

Response to comments from Anonymous Referee on ‘Ocean control on sea ice in the Nordic Seas’

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Dear Reviewer,

We sincerely appreciate your thoughtful review comments and suggestions. Below, we have provided our responses, with your comments highlighted in blue and our replies in black. All line numbers refer to the resubmitted manuscript with track changes.

In this manuscript, Wong and colleagues describe the results of a study of to reconstruct sea-ice extend and subsurface ocean temperatures for a past interval 40ka – 33ka using proxy data from a deep-sea sediment core taken from the eastern side of the Fram Strait. They find variability in both indices during and between regional stadial and interstadial climate intervals that compare with independent evidence for variations in the large-scale ocean circulation, and infer a causal relationship between the two. Modern observations increasingly implicate ocean heat transport in the melting of ice shelves and sea ice, and this study is a useful contribution to the exploration of the connection of ocean and cryosphere during known rapid climate changes in the past. It should be considered for publication after relatively minor revisions that include attention to the following points.

Perhaps the biggest question about this valuable contribution is how useful the summary schematic might be.

Given the positive feedback from the second reviewer regarding the schematic figures 5 and 6, we have decided to keep them.

Identifying five separate modes from a study of exactly five total stadial and interstadial intervals seems a bit much, especially if the goal is to understand any consistent behavior that can provide insights into the general workings of the system.

We recognize that our original presentation may have led to the misunderstanding that we propose one distinct mode for each Greenland Stadial (GS) or Interstadial (GI). This is not the case. Modes II and III are found under both GS and GI conditions, suggesting that the underlying physical processes are not constrained by the prevailing climate state. The reviewer is correct that three of the modes (I, II, and IV) are only found once, likely due to limited length of the investigated time period (40–33.5 ka b2k). We have updated Figure 4 to more clearly indicate when each mode is observed. Additionally, we have restructured the discussion section, by merging the original

Subsections 5.1 and 5.2, to introduce the five identified modes directly (now pages 14–28). This revised structure also specifies the time periods during which each mode is found.

The big picture here seems to be the important role of heat transport in the sea ice distribution, which is an important finding, as is the placement of the new results in the context of previous comparable studies. Defining modes from the limited observations seems under-constrained. Is this a fortuitous study interval, or might there be more than fifty separate modes one for every Greenland stadial and interstadial? At the very least, the rationale for identifying a new mode for each stadial and interstadial interval in this particular study should be elaborated upon and strengthened.

Thank you for the helpful comment. The broader significance of our study lies in highlighting the role of heat distribution in shaping sea ice conditions in the Nordic Seas. Regarding the definition of modes, although the time interval we investigated (40–33.5 ka b2k) is well-studied in the Nordic Seas, this is not the reason we identify five different ‘ocean circulation-sea ice’ modes. These modes challenge the conventional framework of D-O oscillations and emphasize the role of ocean circulation in redistributing heat. To better clarify this and present our discovery, we revised Figure 4 and restructured the discussion section by merging the original Subsections 5.1 and 5.2.

Given the uncertainties and necessarily erratic resolution of the subsurface temperature data, the main finding would seem to involve the new sea ice reconstruction, which contrasts in important ways with the three existing datasets that the authors nicely use to construct a meridional transect through the Norwegian Sea to the Fram Strait. The previously published northernmost site did not experience any substantial changes in sea ice cover throughout the study interval, whereas the two previously published southernmost sites varied systematically, with more stadial sea ice cover and less interstadial sea ice cover. That uniformly variable spatial gradient is largely interrupted by the new results, which include greater sea ice cover in both interstadial intervals and less sea ice cover during two stadial intervals, with only GS7 characterized by greater sea ice coverage at all sites. The contrast with GS7 might be explained by a frequent or persistent polynya at the new study site, which itself would require some explanation rather than the mere mention in passing, or by some other combination of influences that might or might not relate to the five proposed modes. Since that connection is not as clear as it might be, some effort to bolster the interpretation would appear warranted.

To clarify the difference between GS-7 and the other two GSs, we added two statements:

1. ‘Mode II can thus be interpreted as the most representative of expected GS conditions in the eastern Nordic Seas among all five modes, consistent with previous studies (Dokken et al., 2013; Sadatzki et al., 2019, 2020; Sessford et al., 2019),’ in Subsection 5.2, where we presented and discussed Mode II (lines 355–357).
2. ‘Furthermore, polynya activity at MD99-2304 is absent during GS-7, which instead experienced Mode II, characterized by a seasonal sea ice cover in the eastern Nordic Seas under an AMOC of intermediate strength.’ at the end of Subsection 5.3 (lines 400–402), where we discuss polynya activity observed during HS-4 and GS-8 (as Mode III).

Figures 2, 3 and 4 would be more easily assessed by the casual or careful reader if each dataset had error bars, either on individual points or as a scale bar on the panel for each data type.

1. To clarify the negligible uncertainty in biomarker analysis, we have added an explanation to the methodology section (lines 156–160). In addition, we have added the phrase ‘to evaluate instrument stability and analytical accuracy’ following the introduction of internal standards (7-HND, 9-OHD, androstanol, and squalane) (lines 140–141) to further clarify our approach. Given this low analytical uncertainty, we have chosen not to include error bars for the biomarker data in Figures 2–4.
2. The analytical uncertainty in $\text{Mg}/\text{Ca}_{\text{N,p}}$ is negligible. We have added explanations on this in the methodology section (lines 179–185). Regarding the uncertainty in Mg/Ca -based SubSTs, it is calculated based on the original error range provided by Elderfield and Ganssen (2000), as reflected in the revised equation in line 187. SubSTs from MD99-2304 yield an average value of 0.32. We have updated Figures 2, 3 (now lines 220 and 246), and S1 (please see the updated supplementary) to show this error range. Since Figure 4 presents a comparison of records from the eastern Nordic Seas transect, we chose not to include the error range in this figure.

Figure 4 – The caption should specify the extent of smoothing for each dataset.

For the extent of smoothing applied to each dataset in Figure 4, we specified in the original caption: “All smoothed lines are derived using a 3-point moving average.” (lines 273–274).

Axis labels for panels A and B are relatively straightforward, although the meaning of the sea ice extent (% coverage?) might be included.

The sea ice extent is based on PIP_{25} indices, which are semi-quantitative and do not represent actual coverage percentages. As shown in Figures 2 and 3 (now lines 220 and 246), higher PIP_{25} values indicate more sea ice. We have now added “ PIP_{25} -based” to both the y-axis label of panel (A) and the figure caption. Additionally, we included a sentence in the caption to clarify the y-axis values in panel (A): ‘ PIP_{25} indices relate to different sea ice conditions (0.75–1 extensive sea ice, 0.5–0.75 seasonal ice / stable ice edge, 0.1–0.5 little but variable ice extent, 0–0.1 ice-free) (Xiao et al., 2015; Stein et al., 2017).’

The axis label for panel C is not easily consistent with the axis numbers, which must reflect the proxy rather than estimates for AMOC. Arrows or labels for “strong” and “weak” might make the figure more easily accessible, as is the case in Fig. 5. Since the core name was introduced on the map figure 1, it might also be included along with the other cores here.

We added the sentence ‘Lower Pa/Th ratios indicate a stronger AMOC (e.g., Bradtmiller et al., 2014; Robinson et al., 2019; Missiaen et al., 2020)’ to the caption to clarify how the AMOC states are defined, along with the core name (CDH19) and the proxy (Pa/Th) used for AMOC strength reconstruction, to the y-axis in panel (C) in Figure 4. We did not include the definitions in the figure itself, as doing so would make it too cluttered.

Figure 6 and to a lesser extent Figure 5 are well-drafted and relatively clear to understand, although as noted above they describe scenarios that are defined as “modes” of ocean-ice-climate behavior, with relatively limited justification for the necessity and validity of doing so.

We appreciate that the reviewer find that these figures are clearly presented. Regarding the comment about the justification of the defines modes, please see detailed responses to the first two comments on the same issue.

Please find our replies to minor points below.

Originally lines 29–30: Does this refer specifically to perennial sea ice?

Now line 32: To clarify that this refers to variability in SIE across the Nordic Seas, we have rephrased to ‘consistent shifts between an extensive sea ice cover during GSs and seasonal sea ice cover during GIs’ from ‘consistent phase shifts during the D-O events, with an extensive sea ice cover during GSs and seasonal sea ice cover during GIs’.

Originally lines 165–171: This comparison is a very useful step, and the authors are to be congratulated. Their conclusions about the various methods might be expanded in the discussion section rather than relatively buried in the methods. Readers are likely to be interested in the authors’ thoughts on the reason for the observed differences and the methods’ respective utility for reconstructing low temperatures. It might be helpful to the percentage *N. pachyderma* data in the supplement.

Now lines 195–200: We have corrected this statement. The reference to Morley et al. (2024) was previously inaccurate and has now been revised. We have now included information regarding the choice of calibration in the methodology section. A detailed discussion on SubST calibration method is beyond the scope of this work, and we believe including it in the discussion would detract from the main message. Such a discussion would be better suited for a separate manuscript. Therefore, we have kept this information in the methodology section, where it serves as background on our methodological choices. Furthermore, the relative abundance of *N. pachyderma* (sinistral) record has been added to the figure in supplementary (Fig. S1).

Originally lines 179–180: Is there an analytically statistically significant difference between these values? See note about plotting error bars.

Now lines 220 and 246: As noted in the previous response, the uncertainty in the biomarker records is negligible. The uncertainty range of SubSTs ($\pm 0.32^{\circ}\text{C}$) from MD99-2304 has been included in Figures 2, 3, and S1.

Originally line 450: “were driving” might be “drove”.

Now lines 579–583: We agree with the change. However, after merging the original Subsection 5.3 with the conclusion, we have rewritten the sentence to ‘Our records support previous studies suggesting that atmospheric changes over Greenland were driven by changes in sea ice conditions in the southeastern Nordic Seas and the North Atlantic.’

Originally line 455–456: This is an important observation and conclusion of this study, although the sentence might be rearranged so that it reads as making the point about the new data, rather than the previously existing observations.

Now lines 579–583: We revised and expanded the conclusion section by merging the original conclusion with Subsection 5.3 (‘Implications of the eastern Fram Strait findings’). The sentence now reads: ‘Our records support previous studies suggesting that atmospheric changes over Greenland were driven by changes in sea ice conditions in the southeastern Nordic Seas and the North Atlantic.’

Response to comments from Niccolò Maffezzoli on ‘Ocean control on sea ice in the Nordic Seas’

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We thank Niccolò Maffezzoli for your detailed review comments and suggestions. Below, the reviewer’s comments are shown in blue, while our responses appear in black, directly beneath each comment. The same format applies to both his general and specific comments. All line numbers refer to the resubmitted manuscript with track changes.

Wong et al present new measurements from the MD99-2304 core, eastern Fram Strait, between 40 and 33.5 ka b2k. The measurements include proxies for subsurface temperature and sea ice extent. The records are presented by comparing with other marine records from the Fram Strait and the Nordic Seas. The authors explain the differences between sea ice records in relation to an AMOC record as the primary driver. They find significant differences between the eastern Fram Strait and the Nordic Seas, a great variability of the signal compared to both the Nordic Seas and one other record in the Fram Strait. The motivations behind the presented modes seem meaningful to me, although presented from an observational point of view, see point below. The effort of the authors to combine all the available records and have a picture as wide as possible is commendable. I have one main point and a few minor ones. I am confident that all can be dealt with before publication. Overall, I find the study a very valuable contribution.

My main concern resides in the 5 modes. How and why should they be really needed, other than a way to describe the data? Do they have a profound significance over this time interval? In other words, can they be used and applied to other GS-GI or is their validity limited to this time interval? I feel like they are a bit an overfit. I would not necessarily remove them, but I would make clear to the reader what the authors think about their role and general validity.

To some extent, yes—the modes can be seen as a way of describing the data. More importantly, however, they serve to characterize the observed interplay among AMOC strength, subsurface temperatures, and sea ice extent in the eastern Nordic Seas. This interaction appears to be more variable than previously thought and less dependent on the background climate state (GS or GI).

Some of the modes (i.e., Modes III and V) occur more than once within the investigated period. Furthermore, Modes II and III are found under different climatic states (both GS and GI). When

introducing the modes, we now clarify that they represent a way to describe the observed dynamic interplay (lines 295–296): ‘These modes reflect a dynamic interplay among AMOC strength, SubSTs, and SIE in the eastern Nordic Seas.’

We do not have comparable data from more D-O events. Based on the available data, we cannot determine whether these modes are applicable to other GSs and GIs. Therefore, we have also added a statement regarding their potential general validity (lines 296–299): ‘Given that Modes III and V recurred within the limited period investigated, and Modes II and III spanned both GS and GI (Fig. 4), we consider it likely that these modes represent inherent physical processes of the Earth system. However, due to the constraints of the available records, their broader applicability remains uncertain.’

Nonetheless, since some modes recurred even within this short time interval, it is plausible that others might also reappear if the record were extended. To further investigate the validity of the underlying physical processes discussed in the manuscript and their relationship to prevailing climate variability, we are currently analyzing model simulations that reproduce DO-like variability under different boundary conditions.

Structure: I would merge 5.2.1 into 5.1.1 (and similarly the others). When reading 5.2.1 I had to go back and forth between these two sections and Figs 4-5, making the reading quite challenging.

As suggested, we have merged Subsections 5.1 and 5.2 to improve readability (now pages 14–28). We also restructured the content within each new subsection to reduce the need for back-and-forth between sections and figures. Please see the updated manuscript.

Mode III with more sea ice in the Nordic Seas than the Fram Strait is primarily explained by a reduced AMOC signal for the former, and increased vertical ocean mixing and consequent increased sea ice loss in the latter (and polynyas). HH15-1252PC does not show such a strong signal of sea ice reduction though - any comment on that? There is a sea ice gap in Fig. 6 between the two cores. Can we add a tentative explanation for that? I find in general that HH15-1252PC is rarely mentioned. Why is its signal so different to that of MD99-2304?

Regarding the comparison with records from HH15-1252PC within the eastern Fram Strait, we argue that the strong variability in sea ice extent (SIE) observed from MD99-2304 is caused by topographical features at MD99-2304, as shown in Figure 1. Therefore, we added remarks explaining the difference between sites HH15-1252PC and MD99-2304 in both Subsection 5.3 of the discussion on polynya activity (lines 392–393) and the conclusion section (lines 584–586).

Please find our responses to the minor comments annotated in the PDF below.

Originally lines 18 and 21: Define the acronym here instead of L21.

Now lines 18 and 21: We brought the Atlantic Meridional Overturning Circulation (AMOC) forward to line 18 instead of in line 21.

Originally line 27: I would clarify what exactly you are referring to here

Now line 28: We changed the sentence from ‘While the duration of the events as seen in Greenland $\delta^{18}\text{O}$ records differ and deviations in their appearance existed’ to ‘While the duration and amplitude

of changes between each event differed as seen in Greenland $\delta^{18}\text{O}$ records' to clarify the slight difference in temperature patterns between each Dansgaard-Oeschger (D-O) event.

Originally line 36: Consider another wording to replace "constant".

Now line 38: We changed the word 'constant' to 'persistent'.

Originally lines 37–39: Mind that not all readers may be familiar with the distinction between North Atlantic and Nordic Seas. Consider introducing this difference.

Now lines 39–43: To introduce the different roles of the North Atlantic and the Nordic Seas in influencing Greenland ice core climate signals in relation to sea ice changes, we revised the sentence and added a definition distinguishing the Nordic Seas from the North Atlantic.

Originally line 55: Greenland ice core records?

Now line 58: We added 'ice core records' following 'Greenland' by the end of the sentence.

Originally line 62: I would remove this ref here. Use the ref when you introduce the AMOC record (if that is taken from Henry et al).

Now line 65: We removed the citation (Henry et al., 2016) following 'changes in the AMOC' since this reference provides a reconstruction of AMOC strength, which is not the main focus of this sentence.

Originally line 66: specify "in the same record", if appropriate.

Now line 69: We added 'measured on planktonic foraminifera from the same sediment core' to the sentence to emphasize that the new SubST records in the eastern Fram Strait are derived from the same sediment core (MD99-2304) as the new biomarker-based sea ice reconstruction.

Originally lines 66–69: consider specifying these other "parts" by referring to the locations in Fig 1.

Now lines 72–74: To specify the sites used for comparison in this research (originally mentioned as 'other parts of the eastern Nordic Seas', we changed the sentence to 'To evaluate the relationship between ocean circulation and sea ice, we compare our new records from the northernmost Nordic Seas with existing sea ice reconstructions from the eastern Nordic Seas and AMOC reconstruction from the North Atlantic (Henry et al., 2016). The sea ice records used for comparison are from a site slightly further north in the Fram Strait (core 1 in Fig.1; El bani Altuna et al., 2024), the Faroe–Shetland Channel (core 3 in Fig. 1; Sadatzki et al., 2019), and the Vøring Plateau (core 2 in Fig. 1; Sadatzki et al., 2020).'

Originally line 68: 1 reconstruction or multiple reconstructions?

Now line 71: We changed 'AMOC reconstructions' to 'AMOC reconstruction' to reflect that it refers to a single reconstruction based on the Pa/Th record from sediment core CDH19. We have also checked other occurrences of this term in the manuscript to ensure it has been updated consistently.

Originally line 70: I think this figure is fine. One possible improvement you can consider is opening up the view on the Fram Strait a little. It looks a little packed.

Now line 75: Thank you for your suggestions. However, the modern-day SIE from NSIDC is currently in a North Pole orthographic projection and cannot be reprojected. Therefore, changing

the map's projection to open up the view of the Fram Strait does not work in this case and we decided to keep the map unchanged.

Originally lines 79–81: Not sure you need this.

Now lines 85–86: We removed the website link for ODV but kept the reference, as it is required by its usage terms. The reference to Inkscape was removed. Similarly, we removed website links from the captions of Figure 4 (line 269) and Figure 6 (lines 542–544).

Originally line 110: I would end this Section 2 with a brief sentence that links your study (Eastern Fram Strait) to the general oceanographic setting.

Now lines 113–114: To connect this research, which focuses on the eastern Fram Strait, to the broader oceanographic context of the Nordic Seas and North Atlantic without repeating the opening sentence of Section 2, we added the following summary paragraph: ‘This research focuses on the eastern Fram Strait, where the interaction between the AW inflow and sea ice conditions plays a critical role in shaping regional and large-scale climate and hydrography.’

Originally line 111: I think 3.2 and 3.3 read well. However it would be helpful for the reader to know what these proxies mean ahead of time. Consider introducing a brief sentence (for example here in Sect 3) in which you say something like "we measure X-Y-Z, as they indicate of X1-Y1-Z1", so that when the reader dig in 3.2and 3.3, it knows already what proxies they are.

Now lines 116–117: To provide a brief introduction to the proxies used in this research, we the following paragraph before Subsection 3.1: ‘We measured sea ice biomarkers and Neogloboquadrina pachyderma Mg/Ca ratios from core MD99-2304 to reconstruct SIE and SubSTs in the eastern Fram Strait. These proxies are described in more detail in Subsections 3.2 and 3.3, respectively.’

Originally line 114: core 2 in Fig. 1.

Now line 120: We changed ‘the central Norwegian Sea’ to ‘the Vøring Plateau’ ensure precision and consistency throughout the manuscript. We also added ‘core’ in front of ‘2 in Fig. 1’ in the figure reference.

Originally line 116: to some Greenland chronology?

Now lines 125–126: We have slightly rephrased the information on chronology. Following the introduction of the MD99-2284 chronology, we refer to the Greenland Ice Core Chronology 2005 (GICC05) to clarify the chronological framework used for the marine sediment cores in the eastern Nordic Seas.

Originally line 117: I would end up Sect. 3.1 with a sentence mentioning the overall quality of the chronology (or rather the confidence) of the MD99-2304. Maybe specify what is the age uncertainty? Or an upper limit.

Now line 127–131: We have added a few sentences about the overall quality of the chronology of MD99-2304. To clarify, we also slightly rephrased the information already provided in the chronology section (lines 119–127).

Originally line 146: are interpreted when combined together.

Now line 163: We changed ‘all biomarkers are interpreted combined’ to ‘the analyzed biomarkers are interpreted when combined’.

Originally line 151: are related to limited sea ice extent.

Now line 168: We changed the sentence from ‘PIP₂₅ values between 0.1 and 0.5 are defined as limited level of sea ice’ to ‘PIP₂₅ values between 0.1 and 0.5 are related to limited SIE.’

Originally line 151: I would replace "mean".

Now line 168: We changed ‘mean’ to ‘suggest’.

Originally line 152: I would replace "defined".

Now line 169: We changed ‘defined’ to ‘interpreted’.

Originally lines 176–177: Please better define "most frequent variability".

Now lines 204–205: To be precise, we rephrased from ‘HS-4 shows the highest and most frequent variability in biomarker concentrations and PIP₂₅ indices’ to ‘biomarker concentrations and PIP₂₅ indices during HS-4 show the most pronounced and frequent fluctuations’.

Originally lines 184–185: I am not sure I see significant differences between GS-8 and HS-4 SubST.

Now lines 212–215: Some datapoints in the Mg/Ca-based SubST record were accidentally excluded from the original manuscript due to the use of an older version of the dataset. We have now included the missing datapoints in all figures, and the dataset currently available on Pangaea.de is the updated and most complete version. These datapoints did not alter the main argument of the manuscript but rather strengthened it, regarding the ‘ocean circulation-sea ice’ relationship in the eastern Nordic Seas.

In response to this update, we revised the statement from ‘These trends lead to PIP₂₅ indices increasing almost steadily from ca. 0.3 to 0.7 throughout GS-8. SubSTs varied between 1°C and 4 °C during GS-8, with much sharper changes and more low values compared to HS-4.’ to ‘These trends lead to a steady increase in PIP₂₅ indices from ca. 0.3 to 0.7 throughout GS-8. SubSTs varied between 1°C and 9°C during GS-8, with lower temperatures corresponding to higher PIP₂₅ indices.’

After the datapoints were included, we adjusted the boundary between Mode III and IV during GS-8 in Figure 4 (line 261). This revision was necessary because the updated trends in SIE and SubSTs clearly reflect the variability that characterizes Mode IV. Since this change does not alter the message of the manuscript, we did not revise the corresponding text.

Originally lines 193 and 216: I would rephrase as "signal/relate to different" sea ice conditions.

Now lines 224 and 250: We updated sentence from ‘The boxes in the PIP₂₅ indices stand for categories of different sea ice conditions.’ to ‘The boxes in the PIP₂₅ indices relate to different sea ice conditions.’

Originally line 204: I would change this word.

Now lines 235–236: To be more precise, we changed the word ‘otherwise’ to ‘during the mid to late stages of GI-8’.

Originally line 206: Would it be fair to say that the PIP trends in GI-8 are dominated almost completely by the trend of IP?

Now line 238: Regarding the question of whether the PIP_{25} trends during GI-8 are dominated almost completely by the IP_{25} trend, the PIP_{25} indices reflect the combined influence of both IP_{25} and phytoplankton marker concentrations. Therefore, we retained the explanation of how biomarker concentrations differed between GI-7 and GI-8 following the statement ‘ PIP_{25} indices were in general higher during GI-7 than in GI-8’.

Originally line 214: Would it be helpful to add some splines to better display these trends, with the points displayed with transparency?

Now lines 220 and 246: We have updated Figures 2 and 3 to display the data points with 50%–75% transparency. However, the trends shown in these figures and in the results section are based solely on records from MD99-2304. In the discussion section, we further divide the investigated period into five distinct modes illustrating different ‘ocean circulation-sea ice’ relationships, incorporating additional records from the eastern Nordic Seas. To avoid potential confusion for readers, we have chosen not to add splines to Figures 2 and 3.

Originally line 222: than previously assumed in this specific region or elsewhere? Maybe add a comment regarding the fact that the higher variability you found is not related to a different (higher) sampling resolution is MD99-2304 compared to the other records?

Now line 255: To highlight uniqueness of our new records and improve geographical specificity, we added ‘in the eastern Fram Strait’ to the sentence. We also added ‘differences that cannot be attributed to a higher temporal resolution relative to other records’ following ‘our SIE and SubST reconstructions highlight the distinctly different sea ice conditions in the northernmost Nordic Seas’ in line 258 (originally line 224) to specify that the higher variability in MD99-2304 is not related to a different (higher) sampling resolution when compared to the other records.

Originally lines 227–236: Title axis should include the proxy and units.

Now lines 261–274: We revised Figure 4 to better illustrate that the five different modes are found within individual GSs and GIs, instead of being strictly tied to the overarching climate state. We also included the proxy ‘Pa/Th’ for AMOC strength reconstruction on the y-axis of panel (C), but did not add units, as Pa/Th is a unitless ratio. Additionally, we included the site numbers in the map, consistent with those shown in Figure 1.

Originally line 266: I would consider also indicating the time period you think these modes refer to.

Now pages 14–28: We restructured the discussion section and merged Subsections 5.1 and 5.2. Each mode is now introduced with the specific time period during which it occurred.

Originally line 283: You miss the verb in this sentence?

Now line 337: the verb is ‘is supported by’ in this sentence.

Originally line 328: I think as soon as the reader has finished reading Mode V, she/he has already forgot about Mode I and has to go and read again the section. Why not directly indicating the periods straight when the modes are presented?

Now pages 14–28: As noted in our earlier response, the discussion section has been restructured to more clearly present our results.

Originally line 358: "with the suggested AMOC mechanisms"?

Now line 358: We rephrased from ‘while this interpretation aligns with indicated AMOC variability’ to ‘while this interpretation aligns with the suggested AMOC mechanisms’.

Originally line 394: Is this modeling work or an observation that AW surfaced? I would in general throughout the text use some wording that allows the reader to have a better sense of evidence vs models vs hypotheses etc.

Now lines 417–418: Dokken et al. (2013) hypothesized that the AW reached the surface as sea ice retreated from the southeastern Nordic Seas, based on proxy records and conceptual models. This is further supported by proxy data from Sadatzki et al. (2019). Therefore, we have added a reference to Sadatzki et al. (2019) to strengthen this argument.

Originally line 407: modern observations of...

Now lines 445–446: To reduce redundancy while maintaining logical flow after merging the former Subsections 5.1 and 5.2, we revised the sentence from ‘These physical processes are supported by modern observations (e.g., Schlichtholz, 2011; Alexeev et al., 2017).’ to ‘Modern oceanographic observations from this region support the presence of similar physical processes (e.g., Schlichtholz, 2011; Alexeev et al., 2017; Smedsrud et al., 2022).’

Originally line 414: Very nice figure. I would increase the sea ice cartoon, it is barely visible. Maybe you could make it full black or increase its thickness.

Now line 535: Thank you for your suggestion. To better visualize the sea ice cover, we changed the color of the sea ice cover to green. Since the profile figures are drawn to scale using modern real-world oceanographic and topographical data, we chose not to exaggerate the sea ice thickness in the schematic. Furthermore, we included the site numbers on the map, consistent with those shown in Figure 1, and omitted labeling the sites in panels (A) to (D).

Originally line 434: I would merge 5.3 with your current Conclusion section.

Now line 557: We moved and merged this subsection to the conclusion section.

Originally line 439: Any comment to explain these discrepancies?

Now lines 593–596: Dokken et al. (2013), Sadatzki et al. (2019), Sadatzki et al. (2020), and El bani Altuna et al. (2024) proposed a perennial sea ice cover in the eastern Fram Strait during the investigated period. Based on SIE and SubST reconstructions from site MD99-2304, we argue that the discrepancy between these conceptualizations and the more variable sea ice conditions at MD99-2304 is likely due to the coupled effect between the AMOC strength and sea ice conditions in the southeastern Nordic Seas. This effect appears to be localized to site MD99-2304 due to topographic constraints, since results from HH15-1252PC (El bani Altuna et al., 2024) indicate perennial sea ice throughout the same period. Accordingly, we have added statements explaining the differences between sites HH15-1252PC and MD99-2304 in lines 392–393 (Subsection 5.3) and 584–586 (conclusion).