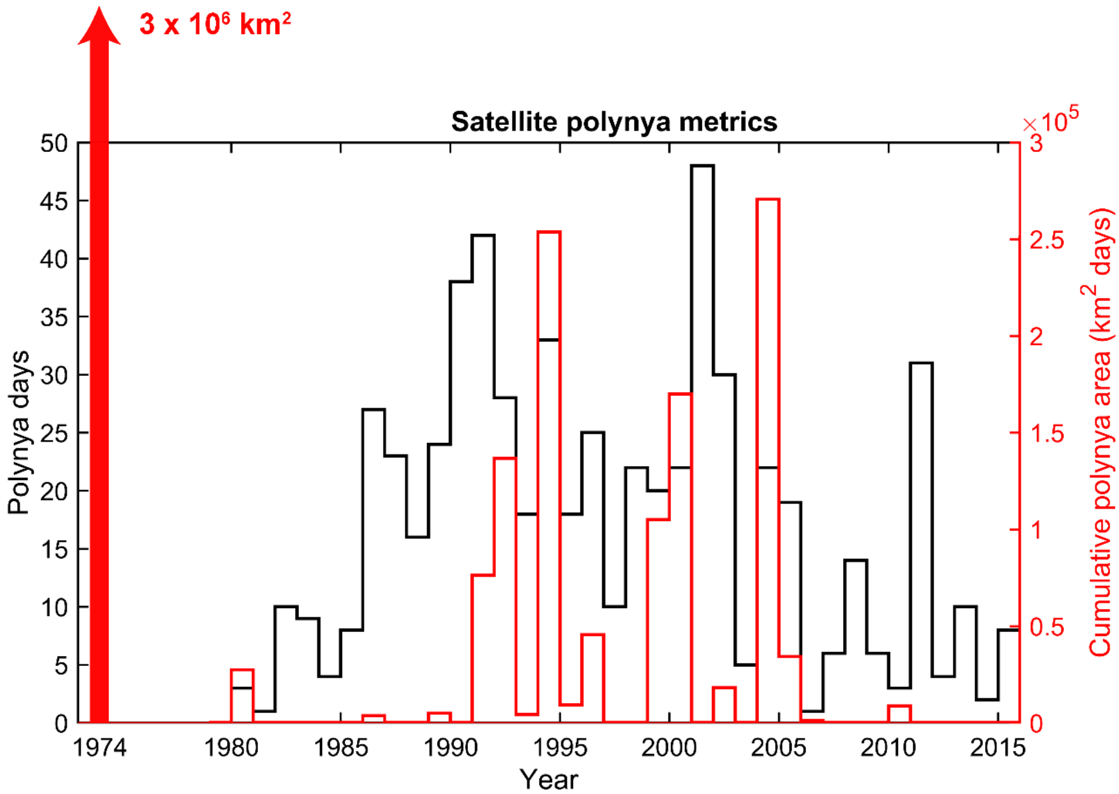


Figure 4 | Satellite polynya metrics. *Two different annual metrics of satellite-derived polynya occurrence. Since the polynya is highly dynamic temporally and spatially, the*

> *metrics do not show a one-to-one resemblance. The red vertical bar shows the 1974 – 76 polynya event, which reached a peak extent of approximately 300,000 km^2 (Carsey 1980), more than ten times of the maximum cumulative polynya area observed between 1979 – 2016.*

Fig 5. You say you used 10 day trajectories and that these are endpoint frequencies. Is that right and does it make sense. Why would the position after 10 days be uniquely interesting; and is it really possible that many airmasses are still in the starting box after 10 days? Perhaps these are actually the positions throughout the 10 days but that is not what the caption says.

> The reviewer is correct that these are not endpoints but rather all positions throughout the trajectory paths. This has been clarified in the main text and figure captions. We also appreciate the reviewer's question regarding the rationale for using 10-day trajectories and focusing on their endpoints. We chose a 10-day duration because it represents the typical atmospheric lifespan and transport timescale for synoptic-scale weather systems in the Southern Ocean that deliver moisture and aerosols to the Antarctic coast. Additionally, air parcels do not merely sample their initial starting points; rather, they continuously accumulate environmental signatures, such as sea-salt aerosols and isotopic moisture signals, along their entire transit pathway.
>
> As shown in Figure 6, the transport patterns remain remarkably consistent between known polynya (2017) and non-polynya (2022) years, confirming that the Djupranen site is geographically positioned to receive air masses from the Maud Rise region regardless of the specific sea-ice state at the time and the changes observed in the ice core records are due to changes in the marine conditions (like polynya opening) and not transport changes.

Line 279. Please be more precise. I think you mean that 2% of trajectories from the MRP arrive in the 1x1 degree box surrounding the site.

> The statement has now been revised to (Line no. 290 – 293):
>
> > *Frequency analysis indicates that our study site (1 x 1 degree box surrounding the ice core site) receives approximately 2% of all trajectories originating from the MRP region, a statistically significant proportion ($p < 0.001$).*

Figs 5 and 6. But I am not sure the above is very relevant. Fig 6 seems much more useful, asking what proportion of trajectories at the site come from the polynya. But you never actually state this number: what proportion of airmasses at your site came from the 48 1x1 degree boxes you identify as potential polynya sites? Please provide this number if possible.

> We have chosen to retain Figure 5 as it provides critical spatial context that distinguishes our study from previous reconstruction of Goosse et al., 2021. Figure 5 shows that atmospheric trajectories originating from the polynya-prone region consistently reach our ice core site, whereas they hardly reach the ice core sites used by Goosse et al., 2021. This spatial mismatch in other cores could possibly explain why the 1964 events may be underrepresented in their reconstruction, highlighting the unique location of our ice core site.
>
> Regarding the reviewer's request for specific proportion of air masses coming from the polynya prone regions, we do mention it in lines 283 – 286 as follows (Line no. 290 – 293):
>
>> *Frequency analysis shows that our study site (1 x 1 degree box surrounding the ice core site) receives approximately 2% of all trajectories originating from the MRP region, a statistically significant proportion ($p < 0.001$) that indicates reliable capture of polynya-related atmospheric signals.*

Fig 6. Please discuss whether trajectories are also coming from other open water areas? It would be useful to draw on an average summer and winter sea ice edge to Fig 6 so we can judge whether there are also trajectories from other open areas. This would then need a discussion in text.

> We have updated Figure 6 to include the average summer and winter sea-ice edges as suggested. This visual addition clarifies the extent of open-water exposure for the trajectories. While air masses do pass over other open-water regions, we have now added a discussion in Section 3.2 explaining why these are unlikely to be the source of the observed proxy anomalies. Specifically, these regions are far more distant, and their contribution is relatively stable year-to-year. In contrast, the Maud Rise polynya is a proximal source that opens in the middle of the winter ice pack, creating a unique and anomalous source for marine aerosol and moisture transport directly to our site. The added text in the main manuscript is as follows (Line no. 303 – 309):
>
>> *We observe that air masses reaching our site also originate from more distant open-water areas (even during the winter months) in the Southern Ocean. However, these distal sources remain relatively consistent across the study period and are located significantly further from the IND-36/9 site compared to the polynya-prone region. Consequently, these background marine inputs are less likely to drive the synchronized, high-magnitude anomalies observed across all proxies, particularly the sharp increases in ssNa flux, that characterize the polynya years.*

Line 299 and following lines. Given 10% uncertainty on IC data, giving ranges to 2 decimal places is way too precise. Please reduce the precision through this para.

> The precision has been reduced as suggested

Fig 7a. I assume the red lines are smoothings, but on what timescale?

> The red line is the moving median over 30-years window. This has been added to the figure caption now.

Fig 7 and section 3.3.1. We need to see the component data that go into the index across the entire record not just the instrumental period. Perhaps an extra figure in the supplement. Most useful would be the component values with a panel also for the composite index (as per 7b) so the reader can assess which components are contributing to the overall index at peaks.

> We have now added a new figure (Fig S2) in Supplement showing the individual proxies and the polynya indices for the complete ice core record.

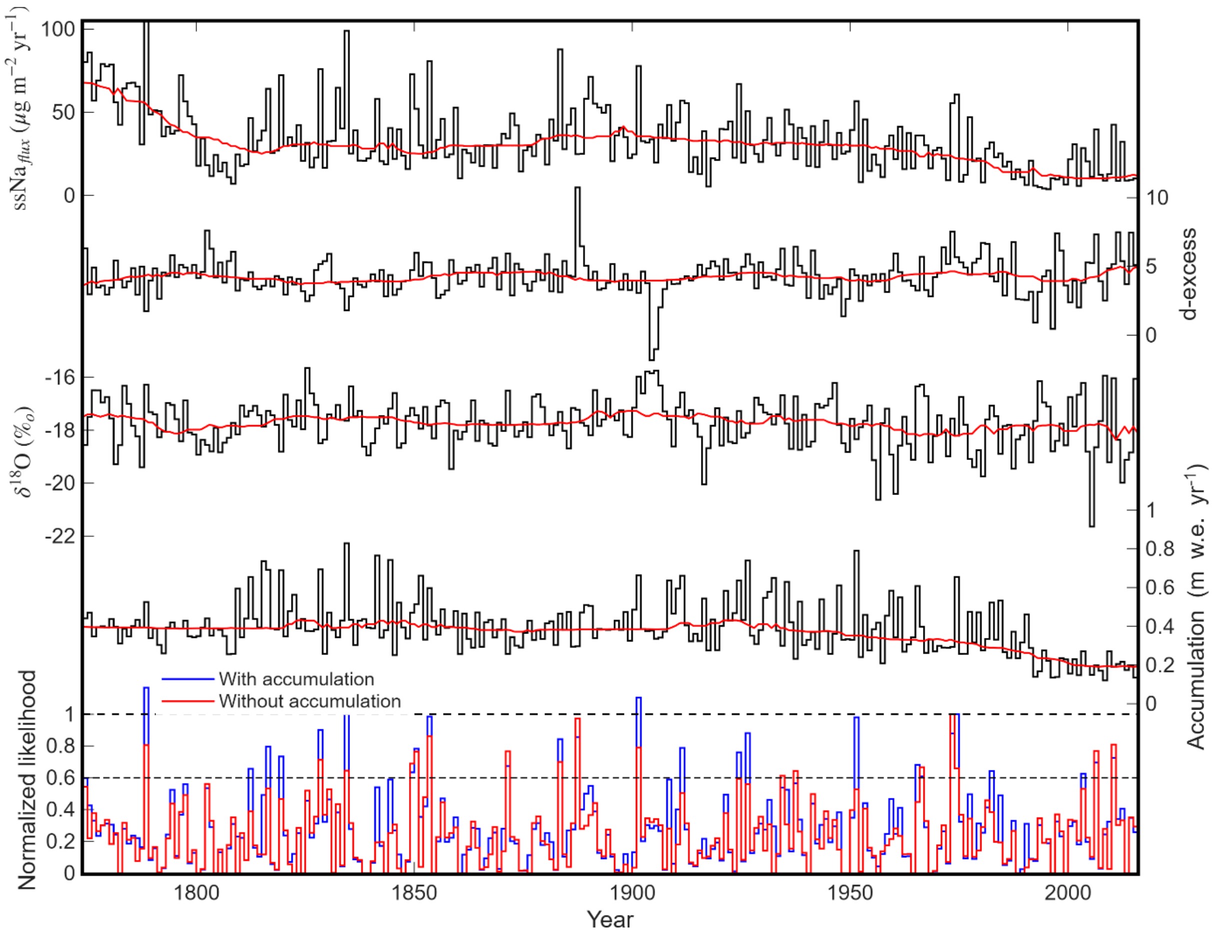


Figure S2 | Ice core proxies and polynya identification. *Annual variability of selected ice core proxies and polynya index from 1774 – 2016. The red curve is the moving median over a 30-year window.*

Line 323. While frost flowers undoubtedly lead to sea salt with reduced sulfate, the leading contender for sea salt from sea ice in recent years is the sublimation of blowing snow (eg Frey, M. M., et al., First direct observation of sea salt aerosol production from blowing snow above sea ice, Atmos. Chem. Phys., 20, 2549-2578, doi: 10.5194/acp-20-2549-2020, 2020 and refs therein). However see also my major comment on the sulfate signal in this core above.

> We have now removed Na/SO_4 from the polynya index calculation. The sea salt source has also been revised in the main text (Line no. 338 – 355).

> *While sea ice typically acts as a physical barrier to air-sea exchange, a polynya creates a localized window for enhanced aerosol production via multiple distinct mechanisms. First, the open-water surface provides a direct source where high-speed cyclonic winds and storm activity trigger bubble-bursting processes, injecting sea-salt particles into the*

> *marine boundary layer. Second, extensive open water polynyas can also act as a large factory for sea ice production, comparable to the production of the largest coastal polynyas (Zhou et al., 2023). Previous studies have shown that the surface of fresh sea ice, including frost flowers, is an important source of sea salt to the Antarctic, and the production of frost flowers is controlled by the amount of new sea ice production (Wolff et al., 2003). Furthermore, blowing snow across these newly formed, brine-wetted ice surfaces facilitates the sublimation of salty snow particles, which is also the major source of sea salt aerosol to the inland sites (Frey et al., 2020). Because these processes occur proximately to the drilling site and are situated directly along the atmospheric trajectories identified in Figure 6, they result in high-magnitude pulses of ssNa that are chemically distinct from the distal, year-round open-ocean baseline.*

Fig 7b and section 4.1. I again emphasise that 1964 appears not to show up in the index which is surprising given what is seen in Fig 7a. Please discuss.

> As mentioned earlier, the 1964 event is now clearly visible in the revised polynya index (likelihood > 0.6) and highlighted in the figures.

Line 390. The comparison with Goosse seems vague and not quantitative. Why not plot the annual peak in your and Goosse's index as an x-y plot with statistics for how well they agree.

> We acknowledge the reviewer's request for a quantitative comparison between our index and that of Goosse et al. (2021). However, a direct annual x-y plot is statistically problematic due to the independent age-scale uncertainties (typically ± 1–2 years) present in both records. Therefore, we calculated a presence absence matrix for polynya at multiple thresholds and assuming a lead/lag of ±2 years. The outcome is added in the revised manuscript and is as follow (Line no. 439 – 460):
>
> > *We compared our polynya index with those from Goosse et al. (2021) and found that the major polynyas identified in our study over the common time period are mostly identified in their reconstruction. A direct annual correlation is however statistically limited by the independent age-scale uncertainties, typically ± 1–2 years, inherent in all ice core records. These chronological offsets cause high-magnitude events to appear out of phase between two indices/studies, which artificially degrades linear correlation coefficients despite clear physical synchronization in the raw signals. To provide a more robust and physically meaningful assessment, we used a binary presence-absence matrix assuming*

a ±2 years lead/lag window at threshold levels of 0.6, 0.7, 0.8, 0.9 and 1. Therefore, we look for polynya events at different threshold levels in our record and then look for polynya events with a threshold of 0.6 from the polynya index from Goosse et al. (2021) within a ±2 years window. For simplicity, we focus on only one index from each study, simple average of the standardized time series (Stat) from Goosse et al. (2021) and polynya index including accumulation from this study. We find that nine polynya years (threshold = 0.6) in our index that are also corroborated by the Goosse et al. (2021) record (Figure 9a). Similarly, seven polynya years are corroborated at a threshold of 0.7 and three at 0.8. At the threshold of 0.9 and 1, only the polynya event of 1974 is corroborated, which is expected as both studies use this event as an assumed truth and normalize all event to it. Similarly, when we repeat the same process for polynyas identified in Goosse et al. (2021) (Figure 9b), we find that 17 polynya years are corroborated at 0.6 threshold, 12 polynya years at 0.7, 9 polynya years at 0.8, eight at 0.9 and seven polynya years with a threshold of 1 are also identified in our polynya index.

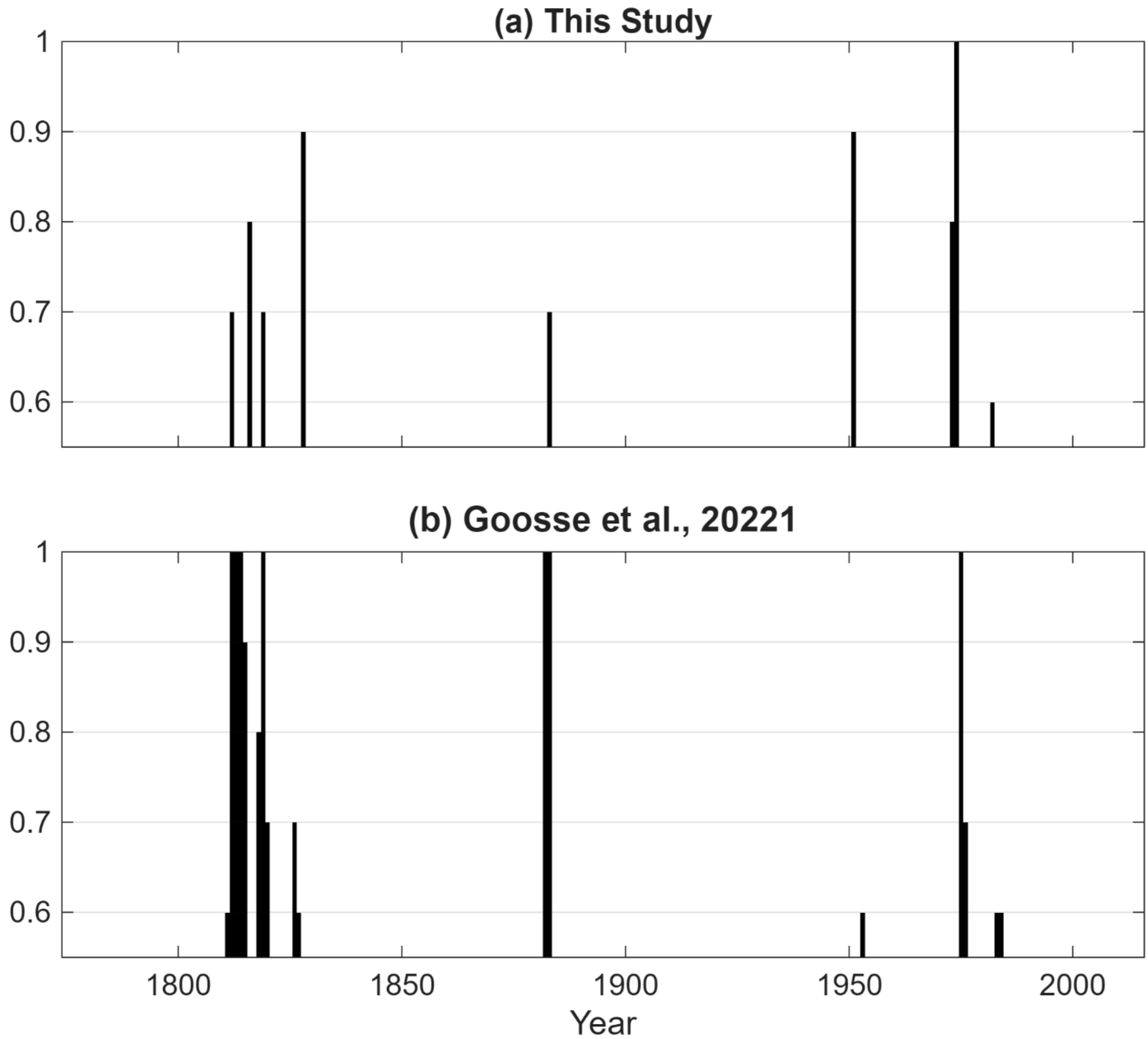


Figure 9 | Comparison of synchronous polynya events. *Binary presence-absence analysis comparing the reconstructed polynya index from this study and the index from Goosse et al. (2021) across five threshold levels (0.6, 0.7, 0.8, 0.9, and 1.0) and assuming a lead/lag of ±2 years. (a) Temporal distribution of polynya events identified at different threshold in this study and corroborated by the Goosse et al. (2021) record (threshold ≥ 0.6). (b) Distribution of polynya events from Goosse et al. (2021) at different threshold corroborated by this study (threshold ≥ 0.6).*

Line 420. I am very nervous about taking seriously an ocean reanalysis for a time and region where there is essentially no data; in particular if a polynya was present it would surely strongly change the reanalysis (earlier it's argued that a polynya drives intense convection and changes in circulation) so this discussion seems a bit pointless. I also don't really see what you claim in lines 431-433 – where the grey bars are seems to be nothing unusual in most of the parameters you plot. I would downplay this part of the paper (lines 424-446) which seems very speculative.

> We acknowledge that ocean reanalysis in the pre-satellite era contains higher uncertainty due to sparse data. Following the reviewer's advice, we have restructured Section 4: Discussion (Line no. 437 – 550) and removed the comparison with SODA products and previous Figure 9.