nature portfolio

Peer Review File

Insolation-forced oceanic changes triggered the Eurasian Ice Sheet collapse and the Last Termination

Corresponding Author: Dr Yanguang Liu

Parts of this Peer Review File have been redacted as indicated to remove third-party material.

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
In this manuscript, the authors present a novel reconstruction of the climatological events driving the early phase of the last deglaciation of the Northern Hemisphere ice sheets. This reconstruction is based on the geochemical analysis of an individual gravity core retrieved north of the Iceland-Faroe Ridge. The core is compared with other proxy records from the Atlantic and Nordic Seas and with a transient climate simulation of the last deglaciation. The main findings of the manuscript are (1) an orbital-driven sub-surface warming in the Nordic Seas triggered the collapse of the marine-based sector of the southern Eurasian ice sheet; (2) the subsequent release of glacial meltwater contributed to weaken the overturning circulation and deep water formation in the North Atlantic, inducing further sub-surface warming which may have promoted meltwater surges from the Laurentide Ice Sheet contributing to Heinrich events.

These results are extremely relevant and provide a novel contribution on the importance and timing of ice sheet, atmosphere and ocean interactions at the onset of the last deglaciation, including widely studied climatic events such as the Heinrich events. The methodology looks mostly sound and well described. One limitation is that the manuscript is analyzing only an individual core, but the authors do a good job in comparing their results with independent lines of evidence such as other proxies and model simulations. Overall, the paper results are worthy of publication; there are however a few major issues that the authors should address before considering the manuscript ready for publication. These are mainly related to the way results are presented and discussed in the text, and it should be possible for the authors to address them without the need for further analysis.

Major comments:

1) In this manuscript, the authors widely cite the TraCE transient simulations of the coupled simulation by Liu et al. 2009, which shows an evolution of ocean properties similar to those found by the authors. However, TraCE does not include interactive coupling with ice sheets, and it's based on freshwater hosing experiments after 19 ka. For clarity, this should be mentioned. Moreover, there is a much more recent modelling work (Mikolajewicz et al. 2024, accepted for the journal Climate of the Past) performing an ensemble of fully coupled climate-ice sheets-solid Earth simulations of the last deglaciation. In the beginning of Section 4.1 of the preprint of Mikolajewicz et al. 2024 ("Drivers of abrupt climate events during the last deglaciation"), it is shown that the model simulates at 18.0 ka the exact chain of events described in this manuscript. Despite the timing difference, this is great - as a numerical model including ice sheets/climate interactions is simulating the chain of events presented in this manuscript. More in general, the authors should use the simulation in Mikolajewicz et al. 2024 as the main benchmark for the manuscript, although it's good to also keep a comparison with TraCE.

2) The sediment core shows surface water warming followed by surface water cooling and input of IRD, of which the latter is attributed to freshwater discharge from the EIS. How can one differentiate between whether the meltwater pulse can be attributed to submarine melt in response to ocean warming versus a negative surface mass balance in response to atmospheric warming, also driven by orbital forcing? Perhaps the answer is that one can't differentiate based on the sediment cores - which is fine, but this should be mentioned in the manuscript. Also in this case, the simulations by Mikolajewicz et al. 2024 might help to answer this.

3) If the authors would like to keep the comparison with the TraCE simulations in the revised manuscript, it would be good to receive more information on the isolated forcings of the TraCE simulations. In the manuscript, the authors conclude that isolated insolation forcing contributes most to the increased wind velocity between 20-22 ka BP by comparing the separate isolated forcing simulations, but there are no figures showing the difference between an orbital-isolated forcing versus the other forcings separately (other than the all-forcing Suppl. Fig. 2A). Are those simulations available for this time frame? Additionally, if Figure 4B shows orbital forcing only, is panel 4A also isolated orbital forcing?

4) In general, the quality of the manuscript should be improved. Text is often not consistent with dashes, spaces or other terms. The resolution of the figures should be higher, as labels or annotations are currently hard to read, and figure captions lack sufficient descriptions (Figures 5-8 lack sufficient descriptions, Figures 5 and 7 specifically lack descriptions of the individual panels). Some suggestions are included in the specific comments below.

Minor comments:

Abstract

The article 'the' is often missing, for example 'in Quaternary' (L17), 'in Eurasian Ice Sheet' (L24), 'into Nordic Seas' (L28).
L18: Perhaps 'primary driver' instead of 'ultimate driver'?
L21: Please provide a time interval (~21 ka, 22-20 ka, or any other form you prefer).
L22: To my knowledge, it is either kyrs BP or ka.
L23: I think demonstrate is a bit too strong, perhaps something lighter like 'indicate', 'show' or 'suggest'?
L27-28: Perhaps 'marine sectors of the Eurasian Ice Sheet' or 'marine terminating outlet glaciers of the EIS' instead of 'marine-based Eurasian Ice Shelves'?
L28: Is it possible to use another term than 'catastrophic'? It doesn't really relate to quantity.
Introduction
L35: Maybe 'understanding' rather than 'elucidating'?
L37: I would try to explain this better. Something like 'leading to surface melt which reduced ice thickness and contributed to lubricating the ice sheet base increasing sliding and ice discharge.'
L38: 'oversized continental ice', not sure why it would be oversized? I would suggest removing that adjective.
L38: 'glacial freshwater'.
L42: I would recommend including a time interval for this (at least the first time Last Termination appears in the text).
L42: Figure 2 appears before Figure 1, while figures should be numbered according to appearance in text.
L44: 'dominated Laurentide Ice Sheet', do you mean dominating? I might leave that adjective out entirely.
L45: I think the terminology is slightly off here, and it should be 'grounding line retreat of marine-based portions of an ice sheet' or something similar.
L45-46: perhaps 'destabilizing the continental ice sheets' instead of 'destabilizing the overall stability of'?
L49: there is more recent literature on this focusing on past interglacials/last deglaciation: see for instance Alvarez-Solas et al. 2019, Climate of the Past, or also Petrini et al., 2020, Quaternary Science Review.
L49: 'northern ice sheets', do you mean 'Northern Hemisphere ice sheets'? The ice sheets are referred to inconsistently throughout the manuscript.
L56: 'contrast' rather than 'disparity'?
L59: it is common to capitalize Last Glacial Maximum, instead of 'last glacial maximum (LGM)', and to provide a time interval. The latter would be useful when referring to 'the later stage of the LGM' (L221) for example.
L61: 'sea-level fall' rather than 'sea-level reduction'?
L61: Not sure of what you mean with 'forming an extensive interface with the Nordic Seas and the North Atlantic'.
L69: Maybe better 'evolution of the upper ocean water masses at the beginning of the last termination'? 'under orbital forcing' is not completely clear to me.
L74: 'early' rather than 'advanced'?
L74: Please provide time interval for HS1.

Results and discussion:

L77-78: appears too much space between degree and minutes of the coordinates. Also, remove space in 'hereafter'?
L94: It could make sense to give the reader an idea of what the sampling interval resolution is (although it is also explained in the Methods section).
L136-137: 'the degree of dispersion of temperature of temperature distribution' is a bit of a mouth full.
L142: Fig. 5B is mentioned before Fig. 3 and 4
L143: Maybe something like 'support our findings, showing ...'
L170: 'latest stage of the LGM'?
L172: should it be '14C age offset' instead of '14C offset'?
L174: 'can be presumably'?
L185: Not sure of what you mean with 'local feedback mechanism to global scale climate driving forces'.
L187: Please rephrase, something like 'surface warming may have started earlier'.
L190: are these 'radiocarbon dates at 22.7 ka BP and 18.6 ka BP' calibrated? The 18.6 ka BP age corresponds to a calibrated age in Table 1, but there is no 22.7 ka. Only a 22.6 ka uncalibrated or a 21.7 ka BP that is calibrated. I suspect it should be 21.7 ka BP in the text.
L191: 'This'.
L205: 'some', or 'several' model simulations (unless you want to refer to one specific simulations).

L222: ‘that enhanced surface winds’.
L233: ‘deglacial epoch’ might be confusing, as an epoch refers to the entire geological unit, i.e. either Holocene or Pleistocene. There exists no deglacial epoch, as the deglaciation lasted from the LGM to the Early Holocene.
L252: I assume submarine melt is meant with ‘heat eroding ice shelves’? I would suggest using ‘melting’ instead of ‘eroding’.
L253: I don't understand why ‘moisture source’.
L259: ‘remain invigorated’ does not sound great, as the rest of the sentence. Could you please rephrase?
L262: ‘delay of mid latitude’, maybe it is clearer as ‘the delayed response of the mid latitudes’?
L263-266: I do not quite follow this sentence, maybe rewrite to clarify your meaning?
L270-274: This should be highlighted more, perhaps also in the abstract, as it is a key finding.
L283: ‘buttressing’.
L290-291: you mention you ‘consider the rising atmospheric CO2 to be a key factor to complete deglaciation’, but not a key factor during the early EIS collapse based on the ice core record. Do the TraCE simulations (as well as Mikolajewicz et al. 2024) show that isolated greenhouse gas forcings do not play a role either?
L293: ‘boreal ice-sheet process’ is a bit of a vague phrase. Do you refer to the dynamics of the Northern Hemisphere ice sheets during the early deglaciation?
Tables and figures
Table 1: the ‘/a’ in the column headings of the AMS radiocarbon age and calendar age can be put between brackets ‘(a)’
Fig. 1: I would suggest considering a legend in the figure for the different point data (yellow star, white squares, black dot) so you do not need to repeat this in the figure caption. The labels for NAC and WSC are not readable.
Fig. 2: I think lower case for the x-axis unit ‘ka BP’ is common, instead of ‘Ka BP’. It seems that the proxy records describe the last 31 ka, not ‘record over the last 30 ka’? Perhaps change the location of ‘NGRIP’ in the figure caption of panel A: ‘Stable oxygen isotopic composition and temperature from the NGRIP ice core’.
Fig. 4: There could be more information on panel A, such as whether the transect is at 9° W, and that the grey fill indicates the ocean floor. Brackets are missing around °C after Temperature for the color bar. Panel B has no variable (wind velocity?) label under the color bar. Also, what do you mean by orbital forcing only?
Fig. 5: This is a nice summary figure, but the caption lacks descriptions of the individual panels. The resolution is too low and the size too small to properly read it. I wonder whether the DEM source for the hill shades should be referred to.
Fig. 6: the figure caption is a bit too vague, describe the ‘different ways’.
Fig. 7: there is no caption describing the individual panels A and B.
Fig. 8: I would put seconds between brackets behind time: ‘Time (s)’.

References:

Mikolajewicz, U., Kapsch, M.L., Schannwell, C., Six, K.D., Ziemen, F.A., Bagge, M., Baudouin, J.P., Erokhina, O., Gayler, V., Klemann, V. and Meccia, V.L., 2024. Deglaciation and abrupt events in a coupled comprehensive atmosphere–ocean–ice sheet–solid earth model. Climate of the Past Discussions, 2024, pp.1-46.

Alvarez-Solas, J., Banderas, R., Robinson, A. and Montoya, M., 2019. Ocean-driven millennial-scale variability of the Eurasian ice sheet during the last glacial period simulated with a hybrid ice-sheet–shelf model. Climate of the Past, 15(3), pp.957-979.

Petrini, M., Colleoni, F., Kirchner, N., Hughes, A.L., Camerlenghi, A., Rebesco, M., Lucchi, R.G., Forte, E., Colucci, R.R., Noormets, R. and Mangerud, J., 2020. Simulated last deglaciation of the Barents Sea Ice Sheet primarily driven by oceanic conditions. Quaternary Science Reviews, 238, p.106314.

M. Petrini and Early Career Researcher K. Ploeg equally contributed to this review, as part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #2

(Remarks to the Author)
Review of “Insolation-triggered Eurasian Ice Sheets collapse initiates the Last Termination” by Wu et al.

The article by Dong Wu et al. explores the role of increasing boreal insolation in triggering the collapse of the Eurasian Ice Sheet (EIS) at the end of the Last Glacial Maximum (LGM) and facilitating the occurrence of the termination. It presents evidence from proxy reconstructions and examines well-known transient climate simulations, to suggest that oceanic processes played a crucial role in destabilizing the EIS before Heinrich Stadial 1 (HS1), which initiates a chain of ocean/atmosphere ice-sheet feedbacks at the heart of the deglaciation dynamics.

I find the article to be well written and it addresses an important question on the climate processes triggering the last deglaciation. The idea is seemingly well supported by proxies (although I am not an expert on data) and justified with the analysis of already existing deglacial simulations; so the work is, in my opinion, suited for publication in Nature Communications.

General comments:

That being said, the study proposes a chain of events initiating the last deglaciation which is coherent with most of the existing literature, but, in order to get an idea of the realism of the climatic processes in play, the reader may find that there are a number of implications and questions that remain not addressed nor answered.

I will, in the following, list several of them. I do not expect (it would be unfair) to get an unambiguous response from the authors of every of these questions. However, I believe the paper will largely benefit from including the discussion they might inspire.

Is the proposed mechanism a necessary condition for a termination to happen? The title and conclusion somehow suggest it is. Would you expect something similar for other terminations? Could your core be analyzed in that sense? Indirectly related, the only coupled climate-cryosphere successful simulations (to a good degree) of glacial cycles are shown in the work of Ganopolski et al. (Which are not cited here) and they do not need such a mechanism. It is true that their intermediate complexity approach is probably not well suited to capture the processes at the heart of the mechanism here proposed. However, that work would suggest that an earlier demise of the EIS is not required for the termination. A small discussion on this would be beneficial for the paper.

Another unaddressed question the reader may have when trying to judge your proposed mechanism is the following: Why is not the increase in insolation at the beginning of MIS3 causing a termination? Of course the EIS was not at that time as extended as it is for the LGM, but, what makes your mechanism unique in the sense of initiating an insolation-triggered deglaciation which does not happen for other periods of the glacial cycle?

Examining Figure 5 one could wonder whether such a release of energy to the atmosphere from the LGM (panel A) to late LGM (panel B) has an impact on ice cores in Greenland. GRIP and NGRIP seem to not show anything. It might however be the case in GISP2. Also the TRACE21 simulations capture well (because of ad hoc meltwater input) the evolution of Greenland temperatures since the beginning of HS1, but they seem to have an important offset with respect to proxies at the beginning (early LGM). In the search for coherence with your current figure 5, it might be interesting to show the sea-ice extent differences between the early and the late LGM in those simulations and also expand the discussion on this matter.

Do you expect that ice-sheet simulations will react to your identified NordicSeas warming the way you hypothesized? In my modelling experience what you suggest makes total sense, but (in the absence of ice-sheet simulations in your paper) perhaps a more complete revision of what the modelling papers of the deglacial EIS show is needed here.

The role of the Nordic Seas and the EIS in modulating millennial variability in glacial cycles has been thoroughly addressed in Dokken et al., 2013, which is not cited here.

Other comments/suggestions:

The abstract and line 45 (and other places in the manuscript) read:
“...thereby accelerating the ablation of the marine-based Eurasian Ice Shelves“ and “...the grounding line retreat of marine ice shelves…”.
This is not accurate: First, the ice shelves are marine by nature (because they are floating in the ocean), what is marine is a region of an ice sheet that lies on a bedrock below sea level. Second, what retreats is the grounding line itself, not “the grounding line of marine ice shelves”.

Line 196 says “caloric summer insolation” while the caption of figure 3 says “Summer solstice insolation”. And even the axis of figure 3 says “Caloric summer half-year insolation”. Please be coherent with the naming, specify what metric you are plotting and justify which one is more appropriate and why.

Line 43: …preceded the episodic…” ⇒ Why not mentioning explicitly HS1 if that is what you meant

Line 44: Dominated? ⇒ Dominating?

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #4

(Remarks to the Author)
Please find my complete review in the attached documnet

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Second round of Review Wu et al.

Comment 1: While it is true that the authors "[…] did not use the TraCE results to investigate ocean–ice sheet interactions or to simulate the behavior of the EIS", the interlinked interaction between EIS/LIS and the ocean is a big focus of the paper. So it's perhaps ok to not explicitly mention the absence of coupled ice sheet in TraCE, but in our opinion more emphasis should be given to the simulation from Mikolajewicz et al. when discussing the main conclusion of the paper, that the melt water pulse that initially slows down the AMOC occurs prior to HS1 and originated from the EIS, not the LIS.

Comment 3/Figure 4: Due to space limitations, the individual figures for each isolated forcing experiment are not included. Is there perhaps space in the supplementary materials?
Comment 16 (L46): The "widely accepted time interval" that is cited describes the deglaciation itself, but not the actual Last Glacial Termination (Termination I), which refers to the transition from full glacial to full interglacial climates as defined by the Marine Isotope Stages. See Broecker & van Donk (1970), which Denton (2010) is referring to. Clark et al. (2009) is cited as well, but this article is about the LGM. Dyke & Prest (1987) are cited, but with the wrong publication date. It should be 1987, not 2008 (which is when it was made available online).

L51: Missing space, 'ocean-ice sheet'

L55: I would remove systematically.

L117-118, L243, L283: Missing uncertainties on the 14C dates

L258: "weakening" instead of "weaking LIG"

L272: typo, 'deglaciation'.

L276: "weakened the buttressing of ice shelves", by melting sea ice? Or melting of ice shelves reduced buttressing of the marine-terminating glaciers and increased ice discharge?
L317: Missing "the" in front of "Last Termination"
L320: "solar activity" implies short-term activity (such as sunspots), I assume you refer to the orbitally controlled "solar insolation" instead
L324-325: "the Last Termination of Northern hemisphere ice sheets", perhaps " the driving mechanisms for the Last Termination and the initial collapse of NH ice sheets"

M. Petrini and Early Career Researcher K. Ploeg equally contributed to this review, as part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #2

(Remarks to the Author)
I congratulate the authors for carrying out a thorough revision of the manuscript and adequately addressed all concerns. So I can now recommend publication of the study

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #5

(Remarks to the Author)
In this study, the authors reconstruct, among other variables, the evolution of subsurface ocean temperatures in the southern Nordic Seas during the last deglaciation using a combination of geochemical proxies, from sediment core ARC5-IS-1B. The authors also emphasize the large marine-based extent of the Eurasian Ice Sheet (EIS), and consequently its potential vulnerability to ocean temperature fluctuations. The central conclusion of the paper relies on the observed correspondence between increasing subsurface ocean temperatures and enhanced freshwater inputs, interpreted as resulting from EIS melting. Based on this temporal correspondence, the authors argue for a dominant role of ocean forcing in the destabilization of the Eurasian Ice Sheet.

While the new subsurface ocean temperature reconstructions presented in this study represent a significant advance and substantially improve our understanding of regional oceanographic changes, I remain skeptical regarding the main conclusion of the paper, specifically, the attribution of a dominant causal role to ocean warming in triggering the initial retreat of the EIS. The following comments outline the main concerns associated with this interpretation.

Comment 1 – Atmospheric temperature evolution

Line 280-282: surface air temperature reconstructions show no significant warming over the EIS between ~21 and 18 ka BP, suggesting the contribution of surface mass balance in this meltwater pulse was likely limited (Osman et al., 2021)

Paleoclimate data assimilation by Osman et al. (2021) indicates that atmospheric temperatures remained roughly constant between 21 and 18 ka over EIS. However, presenting this information in the manuscript as such may be an oversimplification. Depending on the selected age model, simulations performed with iCESM1.2 and iCESM1.3 show that while some runs suggest negligible changes in mean annual atmospheric temperatures over the British–Irish Ice Sheet (BIIS) and the Fennoscandian Ice Sheet (FIS), others indicate warming of up to ~2 °C. Averaging across all age models, Osman et al. (2021) report a mean increase of approximately 1 °C in annual air temperature over the BIIS and FIS between 21 and 18 ka (see attached figures). This atmospheric warming is even more pronounced over regions corresponding to present-day Great Britain.

More generally, other studies also point toward early atmospheric warming during the last deglaciation. Bouttes et al. (2023), using transient simulations of the last deglaciation with the iLOVECLIM model, report a several-degree increase in surface air temperature over the Eurasian Ice Sheet beginning at the onset of deglaciation. In addition, $\delta^{18}O$ reconstructions from the GRIP ice core indicate a marked atmospheric warming between 21 and 18 ka. Furthermore, recent simulations by Mikolajewicz et al. (2024) also explore atmospheric temperature changes over the EIS, though these results are not discussed in the present manuscript. And what about TraCE simulations ?

I understand the argument presented by the author, but all these studies collectively highlight that significant uncertainties still exist regarding temperature evolution. Consequently, it is extremely difficult to use this argument alone to dismiss the role of the atmosphere in the retreat of the EIS.

Moreover, these uncertainties on atmospheric temperature are even more significant due to the extreme sensitivity of the EIS to atmospheric warming, with an increase of 1 or 2 °C relative to the LGM being sufficient to induce substantial ice loss, particularly at the connection between the BIIS and FIS (Van Aalderen et al., 2024).

Comment 2 – Ocean warming

The hypotheses proposed regarding the behavior of the EIS rely primarily on reconstructed subsurface ocean temperature fluctuations derived from the sediment core, without explicitly assessing whether the magnitude of this ocean warming is sufficient, by itself, to trigger ice-sheet retreat.

The authors report an increase of 5.62 °C in subsurface temperatures (SSTsub) after ~20.16 ka and associate this warming with enhanced melting of the EIS. However, Van Aalderen et al. (2024) identify a threshold behavior in their simulations, whereby a minimum subsurface ocean warming of approximately 7.5 °C is required to initiate the first retreat of the EIS. While this threshold likely depends on the modeling framework and experimental design, the discrepancy raises the question of whether the reconstructed ocean warming presented by the authors is quantitatively sufficient to trigger ice-sheet retreat.

In addition, Petrini et al. (2020), using ice-sheet model simulations of the Barents–Kara Ice Sheet (BKIS), show that an initial retreat occurs in the Bjørnøyrenna region between 20 and 18 ka even when ocean forcing remains constant or slightly cooler than during the LGM. This result suggests that other mechanisms, such as rising sea level or atmospheric warming, may have contributed to the initiation of ice-sheet retreat.

Furthermore, Van Aalderen et al. (2024) demonstrate that the sensitivity of the EIS to ocean warming is strongly amplified when atmospheric temperatures increase simultaneously. A combined effect of oceanic and atmospheric warming therefore represents a plausible mechanism for initiating EIS retreat. Finally, global sea level is estimated to have varied by approximately ±20 m between the LGM (~26–21 ka) and 18 ka, and Van Aalderen et al. (2024) also highlight the sensitivity of the Eurasian Ice Sheet to sea-level changes.

Taken together, these results raise a key question that is not addressed in the manuscript: is the reconstructed ocean warming quantitatively sufficient to initiate EIS retreat on its own, or does it represent one contributing factor among several? The paper does not provide a clear answer to this question.

Comment 3 – Relative importance of basal melting

Lignes 265–268 : « Modeling studies showed that basal melting, induced by oceanic temperature fluctuations, is considerably more efficient than surface mass balance changes in leading the higher rates of iceberg discharge along the EIS margins and the meltwater peaks on the multi-millennial evolution. »

Van Aalderen et al. (2024) show that the methodologies employed in some of these modeling studies tend to overestimate the role of ocean forcing in the destabilization of the EIS. When accounting for these methodological biases, the relative importance of ocean-driven basal melting may be significantly smaller than suggested by the cited studies, calling into question the strong emphasis placed on ocean forcing in this context.

Comment 4 – Conclusions

In conclusion, the current scientific literature remains characterized by active debate regarding the processes responsible for the retreat of the EIS during the last deglaciation.

The results presented in this paper constitute an important contribution to our understanding of oceanic changes along the margins of the Eurasian Ice Sheet. However, the hypotheses concerning ice-sheet dynamics rely primarily on interpretations of sediment-core data, without the support of new ice-sheet model simulations that directly incorporate the reconstructed subsurface ocean temperatures.

In the absence of such simulations, it remains difficult to robustly assess the response of the Eurasian Ice Sheet to the reconstructed ocean forcing and to identify the dominant mechanisms responsible for its initial retreat. Several key questions therefore remain unresolved: was the increase in ocean temperatures sufficient, on its own, to trigger EIS retreat between 20 and 18 ka? Did rising sea level contribute to destabilization in this region? Did atmospheric warming play a significant role? These questions are either only partially addressed or not discussed in the manuscript, limiting the ability to identify the primary drivers of the initial EIS collapse.

I acknowledge that implementing ice-sheet model simulations can be complex, and I do not suggest that such experiments are strictly required. However, a more balanced and deeper discussion of the different mechanisms potentially contributing to ice-sheet destabilization would have been desirable. In this context, the title of the paper “Insolation-forced oceanic changes drove the initial collapse of the Eurasian Ice Sheet and triggered the Last Termination” may be perceived as potentially misleading. A title more focused on the presentation of new subsurface ocean temperature reconstructions, rather than on Eurasian Ice Sheet dynamics, would appear more appropriate.

Finally, the experiments presented in this study do not allow for a clear quantification of the relative roles of atmospheric, oceanic or sea-level forcing in driving the initial retreat of the EIS.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Both Early Career Researcher Karlijn Ploeg, who helped me co-review this manuscript, and myself are happy with this latest version of the revised manuscript. We thank the authors for carrying out requested changes and we are looking forward to see the paper out!

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Nature Communications initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Reviewer #5

(Remarks to the Author)
The authors have revised the manuscript in response to the reviewers’ comments, implementing a range of modifications and providing clear and consistent answers to the questions raised. I would particularly like to thank the authors for taking into account the uncertainties that still exist in the literature regarding the respective impacts of the atmosphere and the ocean on the retreat of BKIS during the last deglaciation. In light of these revisions, I support the publication of this study.

Reply to the reviewers' comments on "Insolation-triggered Eurasian Ice Sheets collapse initiates the Last Termination" by Wu et al.

Content

Responses to the suggestions and comments by Reviewer 1

We sincerely thank the reviewer for the thorough and constructive evaluation of our manuscript. We greatly appreciate the insightful comments, which have helped us improve the clarity and quality of the paper substantially. We have carefully revised the manuscript to address all major and minor points raised. We believe that these revisions have significantly strengthened the manuscript. Below, we provide a detailed, point-by-point response to each comment.

• Comment on the limitations of TraCE and and inclusion of the Mikolajewicz et al. (2024) coupled model

1. In this manuscript, the authors widely cite the TraCE transient simulations of the coupled simulation by Liu et al. 2009, which shows an evolution of ocean properties similar to those found by the authors. However, TraCE does not include interactive coupling with ice sheets, and it's based on freshwater hosing experiments after 19 ka. For clarity, this should be mentioned. Moreover, there is a much more recent modelling work (Mikolajewicz et al. 2024, accepted for the journal Climate of the Past) performing an ensemble of fully coupled climate-ice sheets-solid Earth simulations of the last deglaciation. In the beginning of Section 4.1 of the preprint of Mikolajewicz et al. 2024 ("Drivers of abrupt climate events during the last deglaciation"), it is shown that the model simulates at 18.0 ka the exact chain of events described in this manuscript. Despite the timing difference, this is great - as a numerical model including ice sheets/climate interactions is simulating the chain of events presented in this manuscript. More in general, the authors should use the simulation in Mikolajewicz et al. 2024 as the main benchmark for the manuscript, although it's good to also keep a comparison with TraCE.

Response: we thank the reviewer for highlighting the limitations of the TraCE transient simulation, particularly its lack of interactive coupling with ice sheets. In our study, however, we primarily refer to the TraCE simulation to illustrate how insolation forcing drived northward ocean heat transport and upper ocean warming by modulating atmospheric and oceanic circulation patterns. We did not use the TraCE results to investigate ocean–ice sheet interactions or to simulate the behavior of the Eurasian Ice Sheet. So, we believe it is not essential to emphasize this particular limitation of the TraCE model in the manuscript.

Besides, reviewer 1 mentioned an outstanding modelling work of Mikolajewicz et al., 2024, which performs an ensemble of fully coupled climate-ice sheets-solid Earth simulations of the last deglaciation. Their simulated time series of the global freshwater input reveal a pronounced peak at around 18ka BP and Mikolajewicz et al attributed it to a surge event from the Fennoscandian ice sheet[1]. After the recalibration of our age framework according to the reviewer 3's suggestions using the Marine20 curve, our proxy records now show that the upper ocean cooling and IRD peak—occurring immediately after the anomalous warming event—also fall at ~18 ka BP, in excellent agreement with Mikolajewicz et al. Accordingly, we have cited their simulated results of the net fresh water input flux to bolster the credibility of the meltwater pulse documented in our study (Fig. 3F; line 245-247). We greatly appreciate the reviewers' insights, which have deepened our understanding of the interaction between ice sheets evolution and oceanic process, and also enriched the evidence for the mechanisms that initiated the last termination.

• Comment on distinguishing the contribution of submarine melt and negative surface mass balance

2. The sediment core shows surface water warming followed by surface water cooling and input of IRD, of which the latter is attributed to freshwater discharge from the EIS. How can one differentiate between whether the meltwater pulse can be attributed to submarine melt in response to ocean warming versus a negative surface mass balance in response to atmospheric warming, also driven by orbital forcing? Perhaps the answer is that one can't differentiate based on the sediment

cores - which is fine, but this should be mentioned in the manuscript. Also in this case, the simulations by Mikolajewicz et al. 2024 might help to answer this.

Response: recent model simulation results indicate that atmospheric forcing alone could only generate modest iceberg discharge from the Eurasian Ice Sheet during the last glacial period, while the warming of the ocean appears to generate higher rates of icebergs discharge as a result of relatively strong basal melting at the margins of the Eurasian ice sheet[2]. Therefore, considering the notable meltwater peak around 18.0 ka BP originated from the Eurasian Ice Sheet, as highlighted in the article recommended by the reviewer (Mikolajewicz et al., 2024), basal melting may represent a more efficient mechanism for generating this massive and abrupt meltwater release at the onset of last termination[1]. Additionally, the surface air temperature reconstruction by Osman et al. (2021) shows no significant warming over the Eurasian ice sheet between 21 and 18 ka BP[3]. This suggests that the contribution of negative surface mass balance driven by atmospheric warming to the meltwater pulse around 18.0 ka BP was likely limited.

Specifically regarding the regional changes of the Eurasian Ice Sheet, an "unzipping" of the British–Irish Ice Sheet (BIIS) and the Fennoscandian Ice Sheet (FIS) occurred around 18.5–17 ka BP, accompanied by the drainage of a large ice-dammed lake in the North Sea and a significant retreat of the ice margins of both the BIIS and FIS[4,5]. Simulation results, together with bathymetric and shallow seismic data, demonstrate that the rapid collapse of a marine ice sheet sector—driven by grounding line retreat of the marine-based Norwegian Channel Ice Stream—played a major role in the separation of the BIIS and FIS[4,6]. This supports the idea that both the BIIS and FIS were highly sensitive to relatively modest changes in the vertical thermal structure of the ocean and to variations in relative sea level in the Norwegian Channel[6].

As for the FIS, IRD and plumate sediment records from sites on the Norwegian shelf show that the IRD peaks occurred shortly after the multiple temperature rises in the adjacent surface water during the later stage of the LGM[7,8]. This suggests that the oceanic process associated to the particularly upper ocean warming and the eustatic sea-level fluctuations could have acted as key external drivers of FIS instability in the last termination.

A similar pattern of sustained meltwater release is also evident in the Barents–Kara sector. Even though the simulated surface mass balance in the Barents-Kara ice sheets (BKIS) remains positive between 21 and 15 ka BP, a steady retreat of its western margin and the grounding line of the Bjørnøyrenna Ice Stream resulted in a loss of more than 0.15 Mkm^2 loss of ice cover of BKIS between 19 and 18 ka BP[9]. Petrini et al., (2020) pointed out that the rapid increase in ice loss at the western margin, primarily due to sub-shelf melting, can be attributed to relatively strong oceanic thermal forcing from the presence of warm subsurface Atlantic Water[9]. This process led to prolonged ice shelf thinning and grounding-line retreat, accompanied by sustained freshwater discharge reaching rates of up to 76 Gt/yr[9].

Therefore, based on previous model simulations and proxy evidence from the Nordic Seas, we propose that oceanic forcing played a primary role in initiating the retreat of the Eurasian Ice Sheet prior to Heinrich Stadial 1. While some studies have emphasized the role of negative surface mass balance in response to atmospheric warming in driving Eurasian Ice Sheet instability during the last deglaciation, we argue that atmospheric forcing alone was likely insufficient to trigger the large-scale meltwater release observed between 21 and 18 ka BP.

We greatly appreciate the reviewer's valuable suggestion regarding the role of the negative surface mass balance. In response, we have added a statement in the manuscript addressing its potential influence on the Eurasian Ice Sheet during the early phase of the last termination in line 278-282.

• Comment on the isolated forcings of the TraCE simulations

3. If the authors would like to keep the comparison with the TraCE simulations in the revised manuscript, it would be good to receive more information on the isolated forcings of the TraCE simulations. In the manuscript, the authors

conclude that isolated insolation forcing contributes most to the increased wind velocity between 20-22 ka BP by comparing the separate isolated forcing simulations, but there are no figures showing the difference between an orbital-isolated forcing versus the other forcings separately (other than the all-forcing Suppl. Fig. 2A). Are those simulations available for this time frame? Additionally, if Figure 4B shows orbital forcing only, is panel 4A also isolated orbital forcing?

Response: in this study, we have carried out the TraCE simulations to investigate the influence of insolation on the atmospheric and oceanic circulation over the high latitude north Atlantic and Nordic Seas. Following the recalibration of our age model, we have further reperformed additional simulations to assess the effects of other isolated forcings (including greenhouse gases, ice sheets activities and meltwater release) on atmospheric and oceanic circulation as well as vertical ocean temperature profiles (19–21 ka BP). Our results show that the isolated insolation forcing and the all forcing simulation produce a consistent response in the 850 hPa wind field and surface wind strength, characterized by cyclonic intensification of 850 hpa wind and an overall increase in surface wind velocity. Moreover, the other isolated forcings exhibit only limited influence on the surface and 850 hpa wind fields. Notably, the simulation with isolated meltwater forcing shows an effect on upper ocean temperature that is opposite to the general warming trend seen in the all-forcing scenario. Due to space limitations in the main text, we have not included the individual figures for each isolated forcing experiment. For clarification, we would like to note here that Figure 4A presents the upper ocean temperature results from the all-forcing simulation to compare with our proxy results.

• Comment on text-format and figures

4. In general, the quality of the manuscript should be improved. Text is often not consistent with dashes, spaces or other terms. The resolution of the figures should be higher, as labels or annotations are currently hard to read, and figure captions

lack sufficient descriptions (Figures 5-8 lack sufficient descriptions, Figures 5 and 7 specifically lack descriptions of the individual panels).

Response: we sincerely thank the reviewer for the constructive comments regarding the overall quality of the manuscript. Following the suggestions, we have carefully revised the text to improve consistency in punctuation, spacing, and terminology throughout the manuscript.

We have also redrawn all figures with higher resolution to ensure better readability, particularly for labels and annotations. In addition, we have added detailed explanations for each individual panel—especially those of Figures 5 to 8—to provide clearer and more comprehensive descriptions.

• Other comments

5. The article 'the' is often missing, for example 'in Quaternary' (L17), 'in Eurasian Ice Sheet' (L24), 'into Nordic Seas' (L28).

Response: we sincerely appreciate the reviewer for pointing out these grammatical errors in our manuscript. We have corrected them and inserted the missing "the" before these proper nouns.

6. L18: Perhaps 'primary driver' instead of 'ultimate driver'?

Response: yes, it's the primary driver of the collapse of Northern Hemisphere ice sheets. We have revised the "ultimate driver" to "primary driver" (line 18).

7. L21: Please provide a time interval (~21 ka, 22-20 ka, or any other form you prefer).

Response: we appreciate the reviewer's comment on specifying the time interval of this warming trend in the abstract. Accordingly, we have added the interval of ~20–18 ka BP at the end of the sentence (line 19).

8. L22: To my knowledge, it is either kyrs BP or ka.

Response: we appreciate the reviewer's attention to the normative use of age units in our manuscript.

"ka BP" refers to "thousand years before present," and this abbreviation is standard and widely used in paleoceanographic and paleoclimatic studies (e.g., Max et al., 2022; Knies et al., 2018; Patton et al., 2017)[10–12].

9. L23: I think demonstrate is a bit too strong, perhaps something lighter like 'indicate', 'show' or 'suggest'?

Response: we replaced "demonstrate" with "indicate" in accordance with the reviewer's suggestion.

10. L27-28: Perhaps 'marine sectors of the Eurasian Ice Sheet' or 'marine terminating outlet glaciers of the EIS' instead of 'marine-based Eurasian Ice Shelves'?

Response: we fully agree with the reviewer's suggestion to use the proper term "marine-terminating glaciers" to describe the marine-based portion of the Eurasian Ice Sheet, as this terminology has been widely adopted in previous studies. Accordingly, we have replaced "marine-based Eurasian Ice Shelves" with "marine-terminating glaciers" in the abstract and throughout the manuscript.

11. L28: Is it possible to use another term than 'catastrophic'? It doesn't really relate to quantity.

Response: in this sentence, we intended to use the term "catastrophic" to characterize the large-scale meltwater discharge into the Nordic Seas. Indeed, the adjective "catastrophic" is only loosely related to the specific volume of meltwater. Rather, it reflects the close linkage of this meltwater event with subsequent intensive climate changes, including the pronounced decline of the AMOC and the collapse of the Laurentide Ice Sheet—both of which represent catastrophic processes that would likely cause panic in modern society. Therefore, we consider the use of the term "catastrophic" appropriate to describe this meltwater event.

12. L35: Maybe 'understanding' rather than 'elucidating'?

Response: we replaced the "elucidating" with "understanding".

13. L37: I would try to explain this better. Something like 'leading to surface melt which reduced ice thickness and contributed to lubricating the ice sheet base increasing sliding and ice discharge.'

Response: we appreciate the reviewer's suggestion to provide a more detailed explanation of the hypothesized process by which peak surface temperatures triggered ice-sheet collapse. Accordingly, we have added a description of how surface melting could have led to the collapse of the entire continental ice sheet, following Bintanja et al., (2008) and Abe-Ouchi et al., (2013)[13,14]. The revised sentence now reads: "A commonly discussed hypothesis attributes the onset of terminations to the peaks of surface air temperature induced by the insolation maxima, leading to rapid surface melt which reduced ice thickness and enhancing the basal lubrication and sliding of the dominant Laurentide Ice Sheet" (line 38-41).

14. L38: 'oversized continental ice', not sure why it would be oversized? I would

suggest removing that adjective.

Response: we sincerely appreciate the reviewer's concern about why the continental ice sheets is oversized prior to the Last Termination.

Numerous studies have argued that while the onset of terminations coincided with increases in insolation, additional preconditions were required. Specifically, Northern Hemisphere ice sheets had to undergo a prolonged cooling period and expand to their maximum critical extent[14–16], developing vast marine-based sectors over continental margins[2,9]. These ice sheets contained the excess ice volume that we refer to as "oversized ice sheets." The use of the term "oversized" originates from Bintanja et al., (2008)[13]. However, to avoid misleading readers, we have deleted the term "oversized continental ice."

15. L38: 'glacial freshwater'.

Response: we have revised the "freshwater" to "glacial freshwater" in the abstract follow the reviewer's suggestion.

16. L42: I would recommend including a time interval for this (at least the first time Last Termination appears in the text).

Response: we sincerely appreciate the reviewer's suggestion to indicate a time interval for the Last Termination when it first appears in the text. Accordingly, we have added the widely accepted interval of ~20 to 7 ka BP at the end of the sentence (line 46).

17. L42: Figure 2 appears before Figure 1, while figures should be numbered according to appearance in text.

Response: we appreciate the reviewer for pointing out the errors in the order of figure annotations. We rearranged the order of the call for figures in the text.

18. L44: 'dominated Laurentide Ice Sheet', do you mean dominating? I might leave that adjective out entirely.

Response: we apologize for the ambiguous statement regarding the Laurentide Ice Sheet in this sentence.

During the LGM, the Laurentide Ice Sheet was the largest ice sheet in the world, covering almost the entire North American continent from the Arctic Archipelago to ~37–40°N areas, with an ice volume equivalent to 60–70 m of global sea-level rise. It accounted for approximately half of the world's ice reserves and played a dominant role in Late Pleistocene fluctuations of global sea level[17,18]. To improve clarity, we revised the term from "dominated Laurentide Ice Sheet" to "dominating Laurentide Ice Sheet." (line 40)

19. L45: I think the terminology is slightly off here, and it should be 'grounding line retreat of marine-based portions of an ice sheet' or something similar.

Response: we sincerely appreciate the reviewer's comment regarding our unprofessional and ambiguous statement about the grounding line retreat.

The grounding line is the boundary between the grounded part of an ice sheet or tidewater glacier and the adjoining floating ice shelf or glacier tongue. Therefore, the appropriate subject of grounding line retreat should be the marine-based portions of an ice sheet, rather than the marine ice shelves. Accordingly, we have revised the wording to "cause the retreat of the grounding lines and the melting of marine-based portions of ice sheet." (line 48-49)

20. L45-46: perhaps 'destabilizing the continental ice sheets' instead of 'destabilizing the overall stability of'?

Response: we agree the reviewer's suggestion, and removed the "overall stability".

21. L49: there is more recent literature on this focusing on past interglacials/last deglaciation: see for instance Alvarez-Solas et al. 2019, Climate of the Past, or also Petrini et al., 2020, Quaternary Science Review.

Response: we sincerely appreciate the reviewer for providing newly published references focusing on the relationship between oceanic processes and the collapse of the Northern Hemisphere ice sheets. These references have been incorporated into our revised manuscript.

22. L49: 'northern ice sheets', do you mean 'Northern Hemisphere ice sheets'? The ice sheets are referred to inconsistently throughout the manuscript

Response: we have replaced the term "northern ice sheets" with "Northern Hemisphere ice sheets" throughout the manuscript, following the reviewer's suggestion to ensure consistency.

23. L56: 'contrast' rather than 'disparity'?

Response: we appreciated the reviewer's advice and we have replaced "disparity" with "contrast".

24. L59: it is common to capitalize Last Glacial Maximum, instead of 'last glacial maximum (LGM)', and to provide a time interval. The latter would be useful when referring to 'the later stage of the LGM' (L221) for example.

Response: we appreciate the reviewer's suggestion to capitalize "Last Glacial

Maximum” and to specify its time interval. Accordingly, we have capitalized the term and added the interval of ~26.5 to ~19 ka BP (line 64), following the study of Clark et al. (2009)[19].

25. L61: ‘sea-level fall’ rather than ‘sea-level reduction’?

Response: we have replaced the “sea-level reduction” by the “sea-level fall”.

26. L61: Not sure of what you mean with ‘forming an extensive interface with the Nordic Seas and the North Atlantic’.

Response: the statement that the Eurasian Ice Sheet formed an “extensive interface with the Nordic Seas” means the Eurasian Ice Sheet formed the large marine-terminating sectors of the ice sheet that extended into the eastern Nordic Seas, where its margin was directly connected with both the surface and subsurface waters. To be more clarity, we have revised the term “extensive interface” to “extensive ice–ocean interface.”

27. L69: Maybe better 'evolution of the upper ocean water masses at the beginning of the last termination'? 'under orbital forcing' is not completely clear to me.

Response: we accepted the reviewers' suggestion and revised this sentence as: “In the present study, we combined multi-geochemical proxy data from the southern Nordic Seas to reconstruct the evolution of upper ocean hydrography at the beginning of the Last Termination” (73-75).

28. L74: ‘early’ rather than ‘advanced’?

Response: the term "early" conveys a more precise meaning. Therefore, we replaced "advance slowdown" with "early slowdown".

29. L74: Please provide time interval for HS1.

Response: we have provided the time interval of HS1 (~17.1 to ~14.3 ka BP; line 79) according to the study of Hodell et al., (2017)[20].

30. L77-78: appears too much space between degree and minutes of the coordinates. Also, remove space in 'hereafter'?

Response: we have removed the extra space between degree and minutes of the coordinates, and also removed the space in "hereafter".

31. L94: It could make sense to give the reader an idea of what the sampling interval resolution is (although it is also explained in the Methods section).

Response: the "sampling-interval resolution" referred to here denotes the sedimentary time interval between two adjacent sampling points in our core. After calibration using the Bayesian age-modeling approach, these intervals are estimated to be ~180–720 years. To improve clarity, we have provided the specific time interval of two adjacent sampling points in parentheses after the term "sampling-interval resolution," following the reviewer's suggestion.

The revised sentence now reads: "we reconstructed the Mg/Ca-derived temperatures from individual foraminiferal shells within each sampling layer, together with their standard deviation, to investigate the magnitude of temperature fluctuations at the sampling-interval resolution (~180-720 years)" (line 98-101).

32. L136-137: 'the degree of dispersion of temperature of temperature distribution' is a bit of a mouth full.

Response: we appreciate the reviewer for pointing out our overly complicated and wordy description of temperature fluctuations. To simplified this statement, we have revised this part to "drastic increase in the temperature dispersion" (line 138-139).

33. L142: Fig. 5B is mentioned before Fig. 3 and 4

Response: We rearranged the order of the call for figures in the text.

34. L143: Maybe something like 'support our findings, showing ...'

Response: we sincerely appreciate the reviewer's suggestion. Our reconstruction of shallow subsurface warming at the core site, as stated in the earlier part of the section, only reflects regional changes in the southern Nordic Seas. In the later part, we referenced temperature records from the northeastern and mid-eastern Nordic Seas, together with the TraCE simulation results, to conclude that this warming trend was not a regional phenomenon but affected nearly the entire basin. Therefore, we cannot present this conclusion at the beginning without first providing the supporting evidence. For this reason, the statement "supporting our findings that…" may not be entirely appropriate. Consequently, we prefer to retain the phrasing "yield two key observations."

35. L170: 'latest stage of the LGM'?

Response: we have replaced "later LGM" with "late stage of the LGM" throughout the manuscript to ensure consistency and to more accurately describe the corresponding time interval.

36. L172: should it be '14C age offset' instead of '14C offset'?

Response: we sincerely thank the reviewer for pointing out our imprecise statement of "^{14}C offset". We have revised it to "^{14}C age offset".

37. L174: 'can be presumably'?

Response: we have revised the "is presumably considered as" to "can presumably be considered as" follow the reviewer's suggestion.

38. L185: Not sure of what you mean with 'local feedback mechanism to global scale climate driving forces'.

Response: we appreciate the reviewer's question regarding the meaning of "local feedback mechanism to global-scale climate driving forces." By "global-scale climate driving forces," we refer to large-scale forces that influence the global marine environment and climate system, such as orbital forcing and greenhouse gas concentrations. The "local feedback" we originally mentioned refers to the warming trend and shoaling of AW reconstructed from our proxy records in the Nordic Seas, which may represent a regional response to these global-scale climate drivers (e.g., variations in insolation). To avoid misunderstanding and improve clarity, we have replaced "local feedback mechanism" with "could have been a regional feedback mechanism" (line 195).

39. L187: Please rephrase, something like 'surface warming may have started earlier'.

Response: we appreciate the reviewer for pointing out our imprecise statement about the timing of the onset of the warming trend. To address this, we have added a specific time point and revised the grammar to provide readers with a clearer understanding of how early the warming began and its synchrony with the apparent recovery of insolation.

The revised sentence now reads: "Our reconstructed temperature standard deviations indicate that the onset of instability in shallow subsurface water mass and upper halocline layer, likely resulting from enhanced shoaling of AW, could have occurred as early as ~21 ka BP." (line 196-198).

40. L190: are these 'radiocarbon dates at 22.7 ka BP and 18.6 ka BP' calibrated? The 18.6 ka BP age corresponds to a calibrated age in Table 1, but there is no 22.7 ka. Only a 22.6 ka uncalibrated or a 21.7 ka BP that is calibrated. I suspect it should be 21.7 ka BP in the text.

Response: we appreciate the reviewer's concern regarding the accuracy of our age framework calibration and the age point cited in this section. We apologize for mistakenly reporting the uncalibrated AMS ^{14}C age of 22.7 ka BP instead of the calibrated age of 21.7 ka BP.

In addition, Reviewer 3 highlighted a newly published calibration approach for marine ^{14}C samples in polar regions using the Marine20 curve developed by Heaton et al. (2023)[21], which is more suitable for our study. Accordingly, we recalibrated our AMS ^{14}C ages following this method to establish a more accurate age framework. Furthermore, as Reviewer 3 also noted that age models based on AMS ^{14}C ages carry relatively high uncertainties, we have removed the statement that "the warming trend is systematically independent from the age model uncertainties."

41. L191: 'This'.

Response: we have revised "these" to "this".

42. L205: 'some', or 'several' model simulations (unless you want to refer to one specific simulations).

Response: we have revised the "the model simulation results" to "several model simulation results" follow the reviewer's suggestion.

43. L222: 'that enhanced surface winds'.

Response: we have replaced "the" with "that".

44. L233: 'deglacial epoch' might be confusing, as an epoch refers to the entire geological unit, i.e. either Holocene or Pleistocene. There exists no deglacial epoch, as the deglaciation lasted from the LGM to the Early Holocene.

Response: we sincerely appreciate the reviewer's guidance on the correct use of terminology "epoch". Accordingly, we have replaced "deglacial epoch" with "deglaciation stage." (line 242)

45. L252: I assume submarine melt is meant with 'heat eroding ice shelves'? I would suggest using 'melting' instead of 'eroding'.

Response: we have replaced the "eroding" with "melting".

46. L253: I don't understand why 'moisture source'.

Response: we appreciate the reviewer's question regarding why upper-ocean warming in the Nordic Seas and enhanced heat storage near the grounding line could constitute a warm moisture source. Similar to the Atlantification process observed in the Eurasian Basin, the shoaling of subducted AW may result in the thinning of the halocline and enhancing vertical heat transport to the surface, and then support the melting sea ice[22]. Compared with the surface mixed layer and sea ice, AW is considerably warmer, and as sea ice gradually declines, the barrier between the atmosphere and ocean heat weakens and eventually disappears and the seasonal open water range increased[22,23]. Consequently, the warming trend in the Nordic Seas during this interval could have enabled the upper ocean to act as a significant warm moisture source. However, to avoid misleading readers, we have deleted the term "moisture source."

47. L259: 'remain invigorated' does not sound great, as the rest of the sentence. Could you please rephrase?

Response: we appreciate the reviewer for pointing out our unreasonable expression in the second half of this sentence.

After recalibrating our age framework follow the reviewer 3 suggestion, we found that the timing of the reconstructed large-scale meltwater input and the associated collapse of the Eurasian Ice Sheet occurred at around 18 ka BP, approximately 1 ka later than previously inferred. This timing coincides precisely with the initial decline of AMOC prior to HS1, as indicated by the Pa/Th record, in contrast to our earlier results that suggested a ~1 ka lag between meltwater input and AMOC reduction. Therefore, the hypothesis of a two-step release of freshwater and icebergs from the Nordic Seas presented in our previous manuscript is no longer valid. Consequently, we have removed these statements in this section, including this sentence (line 256-269 in the original manuscript version), in the revised version of the manuscript.

48. L262: 'delay of mid latitude', maybe it is clearer as 'the delayed response of the mid latitudes'?

Response: we sincerely thank the reviewer's suggestion to provide a clearer statement regarding the delayed reduction of AMOC in mid latitude.

However, this section has been completely removed in the revised version of the manuscript (see Response 27 for details).

49. L263-266: I do not quite follow this sentence, maybe rewrite to clarify your meaning?

Response: in this sentence, we intended to describe a scenario in which massive icebergs calved from the EIS were frozen and trapped by intensified sea ice in the Nordic Seas, consistent with the 19 ka Meltwater Pulse. However, this hypothesis is no longer reasonable after the recalibration of our age framework. So, this section has been completely removed in the revised version of the manuscript (see Response 27 for details)

50. L270-274: This should be highlighted more, perhaps also in the abstract, as it is a key finding.

Response: we appreciate the reviewer's concern and acknowledgment of the cascading response of ocean circulation to the meltwater input from the EIS during this interval. In the revised manuscript, we have also incorporated the B–A ^{14}C age-offset record and the Pa/Th record to further support the influence of this meltwater input on deep-water ventilation and, respectively, the reduction of AMOC. Details see in line 283-287.

51. L283: 'buttressing'.

Response: we have replaced the "buttress" with "buttressing".

52. L290-291: you mention you 'consider the rising atmospheric CO2 to be a key factor to complete deglaciation', but not a key factor during the early EIS collapse based on the ice core record. Do the TraCE simulations (as well as Mikolajewicz et al. 2024) show that isolated greenhouse gas forcings do not play a role either?

Response: we appreciate the reviewer's concern regarding whether isolated greenhouse gas forcing played a significant role in the early collapse of the EIS and the early phase of deglaciation. In the study by Mikolajewicz et al. (2024)[1], a fully coupled atmosphere–ocean–vegetation–ice-sheet–solid-Earth model was applied to simulate transient climate evolution during the last deglaciation. However, this study did not separate the effects of greenhouse gas forcing from orbital forcing on the climate system and therefore did not directly evaluate the individual impact of rising atmospheric CO_2 on Heinrich-like ice-sheet surges around 18 ka BP.

Results from the TraCE simulations under isolated greenhouse gas forcing indicate that between 21–19 ka BP, the 850 hPa wind field exhibited only a slight increase in wind velocity, without a significant directional change toward the EIS (Fig. R1). Moreover, these increases were minor compared with the stronger changes caused by isolated orbital forcing (Fig. R1). Therefore, we consider that isolated greenhouse gas forcing did not play a critical role in triggering the initial collapse of Northern Hemisphere ice sheets during the Last Termination.

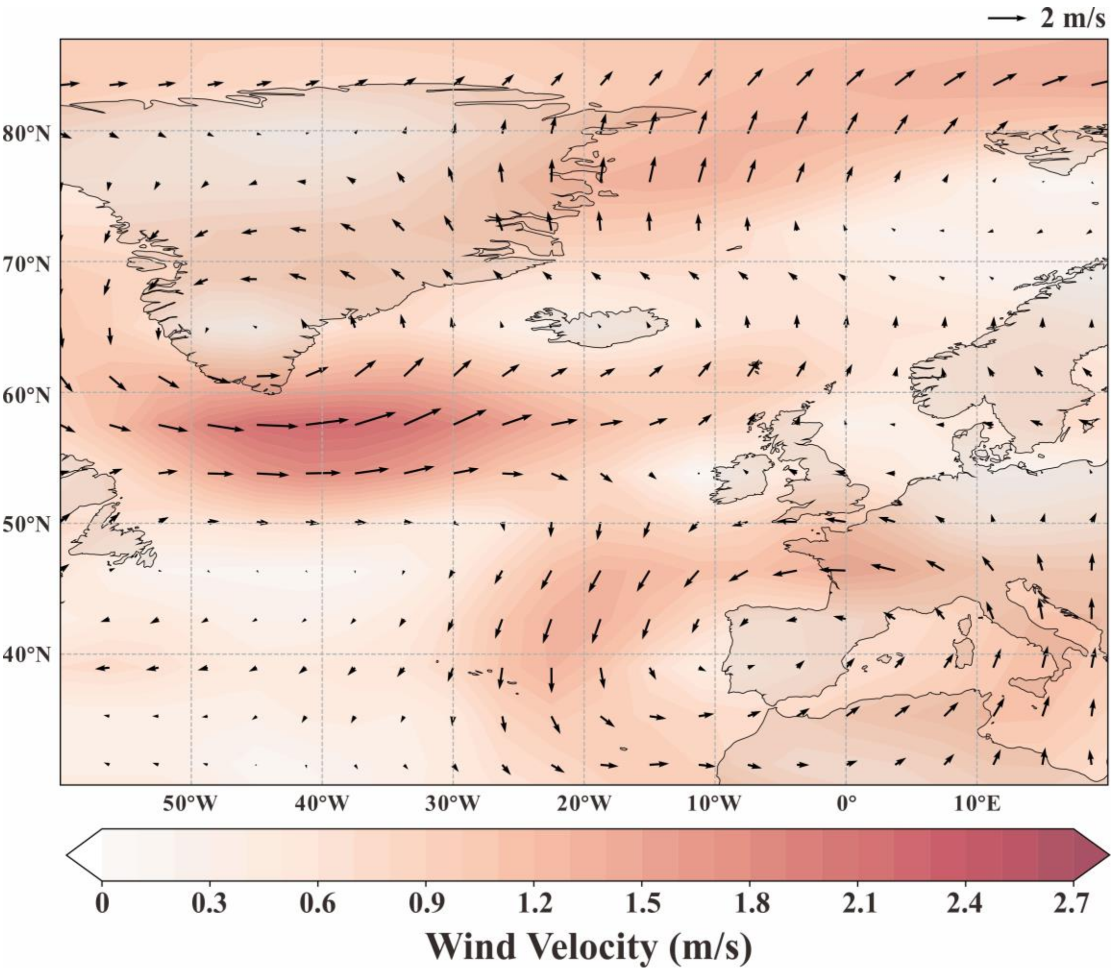


Figure R1. Winter anomalies (19-21 ka BP) of 850 hPa wind over the North Atlantic and Nordic Seas, simulated under isolated greenhouse gas forcing.

53. L293: 'boreal ice-sheet process' is a bit of a vague phrase. Do you refer to the dynamics of the Northern Hemisphere ice sheets during the early deglaciation?

Response: Yes, we indeed refer to the dynamics of the Northern Hemisphere ice sheets during the early deglaciation. However, to make the expression clearer and more concise, we have deleted this sentence.

54. Table 1: the '/a' in the column headings of the AMS radiocarbon age and calendar age can be put between brackets '(a)'

Response: we have placed the units (e.g., "cm", "year", etc.) in brackets following the reviewer's suggestion. In addition, due to space limitations, Table 1 has been moved to the supplementary materials as Suppl. Table 2.

55. Fig. 1: I would suggest considering a legend in the figure for the different point data (yellow star, white squares, black dot) so you do not need to repeat this in the figure caption. The labels for NAC and WSC are not readable.

Response: we appreciate the reviewer's suggestion to add a legend to Figure 1. However, because the figure already contains too many elements and space is limited, it was not feasible to include a legend. To simplify the figure caption, we have moved the information on the reference stations to the supplementary materials. In addition, to improve readability, we enlarged the font size and darkened the color of the NAC and WSC labels.

56. Fig. 2: I think lower case for the x-axis unit 'ka BP' is common, instead of 'Ka

BP'. It seems that the proxy records describe the last 31 ka, not 'record over the last 30 ka'? Perhaps change the location of 'NGRIP' in the figure caption of panel A: 'Stable oxygen isotopic composition and temperature from the NGRIP ice core'.

Response: we have revised "Ka BP" to "ka BP" on the x-axis of Fig. 2. In addition, we set the upper age limit of all curves to 30 ka to ensure consistency with the description in the manuscript. For both consistency and clarity, we combined the vertical axes of temperature and the stable oxygen isotopic composition of NGRIP into a single axis, with each scale distinguished by color, as suggested by the reviewer.

57. Fig. 4: There could be more information on panel A, such as whether the transect is at 9° W, and that the grey fill indicates the ocean floor. Brackets are missing around °C after Temperature for the color bar. Panel B has no variable (wind velocity?) label under the color bar. Also, what do you mean by orbital forcing only?

Response: we sincerely appreciate the reviewer's suggestion to provide more detailed information in the caption of panel A. The original temperature anomaly transect across the Atlantic was calculated as the zonal average between 40°W and 0°W. However, To more clearly illustrate the temperature variations of the upper ocean in the Nordic Seas and the North Atlantic, we have modified the temperature transect figure into regional temperature anomaly maps at different depths. In addition, we added brackets for "°C" in panel A and marked "wind velocity" in the color bar of panel B.

We apologize for the imprecise wording in the caption of panel B. The term "orbital forcing only" was intended to indicate isolated insolation forcing, and we have now replaced it with "isolated insolation forcing."

58. Fig. 5: This is a nice summary figure, but the caption lacks descriptions of the individual panels. The resolution is too low and the size too small to properly read

it. I wonder whether the DEM source for the hill shades should be referred to.

Response: we appreciate the reviewer's positive comments on Figure 5. Although the figure already contains many descriptions for the arrows and layers, we have provided more detailed explanations for the individual panels. To improve readability, we have also increased the resolution of the figure and enlarged the annotations.

As this figure primarily focuses the evolution of the upper-ocean structure and ice-sheet extent, hill shading is not essential. Therefore, we continue to use the base map from *emodnet.ec.europa.eu/geoviewer* in the revised manuscript.

59. Fig. 6: the figure caption is a bit too vague, describe the 'different ways'.

Response: we appreciate the reviewer for pointing out the imprecision in the caption of Figure 6. Following the suggestion of Reviewer 3, we recalibrated our age framework and now use only the method of Heaton et al. (2023)[21] to calibrate the ^{14}C ages; therefore, no alternative methods are involved. Accordingly, we have revised the caption of Figure 6 to "Age–depth models of core IS-1B" and added descriptions for each curve.

60. Fig. 7: there is no caption describing the individual panels A and B.

Response: we added the description and legend for panels A and B.

61. Fig. 8: I would put seconds between brackets behind time: 'Time (s)'.

Response: we sincerely thank the reviewer's suggestion, and we have put the seconds between brackets.

Responses to the suggestions and comments by Reviewer 2

We would like to express our heartfelt gratitude to the reviewer for conducting a meticulous and highly constructive assessment of our manuscript. The insightful comments provided have been invaluable, enabling us to substantially enhance both the clarity and overall quality of the paper. We have thoroughly revised the manuscript, taking into account every major and minor concern raised. We are confident that these revisions have markedly bolstered the manuscript's robustness. Below, we present the point-by-point response to each comment.

• Comment on the necessity of the proposed mechanism for glacial terminations

1. Is the proposed mechanism a necessary condition for a termination to happen? The title and conclusion somehow suggest it is. Would you expect something similar for other terminations? Could your core be analyzed in that sense? Indirectly related, the only coupled climate-cryosphere successful simulations (to a good degree) of glacial cycles are shown in the work of Ganopolski et al. (Which are not cited here) and they do not need such a mechanism. It is true that their intermediate complexity approach is probably not well suited to capture the processes at the heart of the mechanism here proposed. However, that work would suggest that an earlier demise of the EIS is not required for the termination. A small discussion on this would be beneficial for the paper.

Response: We sincerely thank the reviewer for the thoughtful and insightful comments on the climatic mechanism we proposed for triggering the initiation of the last termination. During the Quaternary, the ice sheets in the Northern Hemisphere experienced up to 41 times of termination processes. However, the precise mechanisms that triggered the rapid, large-scale collapse of ice sheets during brief warming phases after prolonged global cooling remain under active debate[15,24]. While current studies suggest that increased summer insolation at 65°N, driven by orbital variations, plays a fundamental role in initiating deglaciation, not every insolation peak leads to a glacial termination[25,26]. Many researchers have argued that several additional preconditions are required for a termination to occur, including:

(1) The Northern Hemisphere's ice sheets have to go through long-term cooling period and expand to reach their maximum critical extent[14–16]. (2) Northern Hemisphere's ice sheets have to develop vast marine-based ice sheet sectors that are more sensitive to ocean temperature and sea-level changes[2,9]. This because of the maximum isostatic depression at the global sea-level minimum, raising the equilibrium line altitude (ELA) of inland ice sheets and increasing their susceptibility to collapse[24]. (3) Minimal global precipitation, low vegetation cover, and maximum atmospheric dust, all of which contribute to weakening the hydrological and radiative balance needed to sustain ice sheets[27].

These means each glacial-interglacial cycle differs in terms of background climate conditions and ice-sheet extent; thus, the initial processes triggering each termination may not be identical.

We also compared studies on the evolution of other termination processes. For example, multiple paleoclimate reconstructions suggest that Northern Hemisphere ice sheets responded rapidly to the increased summertime solar insolation during the Termination II[15,28]. Stoll et al., 2022 pointed out that during Termination II, the rapid retreat of the marine-based Eurasian Ice Sheet, similar to the mechanism proposed in our study, was the primary source of the two stages of freshwater pulse into the North Atlantic (MWPTII-A and MWPTII-B), which in turn led to the early reductions of the AMOC and triggered the Termination II climate[29]. Moreover, the Eurasian Ice Sheet during Termination II is thought to have contained a larger ice volume in the marine-based sector than during Termination I, potentially making it even more susceptible to destabilization by oceanic processes[29]. Its subsequent melt water release could have played a critical role in altering ocean circulation and influencing the behavior of other Northern Hemisphere ice sheets in Termination II.

However, direct evidence is still lacking as to whether significant oceanic processes—such as enhanced northward heat transport and widespread upper-ocean warming close to the ice sheet margin—affected the Eurasian Ice Sheet during Termination II. Additionally, whether atmospheric and oceanic circulation patterns in the high-latitude North Atlantic evolved in the manner similar to those we reconstructed in Termination I is still unclear. Furthermore, sedimentary records near former ice-sheet margins in older terminations were often eroded or disturbed by the repeated glacial advances and retreats, making the preservation of high-resolution archives difficult.

Therefore, obtaining robust reconstructions for older terminations' evolution requires a combination of multiple proxies, multiple stations, and integration with model simulations. As a result, research focused specifically on the ice sheets evolution during older terminations is currently limited.

In our study, the mechanism we propose—highlighting how insolation may have triggered widespread Northern Hemisphere ice-sheet collapse through a chain of oceanic and atmospheric feedbacks—is specifically framed within the context of the last termination. Our proxy reconstructions (due to the core depth limit) and simulations cover only the last ~30 ka, and therefore do not allow direct conclusions about earlier terminations. Nevertheless, we hope that the insights gained from Termination I may offer valuable perspectives for understanding the initial mechanism of other terminations as well.

We also appreciate the reviewer's recommendation of the coupled climate–cryosphere simulation study by Ganopolski et al., which represents one of the few successful attempts to simulate the evolution of the last glacial cycle by using a three-dimensional polythermal ice-sheet model (SICOPOLIS). (Although the reviewer did not specify the publication year, we infer that the reference is the widely cited article published in *Climate of the Past* (Ganopolski et al., 2010)[30], titled *"Simulation of the last glacial cycle with a coupled climate ice-sheet model of intermediate complexity."*) Their results effectively captured the spatial and temporal dynamics of the major Northern Hemisphere ice sheets, including rapid glacial inception and the marked asymmetry between the relatively long ice-sheet build-up phase and the abrupt glacial termination. Notably, the study emphasized the dominant role of Milankovitch forcing in driving the retreat of the Northern Hemisphere ice sheets during the last termination, showing that deglaciation began shortly after 20 ka BP, with the Eurasian Ice Sheet collapsing earlier first—consistent with the scenario presented in our manuscript. Furthermore, their findings suggest that subsequent millennial-scale climate events (e.g., Heinrich Stadial 1) were the results of internal instabilities within the ice sheets and the AMOC. Their results showed that the ice calving plays more important role than the surface ablation in the millennial scale variability of AMOC. This aligns well with our results, which indicate that the meltwater pulse from the EIS played a crucial role in the AMOC reduction and instability of the Laurentide Ice Sheet.

Ganopolski et al.'s model emphasized the influence of surface energy and atmospheric dust on ice-sheet evolution, suggesting that a large portion of the European ice sheets disappeared before 13 ka BP, driven by increasing summer insolation and rising greenhouse gas concentrations[30]. However, their study did not focus on the role of oceanic processes and heat transport in driving ice-sheet retreat, nor did it focus on the early collapse of the Eurasian Ice Sheet at the onset of the last termination. This may be due to the ocean component of the CLIMBER-2 model, which is a relatively coarse-resolution module of intermediate complexity and does not explicitly resolve the precise impact of ocean heat transport on ice-sheet dynamics. As a result, their simulations did not fully capture the mechanism we propose in our manuscript. In addition, Ganopolski et al.'s model exhibits more pronounced ice-sheet variability on the precessional (~20 ka) timescale[30]. This may suggest that the model's relatively coarse resolution limited its capacity to resolve the short-term, rapid retreat of the marine-based Eurasian Ice Sheet in response to regional oceanic processes in the Nordic Seas.

Due to differences in model structure and resolution, Ganopolski et al.'s simulation may not have fully captured the variability in the Nordic Seas and the behavior of the Eurasian Ice Sheet. This does not preclude the existence of interactions between the ocean and ice sheets during the last termination as we described in our manuscript.

In future research, we hope to communicate with Ganopolski et al. to gain a deeper understanding of their model framework and its treatment of key processes. We also plan to compare their simulation results with those from the TraCE experiment to further evaluate the relative contributions of different climate forcings to the retreat of Northern Hemisphere ice sheets.

In summary, to address the reviewer's concern, we have also revised the title and conclusion to clarify that our findings are specific to the Last Termination and are not intended to suggest a universally necessary mechanism for all glacial terminations.

• Comment on why similar insolation increases (e.g., early MIS 3) did not trigger a termination

2. Another unaddressed question the reader may have when trying to judge your proposed mechanism is the following: Why is not the increase in insolation at the beginning of MIS3 causing a termination? Of course the EIS was not at that time as extended as it is for the LGM, but, what makes your mechanism unique in the

sense of initiating an insolation-triggered deglaciation which does not happen for other periods of the glacial cycle?

Response: we sincerely thank the reviewer for raising this important question from the perspective of the readers who may not be familiar with the glacial cycle dynamics and ice-sheet termination process. During the glacial-interglacial cycles of Quaternary, there were indeed some periods when summer insolation at 65°N increased markedly, yet the global ice sheets did not experience large-scale retreat, such as the onset of MIS3 (approximately 70–55 ka BP; Fig. R2A). This highlights a crucial point: not every period of increased summer insolation leads to widespread Northern Hemisphere ice sheet collapse and the Termination climate process.

Of course, according to Milankovitch Theory[31], orbitally driven variations in solar insolation are the primary forcing behind the termination of Northern Hemisphere ice sheets.

However, Terminations only occur after the ice sheets have experienced a prolonged period of slow growth and reached a critical maximum extent[14–16]. This threshold typically coincides with: (1) the lowest global sea level, as vast amounts of ocean water are stored in the form of ice sheet and sea ice[19,32]; (2) the expansion of major ice sheets not only over high-latitude landmasses, but also formed extensive marine-based sectors due to maximum isostatic depression over the continents[2,9,24]. (3) minimum atmospheric CO_2 concentrations, along with the coldest global temperatures[3,33].

Before the beginning of MIS 3, global relative sea level was already relatively low (around 84.5 m; Fig. R2B), but had not yet reached the lowest point of glacial period, such as that observed during the LGM (approximately -120 m)[34]. This indicates that the Northern Hemisphere ice sheets had not yet attained their maximum ice volume. In the latter half of the Quaternary, ice sheets typically require longer period to reach their full extent and the major glacial cycles became dominated by a ~100 ka periodicity. From Termination II to the MIS 3, the Northern Hemisphere ice sheets clearly had not had sufficient time to grow and accumulate the critical ice volume necessary for a termination. Moreover, although atmospheric CO_2 concentrations declined significantly during this interval (to around 210 ppm; Fig. R2D), they had not yet reached the minima observed during other terminations (around ~180 ppm)[35,36]. Therefore, at the beginning of MIS3, both ice-sheet extent and the associated global temperatures—modulated by greenhouse gas forcing were likely not enough to initiate large-scale ice-sheet destabilization and a full deglacial transition.

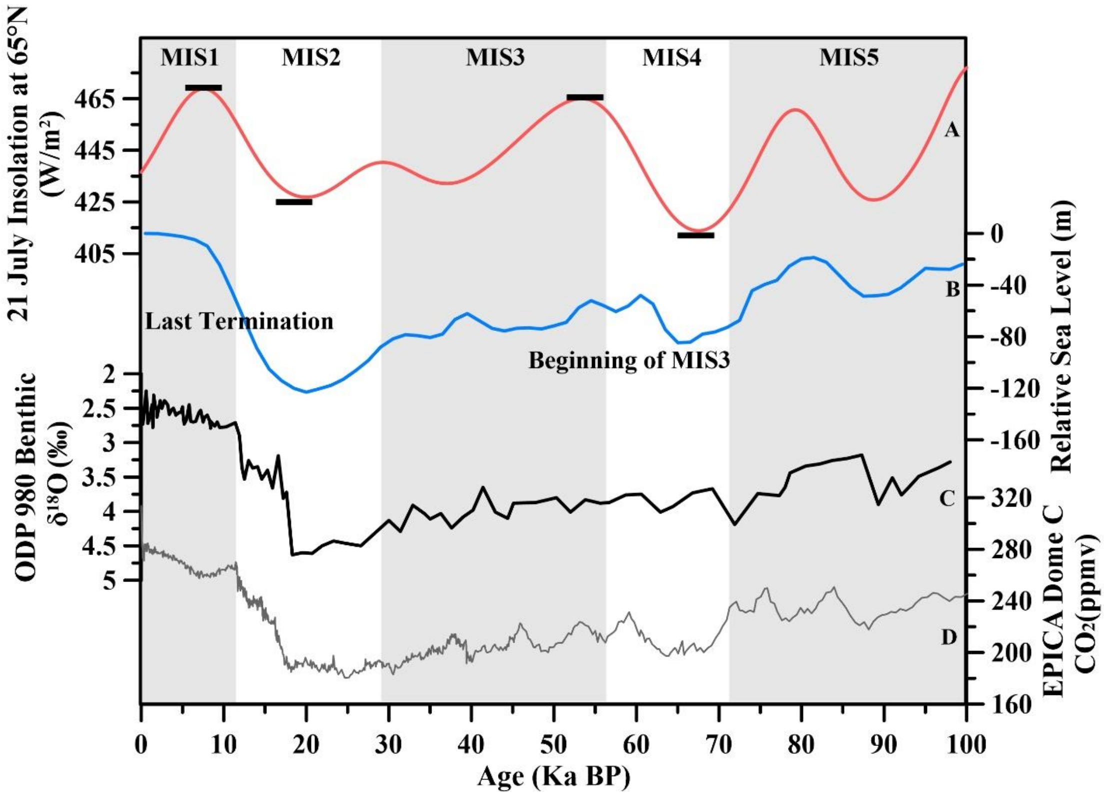


Figure R2. Evolution of the main parameters related to the Terminations

We also appreciate the reviewer's insightful question regarding what makes our proposed mechanism unique in initiating a termination—specifically, why similar insolation increases do not trigger termination during other periods in the glacial cycle. This question touches on a central issue concerning the its necessity and broader applicability of our proposed deglaciation mechanism in other termination processes. As we discussed above, the terminations of ice sheets typically occur when the ice sheets reached their maximum extension, particularly by the development of extensive marine-based sectors.

To emphasize this point, we have also noted in line 65, line 70-71 and line 262-263 of the manuscript that our proposed mechanism for the Last Termination is based on the prerequisite conditions that, after the LGM, the Northern Hemisphere ice sheets had reached their maximum extent and had developed extensive marine-based sectors.

During glacial maximum, extremely low global sea levels significantly reduce buttressing from surrounding ocean water, rendering the imbalance of the ice sheets masses, in turn making the marine-based glaciers more unstable and susceptible to oceanic forcings (such as sea temperature variations and sea level fluctuations).

Our proposed mechanism highlights the rapid response of oceanic circulation to increased solar insolation, which enhances northward oceanic heat transport toward high-latitude ice sheets. This process is more likely to lead the sudden calving and disintigration of marine-based ice sheets, rather than gradual surface melting[30]. Crucially, this mechanism may have played a key role in triggering the initial collapse of North Hemisphere ice sheet at the very beginning of Terminations—at a time when global temperatures had not yet significantly increased.

Subsequently, the massive influx of melt water into the North Atlantic weakened the AMOC, resulting in a long period cold stadial in North Hemisphere. This led to a southward shift of the ITCZ and mid-latitude westerlies, leading to a warmer Antarctic and enhancing CO_2 outgassing from the Southern Ocean via the bipolar seesaw mechanism. The resulting rise in atmospheric CO_2 pushed concentrations above the threshold required to sustain interglacial conditions, thereby creating the climatic environment necessary for continued global ice-sheet melting[24]. In this context, our mechanism suggests that the oceanic response to insolation acts as an initial trigger, setting the stage for the chain of feedbacks that ultimately drive full glacial terminations.

For other periods in the glacial cycle outside of Terminations (such as the onset of MIS 3, as the reviewer noted), the Northern Hemisphere ice sheets had not undergone a sufficiently long period of ice accumulation, nor had they developed extensive marine-based sectors with large enough ice volume. As a result, even intense oceanic perturbations during those times may not have been sufficient to trigger sudden, large-scale ice sheet retreat with meltwater discharge, and initiate the subsequent chain reactions in the global climate system.

In the current manuscript, the mechanism we provide is mainly to reveal the evolutionary process of Last Termination, and whether it is applicable to other terminations still requires more evidence for discussion. In the present study, the mechanism we propose is intended to explain the evolutionary process specific to the Last Termination. Whether this mechanism is applicable to earlier Terminations still requires further investigation and supporting evidence.

• Comment on the oceanic energy-release signal recorded in GISP2

3. Examining Figure 5 one could wonder whether such a release of energy to the

atmosphere from the LGM (panel A) to late LGM (panel B) has an impact on ice cores in Greenland. GRIP and NGRIP seem to not show anything. It might however be the case in GISP2. Also the TRACE21 simulations capture well (because of ad hoc meltwater input) the evolution of Greenland temperatures since the beginning of HS1, but they seem to have an important offset with respect to proxies at the beginning (early LGM). In the search for coherence with your current figure 5, it might be interesting to show the sea-ice extent differences between the early and the late LGM in those simulations and also expand the discussion on this matter.

Response: we appreciate the reviewer’s comments regarding the potential impact of the energy release from the warming Nordic Seas on the Greenland Ice Sheet during the late LGM, particularly in relation to signals in Greenland ice-core records that may reflect ice-sheet dynamics. Although the expansion of Greenland Ice Sheet is not as large as other North Hemisphere ice sheets like LIS and EIS, it still accumulated an excess volume of about 4.1 m ice-equivalent sea level at the LGM and extended to cover parts of the continental shelf[37]. In response to the reviewer’s suggestion, we examined the δ18O and temperature fluctuations recorded in the GISP2 ice core, which is located further south than the NGRIP core and may better capture the evolutionary characteristics of southern Greenland in Last Termination[38,39]. Temperature records from the GISP2 ice core indicate a very slight increase in surface air temperature over the southern Greenland Ice Sheet beginning in the late LGM (since around 21 ka BP, Fig. R3), possibly due to enhanced solar insolation or oceanic heat release to the atmosphere, which could have contributed to increased surface ablation. This warming may have been driven by enhanced solar insolation and/or the release of oceanic heat into the atmosphere, which could have contributed to increased surface ablation. However, neither the NGRIP nor GRIP ice cores exhibit a similar signal, suggesting that this warming may have been a regional phenomenon that did not significantly affect the overall stability of the Greenland Ice Sheet.

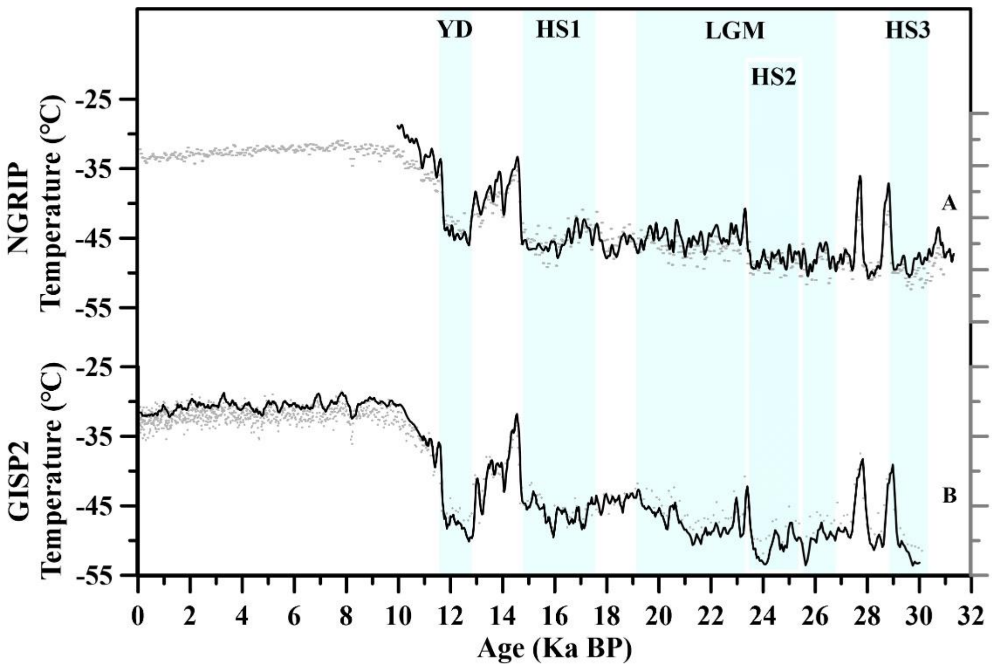


Figure R3. Comparison of temperature and δ18O records between the NGRIP and GISP2 ice cores

Previous research has established a fairly robust framework for understanding the Greenland Ice Sheet's advance since the Last Glacial Maximum, along with considerable progress in reconstructing the regional evolution.

Precise chronological constraints indicate that the initial retreat of the Greenland Ice Sheet began around 18–17 ka BP, with slight regional differences in timing between the southwestern and southeastern sectors[40], likely coinciding with the early reduction of the AMOC. But actually, model simulation results show that it eventually reached its maximum value of 4.6 m ice-equivalent sea level at 16.5 ka BP, before this timing, Greenland Ice Sheet was merely a fluctuation in advance and retreat, with less meltwater production and limited impact on the AMOC. However, model simulations suggest that the ice sheet as a whole did not reach its maximum value (4.6 m ice-equivalent sea level) until approximately 16.5 ka BP[37,41]. Prior to this time, the Greenland Ice Sheet experienced only minor fluctuations in its extent, likely resulting in limited meltwater production and a negligible influence on the AMOC.

Furthermore, the Greenland Ice Sheet also featured large parts of marine-terminating outlet glaciers within its fjord systems and ice margins, similar to the EIS and LIS. Proxy data from Winsor et al., 2015[40] suggest that the early retreat of the marine-terminating portions of the Greenland Ice Sheet was relatively gradual, with significant acceleration and large-scale collapse likely not occurring until around 12–10 ka BP.

As shown in our reconstruction results, beginning around 21 ka BP, the intensification of 850 hPa winds in a cyclonic pattern led to a pronounced east–west extension of the subpolar gyre. This primarily enhanced the eastern branch of the North Atlantic Current, directing increased heat flux toward the margins of the EIS. This suggests that the most direct oceanic heat transfer mainly impacted the marine-terminating sectors of the EIS. Even though the Atlantic Water shoaling in the subsequent millennium triggered large-scale upper ocean warming across the Nordic Seas, the western part of the basin remained influenced by the East Greenland Current, which likely acted as a barrier to direct oceanic heat transport toward the Greenland Ice Sheet. Therefore, the period of widespread warming in the Nordic Seas shown in our study likely had limited impact on the Greenland Ice Sheet. Neither the upper ocean warming nor the associated oceanic heat release into the atmosphere appears to have triggered any large-scale collapse of the Greenland Ice Sheet.

We also thank the reviewer for pointing out the mismatch between TraCE-21 simulations and proxy records during the early LGM. TraCE-21 is a transient simulation conducted with CCSM3, specifically focused on the time interval from 22 ka BP to the present. In particular, both meltwater fluxes and ice-sheet boundary conditions become better constrained only after ~22 ka BP, while earlier periods are subject to much greater uncertainties[42,43]. As such, TraCE-21 is not well-suited for simulating climate evolution prior to 22 ka BP, and the offset mentioned by the reviewer between the simulation and proxy records during the early LGM is likely a reasonable and expected outcome. For this reason, we did not attempt to simulate the evolution of the Nordic Seas and the North Atlantic during the early LGM either.

We appreciate the reviewer's concern regarding the sea-ice extent changes in Nordic Seas between the early and late LGM, as well as its potential response to the initial processes leading to the Last Termination. Following the reviewer's suggestion, we have redrawn Figure 5 to show the sea-ice extent during the early and late LGM in the Nordic Seas, based on the reconstruction of Hoff et al., 2016; Dokken et al., 2013; Müller et al., 2009 on sea-ice variability since the Last Glacial Maximum[44–46].

Müller et al and Hoff et al's biomarker results showed that a stationary margin between sea-ice cover and the open ocean existed, with a very extensive permanent sea ice cover during the early LGM weather in the Fram Strait or close to the Faroe Island from the north to south. Meanwhile, the study by Knies et al., 2018 also revealed the occurrence of polynyas proximal to ice sheets and persistent sea ice during the LGM, which likely created ice-free conditions that promoted active regional ocean ventilation and marine productivity[11]. As for the late LGM, evidence from the near the Svalbard Islands indicates the northern Nordic Seas appear to have remained under near-perennial sea-ice cover[11]. However, Hoff et al., 2016 study reveals extremely low IP25 values combined with relatively high Brassicasterol concentrations at around 19 ka BP, suggesting the presence of a perennial ice-free condition in the southern Nordic Seas[44]. This coincides with the oceanic heat release from Atlantic Water shoaling, as demonstrated in our study, which likely triggered sea-ice melt in this region. These findings are now illustrated in our revised Figure 5 and on line 188-189, further indicating that the sea-ice evolution patterns reconstructed in previous studies align well with those revealed in our results.

• Comment on ice-sheet simulations

4. Do you expect that ice-sheet simulations will react to your identified Nordic Seas warming the way you hypothesized? In my modelling experience what you suggest makes total sense, but (in the absence of ice-sheet simulations in your paper) perhaps a more complete revision of what the modelling papers of the deglacial EIS show is needed here.

Response: we sincerely thank the reviewer for his concern about whether simulated ice-sheet variations would respond well to the Nordic Sea warming we have hypothesized. We also appreciate the reviewer's positive assessment of our proposed mechanism, as supported by his own modelling experience. As the reviewer rightly noted, our manuscript would benefit from a more comprehensive discussion of the evolution of the EIS from the late LGM to the Last Termination period revealed by modeling papers. Indeed, numerous modelling studies have demonstrated that, during this interval, different sectors of the EIS underwent a phased and large-scale collapse.

Mikolajewicz et al. revealed, using a fully interactive and coupled atmosphere–ocean–ice-sheet–solid-Earth model, that the first meltwater peak of the last deglaciation occurred around 18.0 ka, caused by a surge event from the EIS[1]. This meltwater release event closely followed the upper Nordic Seas warming reconstructed from our proxy records, suggesting a potential causal link between EIS instability and the observed upper-ocean processes.

Alvarez-Solas et al. (2019), using a hybrid ice-sheet–shelf model to simulate the millennial-scale variability of the Eurasian Ice Sheet, showed that atmospheric forcing alone produces only modest iceberg discharges, whereas ocean warming leads to substantially higher discharge rates, ultimately generating pronounced meltwater peaks[2]. For specific sectors of the EIS, Petrini et al. (2020), through a perturbed-physics ensemble of ice-sheet model simulations, highlighted that rapid sub-shelf melting of the Barents Sea Ice Sheet (BSIS) occurred between 21 and 18 ka BP, exerting a strong control on simulated grounding-line discharge[9]. In particular, the collapse of the junction between the BSIS and the Svalbard Ice Sheet in the central Barents Sea was triggered by an increase in ocean temperatures, resulting in a ~65% increase in grounding-line discharge of BSIS[9,47].

Moreover, in the southern sector of the EIS, modelling and geological studies[6,48] have shown that the Norwegian Channel Ice Stream played a critical role in the separation of the BIIS and FIS, retreating between 20 and 18 ka BP through the interaction of marine-terminating ice-sheet instability processes that were highly sensitive to ocean temperature variations. This separation also triggered a massive meltwater outburst from an ice-dammed lake in the North Sea into the Nordic Seas and the North Atlantic[4,5]. Taken together, these modelling results provide compelling evidence that the EIS experienced substantial meltwater release during the onset of the Last Termination, and underscore the pivotal role of oceanic processes in initiating its collapse. Therefore, in our revised manuscript, we have incorporated additional evidences from previous modelling studies in line 265-268, thereby enriching the understanding of deglacial EIS evolution in the latest simulations and providing further evidence in support of the mechanism we propose for the initiation of the Last Termination.

• Other comments

5. The role of the Nordic Seas and the EIS in modulating millennial variability in glacial cycles has been thoroughly addressed in Dokken et al., 2013, which is not cited here.

Response: we appreciate the reviewer's reference to the outstanding work of Dokken et al., 2013 which investigated the interactions between the ocean and sea ice in the Nordic Seas during Dansgaard–Oeschger (D–O) cycles, and examined the influence of AMOC strength and warm Atlantic inflow on the upper-ocean thermohaline structure during stadial and interstadial periods prior to the LGM[45]. Their study also provided a thorough discussion of the role of the Nordic Seas and the EIS in modulating millennial-scale variability throughout glacial cycles, and emphasized the significance of the FIS as a major freshwater input source during interstadials. We fully agree with the key insights of Dokken et al. and greatly value the model they developed for the sea ice and water masses structure in the Nordic Seas during stadials, interstadials, and their transitions. In fact, our reconstruction of upper-ocean structure evolution from the late LGM to the HS1 period (Figure 5) is largely inspired by and builds upon the framework proposed by Dokken et al., which has been highly valuable to the development of our study. Therefore, we have also newly added a citation of the work of Dokken et al. to further support our reconstruction of the evolution of the upper ocean structure during the study interval.

6. The abstract and line 45 (and other places in the manuscript) read: "...thereby accelerating the ablation of the marine-based Eurasian Ice Shelves" and "...the grounding line retreat of marine ice shelves…".
This is not accurate: First, the ice shelves are marine by nature (because they are floating in the ocean), what is marine is a region of an ice sheet that lies on a bedrock below sea level. Second, what retreats is the grounding line itself, not "the grounding line of marine ice shelves".

Response: We sincerely thank the reviewer for pointing out the inaccuracy in our wording and for clarifying the precise definitions regarding ice shelves and what is the grounding-line retreat.

Following the reviewer's suggestion, we have revised the text accordingly: in the abstract, "the ablation of the marine-based Eurasian Ice Shelves" has been changed to "the disintegration of the marine-terminating glaciers of the Eurasian Ice Sheet," and in line 48-49, "the grounding line retreat of marine ice shelves" has been replaced with "the retreat of the grounding lines and the melting of marine-based portions of ice sheet."

7. Line 196 says "caloric summer insolation" while the caption of figure 3 says "Summer solstice insolation". And even the axis of figure 3 says "Caloric summer half-year insolation". Please be coherent with the naming, specify what metric you are plotting and justify which one is more appropriate and why.

Response: We sincerely thank the reviewer for pointing out the inconsistency and lack of clarity in our terminology regarding summer insolation.

The caloric summer-half year insolation we mentioned in figure 3 represents the amount of insolation integrated over the caloric summer half of the year, defined such that any day of the summer half receives more insolation than any day of the winter half[31,49]. Near 65°N, the variance of this measure has almost equal contributions from climatic precession and the obliquity. Since ice-sheet growth and decay depend both on the intensity of incoming solar radiation and the duration over which it remains elevated[31,50], we use the caloric summer half-year insolation metric in our manuscript instead of "mid-month July insolation" or "summer solstice insolation" (peak insolation index) to better represent variations in orbital energy input and their impact on Northern Hemisphere ice sheets. In the revised manuscript, we have carefully checked all occurrences and made them consistent.

8. Line 43: …preceded the episodic…" ⇒ Why not mentioning explicitly HS1 if that is what you meant

Response: We thank the reviewer for the suggestion to replace "episodic retreat of the Laurentide Ice Sheet" with an explicit description of HS1. The prevailing view is that, during HS1, the catastrophic freshwater discharge into the North Atlantic was primarily sourced from the LIS, and that this influx disrupted structure of circulation in North Atlantic, thereby reduced the strength of AMOC[51–53]. However, in the Last Termination process, the AMOC also experienced large scale slowdowns during other periods such as the Younger Dryas[54,55]. Moreover, while the LIS was indeed a dominant contributor to meltwater release during HS1, the EIS was also retreating and continuously releasing freshwater to the North Atlantic[12,56].

Our choice to state that the initial weakening of the AMOC preceded the large-scale collapse of the LIS was intentional for two reasons: (1) to emphasize that the termination process had already begun before the LIS responded to significant atmospheric warming; and (2) to highlight that other ice sheets, such as the EIS, may have an earlier respond to orbital forcing and began melting prior to the large collapse of LIS. This framing also allows us to naturally introduce the early retreat of the EIS there-in-after and underscore the role of EIS-derived meltwater in driving the early reduction of AMOC and the Last Termination climate.

9. Line 44: Dominated? ⇒ Dominating?

Response: we have changed the "dominated Laurentide Ice Sheet" as "the Laurentide Ice Sheet, the dominant ice sheet in the Northern Hemisphere" (line 46-47).

Responses to the suggestions and comments by Reviewer 3

We would like to extend our sincere gratitude for reviewers' thorough and constructive comments of our manuscript. In the comments, reviewers raised critical concerns about the accuracy of our ^{14}C age calibration and the lack of carbonate ion effect correction for the Mg/Ca-derived temperature. Following their suggestions, we have thoroughly revised the age model using the method of Heaton et al. (2023). The recalibrated age framework indicates that the apparent warming trend in the Nordic Seas was shifted from 21–19 ka BP to approximately 20–18 ka BP. The subsequent cooling phase, attributed to meltwater discharge from the Eurasian Ice Sheet (EIS), occurred around 18 ka BP—about 1 ka later than previously inferred. Despite this temporal adjustment, this warming trend remains well supported by multiple temperature records from the northern North Atlantic along the NAC pathway and from the eastern Nordic Seas near the EIS margin (Fig. 3J–N), as well as the simulation results, confirming the robustness of this warming trend.

After recalibration, the surface cooling and meltwater input at around 18 ka BP no longer coincide precisely with the 19 ka MWP, but they now align well with the well-dated timing of the disintegration of the British–Irish and Fennoscandian Ice Sheets and the collapse of the North Sea Ice Bridge, as well as the timing of obvious retreat of the Bjørnøyrenna Ice Stream and the modeled global freshwater input peak reconstructed by Mikolajewicz et al. (2025, as suggested by Reviewer #1). All of these events occurred prior to Heinrich Stadial 1. Therefore, the core mechanism proposed in our original submission remains valid: upper-ocean warming in the Nordic Seas accelerated the ablation of marine-terminating glaciers of EIS, triggered the initial collapse of EIS and associated meltwater input into the North Atlantic, and consequently led to the early reduction of AMOC before HS1.

Furthermore, by applying the method of Morley et al. (2024) to correct for the carbonate ion effect on the Mg/Ca-derived temperature, the reconstructed warming signal between 20–18 ka BP became even more pronounced (the temperature increase rose from 2.72 °C to 5.62 °C). This reinforces the reliability of our reconstruction and confirms that the inferred warming trend was not significantly affected by carbonate ion concentration or salinity changes.

Overall, after comprehensive revision, our updated proxy data and main interpretations remain consistent with our original conclusions. The revised manuscript integrates proxy evidence and model results to comprehensively investigate the role of solar insolation in driving the evolution of high-latitude ocean circulation from the late LGM to HS1, as well as the corresponding response of Northern Hemisphere ice sheets to oceanic processes during the onset of the Last Termination. We have redefined the timing of the onset of the Last Termination, moving it forward to the late LGM, and emphasized the role of oceanic heat transport and the initial collapse of the EIS as key triggers initiating the termination process. We also explored the teleconnection between the EIS and Laurentide Ice Sheets, offering new insights into the ocean-driven mechanisms underlying the rapid reduction of continental ice sheets.

In light of these substantial changes, we respectfully ask the reviewers to reconsider the revised version of our manuscript. Below, we provide a detailed, point-by-point response to each comment.

• Comment on chronology

1. Chronology

Correct age modelling over glacial interglacial periods for the Nordic Seas is not trivial. Fortunately, calibration curves have recently been updated by the Marine20 radiocarbon (14C) age calibration curve and further refined by Heaton et al (2023) for polar regions. This is important, because Heaton et al. (2023) suggest applying much larger reservoir corrections for the LGM and weaker AMOC stages such as Heinrich events. Please see also Table A2 in Heaton et al (2023) where they list estimates of the latitudinal increase in ΔR values to be applied to modern day ΔR values while still using the Marine20 curve. This is relevant in the context of the interpretation of the data presented by Wu et al.

because larger offsets will lead to younger calibrated ages and thereby place the onset of warming very close to the 19ka-MWP at 19.6-18.8 ka and even H1 (within uncertainties) rather than at the end of the LGM. Specifically, the date at cm 80-82 calibrates to 19.87-20.39 ka (2σ) instead of 21.44 22.02 ka (using Calib 8.20 and Marine20) and therefore the onset of the observed warming is in range of 19ka-MWP and very similar to observations made in Max et al. 2022. As a result, the cause-and-effect interpretation doesn't hold.

Recommended revisions: • A detailed discussion using the most recent literature on polar marine reservoir ages (the references provided e.g. Austin et al. 1995 and Klitgaard-Kristensen et al 1998 are outdated) and their uncertainties (at the 2-sigma range) is required to assess the timing of changes in the proxy record. • Recalibrate all ages using Marine20 calibration curve and the additional reservoir corrections for polar waters as suggested by Heaton et al. 2023. • Report calibrated ages at the 2 σ range. • Given the uncertainties associated with marine reservoir corrections in the Nordic Seas an independent chronological marker (ash layers) or tie points to a well dated core nearby (e.g., JM11-FI-19PC) would further support the validity of the proposed age model.

Response: We sincerely thank the reviewer for raising this important concern regarding the accuracy of our chronology framework. We fully acknowledge that this is a fundamental issue, as any inaccuracy in chronology could lead to deviations in our interpretation of proxy records and raise questions about the validity of the mechanisms we propose. The accurate calibration of marine ^{14}C samples from the high-latitude Atlantic, Nordic Seas, and polar regions over glacial and stadial periods has long been a major challenge and remains an active pursuit. The most recent marine calibration curve is Marine20 that provided a best estimate of the global-scale changes in open ocean surface ^{14}C levels over the past 55,000 years[57]. However, as Heaton et al. pointed, the original Marine20 curve is only suit for the calibration of marine ^{14}C samples arising from non-polar locations, nominally/approximately ranging from 40ºS–40/50ºN, due to the significantly increased variability in ocean ventilation and air-sea gas exchange mostly arising from changes in sea ice extent and differences in wind strength[57]. Accordingly, in our original manuscript we calibrated the radiocarbon dates using the earlier curve (Marine13 dataset), applying reservoir age corrections of 800 years for the LGM and 400 years for the deglacial–Holocene, referring calibration results from adjacent stations[58,59].

Fortunately, as the reviewer suggested, the newly refined reservoir age corrections of the Marine20 curve specific for polar regions proposed by Heaton et al. (2023) can provide a more accurate calibration framework and have already gained wide recognition[21]. According to reviewer's recalculation in the comments, after calibrated with the additional latitudinal increase in ΔR values (according to the Heaton et al. (2023) method) with Marine20 curve, our original age framework showed larger offsets which will lead to younger calibrated ages and thereby place the onset of warming very close to the 19ka-MWP rather than at the end of the LGM. We sincerely thank the reviewer for helping us to recalculate the age framework and we hold a view that the offsets observed between the two frameworks may represent the multiplicity of solutions of different calibration methods.

It is worth stressing that the approach of Heaton et al., 2023 to calibrate against Marine20 under two different scenarios for the samples from glacial periods: the low-depletion glacial scenario (oldest calendar age limit) and the high-depletion glacial scenario (youngest calendar age limit)[21]. Especially for reviewer's recalculated dating result at cm 80-82 (19.87-20.39 ka, 2σ), it was calibrated with the Holocene ΔR (ΔR_{Hol}) + 1370 (additional latitudinal ΔR increase; obtained from Table A2 in Heaton et al., 2023) and is actually the youngest calendar age limit. That's why it has so larger offset with our original dating result[21].

We do agree that the method proposed by Heaton et al. (2023) provides a significant improvement for the dating calibration accuracy in polar regions[21]. Following this valuable suggestion, we have recalibrated all of our ages using Marine20 curve with Heaton et al. (2023) approach to ensure greater accuracy and consistency in our chronology. We applied two separate age-depth models to constraint the upper and lower boundary of calendar age estimates according to the method of Heaton et al. (2023)[21]. The first age-depth model was calibrated with an uniformly Holocene ΔR_{Hol} of -86 ± 65 years for the entire core, calculated based on 10 modern ΔR values from stations located within 500 km around the core IS-1B site (http://calib.org/marine/). The second age-depth model incorporated variable ΔR values for different climatic intervals: the same ΔR_{Hol} of -86 ± 65 years for the Holocene (younger than 11500 a BP), a $\Delta R_{CS} = \Delta R_{Hol} + 1320 = 1234 \pm 65$ years for the Heinrich Stadial 1, and a $\Delta R_{GS} = \Delta R_{Hol} + 1370 = 1284 \pm 65$ years for the glacial period. All the ΔR offset of ΔR_{CS} and ΔR_{GS} were derived from the modelled latitudinal reservoir age estimates of Heaton et al. (2023) at 66.25°N[21], near the core IS-1B site.

The calendar age estimates resulting from these two models are intended to represent, respectively, the low limiting depletion scenarios and high limiting depletion scenarios. Therefore, the combination of both models brackes the maximum potential calendar age for the radiocarbon dates, encompassing the 95.4% confidence interval[21].

Due to the absence of independent constraints on whether the IS-1B core site experienced low- or high-depletion scenarios during glacial periods, and lacking detailed reconstructions of sea-ice extent in the southern Norwegian Sea, we adopted the average of the mean ages from the two age-depth models as the best estimate for the calibrated calendar ages[60] (Table R1).

Table R1. AMS^{14}C dating data and the calibrated calendar ages

Depth (cm)	**Laboratory code**	**AMS^{14}C age (year)**	**Low depletion ΔR**	**Low depletion calendar age bracketing (2σ, year)**	**High depletion ΔR**	**High depletion calendar age bracketing (2σ, year)**	**Mean calendar age (year)**
5-7	Beta-426129	1560±30	-86±65	851-1245	-86	851-1245	1048
17-18	Beta-426130	4350±30	-86±65	4149-4658	-86	4149-4658	4403
30-31	Beta-426131	6780±30	-86±65	6967-7383	-86	6967-7383	7175
39-40	Beta-497030	8550±30	-86±65	8857-9347	-86	8857-9347	9102
70-72	Beta-426133	15760±50	-86±65	18087-18619	1234	16455-17037	17537
80-82	Beta-497031	18710±60	-86±65	21502-22153	1284	19844-20439	20998
92-96	Beta-426134	22620±90	-86±65	25786-26341	1284	24261-25005	25301
125-130	Beta-426135	28010±140	-86±65	31022-31637	1284	29761-30464	30699

Our newly calibrated age model shows only minor deviations from the original age model during the Holocene, with the largest offset being less than 128 years. As for the samples in Heinrich Stadial 1 and LGM, the largest offset occurred at 70-72cm, where the new model yields ages that are approximately 1099 years younger than in the original framework. And other samples are consistently younger by about 450–700 years compared to the original model.

Furthermore, we compared our newly age-constrained $\delta^{18}O$ records of *N. pachyderma* (s) with those from the well-dated core JM11-FI-19PC (62°49′N, 03°52′W), located north of the Faroe Islands and very close to our core site, essentially at the same latitude[44]. The comparison shows that the overall patterns of $\delta^{18}O$ value variability at the two sites are broadly consistent, particularly in the occurrence of $\delta^{18}O$ peaks during the key climatic events such as Heinrich Stadial 1 and Younger Dryas, where their timing appears to be closely synchronized (as shown in the shaded grey bars in Fig. R4). This consistency further supports the reliability of our newly established age framework, as it is in good agreement with that of core JM11-FI-19PC.

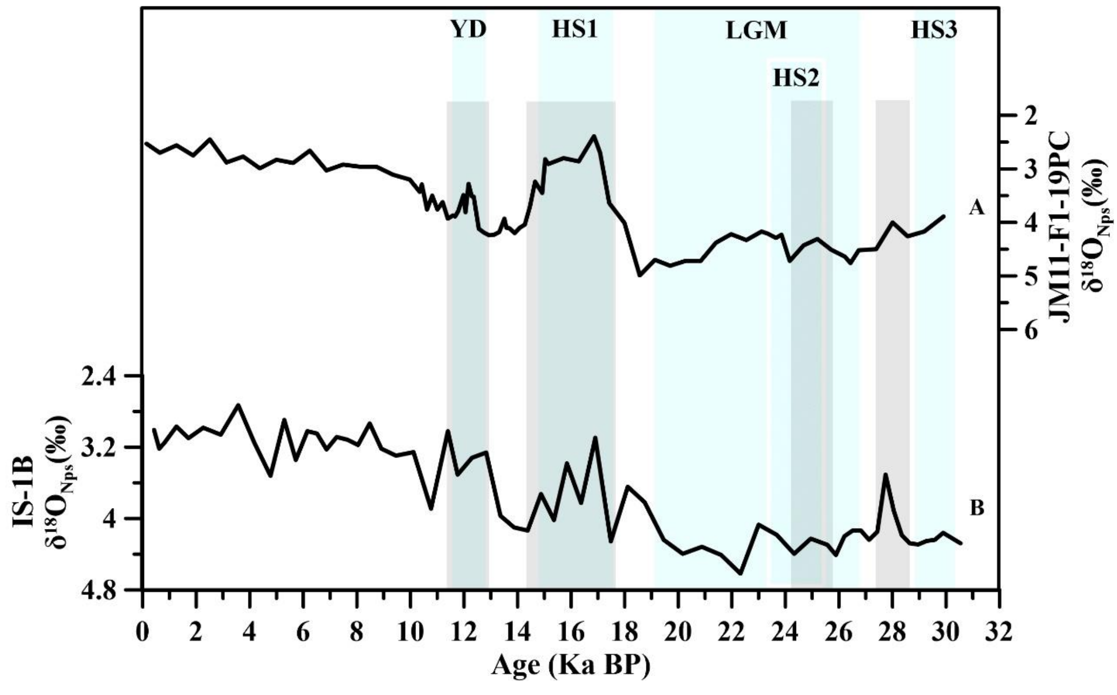


Figure R4. $\delta^{18}O$ values of *N. pachyderma* (s) from core IS-1B and core JM11-FI-19PC[61]

We also greatly appreciate the reviewer's suggestion to use volcanic ash (tephra) layers as independent chronological tie points to further support our age framework. The North Atlantic and Nordic Seas, lying close to the Iceland and Faroe Islands volcanic systems, have experienced numerous major eruptions, and their seafloor sediments preserve abundant visible tephra and cryptotephra. Among the most widely distributed and well-preserved markers since the LGM are the Vedde Ash (linked to Katla system[62,63]) and FMAZ II (linked to the Hekla–Vatnafjöll system[64,65]), deposited at approximately 12.064 ka BP and 26.74 ka BP, respectively. However, because these two tephra age control points lie fall well beyond the time window central to our study (late LGM–HS1), and no additional tephra deposits are observed in the intervening section of the core, and our ^{14}C dating control points already bracket both tephra layers, incorporating these tephra markers is unlikely to materially improve the precision of the age framework in our core. In future work, we plan to systematically extract and analyze the tephra layers throughout the core (including the older intervals beyond the range of ^{14}C dating) and integrate these with radiocarbon ages and oxygen isotope records to further refine the age framework of the core.

According to our newly established age framework, the onset of upper-ocean warming in the Nordic Seas, as indicated by our proxy records, occurred at ~20.16 ka BP, rather than close to the 19 ka-MWP or even during HS1, as suggested by the reviewer. Moreover, our results indicate that the large-scale meltwater release associated with the collapse of the Eurasian Ice Sheet took place at ~18–17.5 ka BP, immediately following the warming event. This timing is consistent with the first pronounced global freshwater input peak at ~18 ka BP simulated by Mikolajewicz et al. (2025)[1] and with the subsurface North Atlantic warming reported by Max et al. (2022)[10] for 17–18 ka BP. Thus, our revised age framework continues to support the main conclusion obtained from our original manuscript: the upper-ocean warming in the Nordic Seas during the late LGM triggered the large-scale collapse of the EIS. The resulting meltwater influx into the North Atlantic weakened the AMOC and induced widespread subsurface warming, which in turn initiated the ablation of the LIS and set the stage for the climate changes of the Last Termination.

• Comment on the LA-ICP-MS test

2. Mg/Ca proxy on N. pachyderma

There is a long history in determining the temperature signal recorded in N pachyderma Mg/Ca values. Many authors have hypothesized and demonstrated that there are clear secondary controls that affect Mg/Ca values especially at low

temperatures (Meland et al. 2006, Kozdon et al 2009, Hendry et al. 2009). Specifically, for N. pachyderma, Mg incorporation appears to be sensitive to seawater carbonate-ion concentration $[CO_3^{-2}]$, pH, and/or salinity which often covary in the surface ocean making it difficult to isolate the sensitivity to SST particularly in cold environments where $[CO_3^{-2}]$ and pH are low (Russel et al. 2004, Evans et al. 2016, Kisakürek et al. 2008, Gray et al 2018, Morley et al. 2017, Hendry et al 2009). The sensitivity and impact of $[CO_3^{-2}]$ on Mg/Ca recorded in N pachyderma specifically has recently been demonstrated in Morley et al. 2024.

Authors state that "At present, there is no consensus regarding Mg/Ca-temperature calibration methods for N. pachyderma (s) in previous related research on the Nordic Seas (Kozdon et al. 2009)." There may not be consensus, however, there has been a lot more work published on N pachyderma since 2009 that is relevant to the data presented here. In particular, it would be important to discuss findings of Livsey et al 2020 and Davis et al. 2017, since both studies attempted to calibrate Mg/Ca values to temperatures using laser ablation, the same methods as used here. Their results are not very conclusive, which raises question about the application of laser ablation for temperature reconstructions on this species. Notably Davis et al 2017 state: "A marked improvement is made in reconstructions using N. pachyderma when crust calcite is excluded, and temperature is derived from Mg/Ca ratios in ontogenetic calcite alone, using our culture-based relationships".

I would recommend that authors discuss these constraints and apply or test the recommendations made in Davis et al 2017. I would also recommend that authors clearly outline how the Mg/Ca signal was processed and integrated for each measurement. For example, it would help to show the Mg/Ca profiles and clearly outline the portions of crust and ontogenetic calcite used for signal integration (as in Davis et al 2017, Livsey et al. 2020). Also please compare the processing strategy with Livsey et al 2020 and Davis et al. 2017 to elucidate why average Mg/Ca values shown here are so much lower (!) when compared to the calibration datasets shown in both Livsey et al. 2020 and Davis et al. 2017.

Response: We sincerely appreciate the reviewer's concern regarding our Mg/Ca analytical methods on *N. pachyderma* (s) and the accuracy of calibrating Mg/Ca values to temperature. As the reviewer noted, numerous excellent studies have been published relevant to the same laser ablation technique we used on *N. pachyderma* (s) for temperature reconstructions, both in laboratory cultured individuals and in living individuals from the ocean[66,67].

The study of laboratory-cultured *N. pachyderma* (s) by Davis et al. (2017) demonstrated that the ontogenetic calcite Mg/Ca of *N. pachyderma* (s) (uncrusted or crust calcite excluded) exhibits a strong relationship with calcification temperature and is essentially unaffected by changes in pH[67]. However, in all *Neogloboquadrinids*, individuals typically precipitate an additional biogenic calcite crust prior to gametogenesis and death，and crusted shells are more likely to be preserved in the fossil record. Therefore, most *N. pachyderma* (s) extracted from sediment cores are dead individuals with crusted shells[68,69]. Furthermore, the crust composes ~40%–70% of the shell volume in crusted shells of *N. pachyderma* (s), and the thickness of the crust varies among different individuals[68]. Although Davis et al. (2017)[67] noted that the Mg/Ca ratio of ontogenetic calcite is much higher than that of the crust, the presence of heterogeneity and alternating high- and low-Mg/Ca bands within the ontogenetic calcite often cause strong fluctuations in the Mg/Ca signal during ablation analyses[66,68] (Fig. R5). This makes it difficult to precisely distinguish the boundary signals between the ontogenetic calcite and crust, and to isolate the ontogenetic calcite signals when analyzing crusted *N. pachyderma* (s). Meanwhile, Brittany et al., 2023 also point out the necessity to establish calibrations from crusted shells, as application of calibrations from Mg-enriched uncrusted shells may yield attenuated or misleading paleoceanographic reconstructions[68].

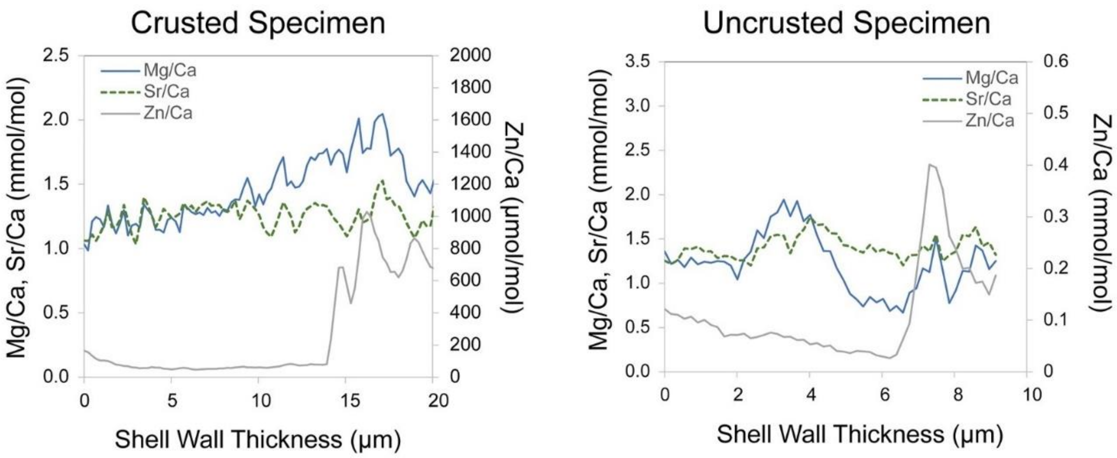
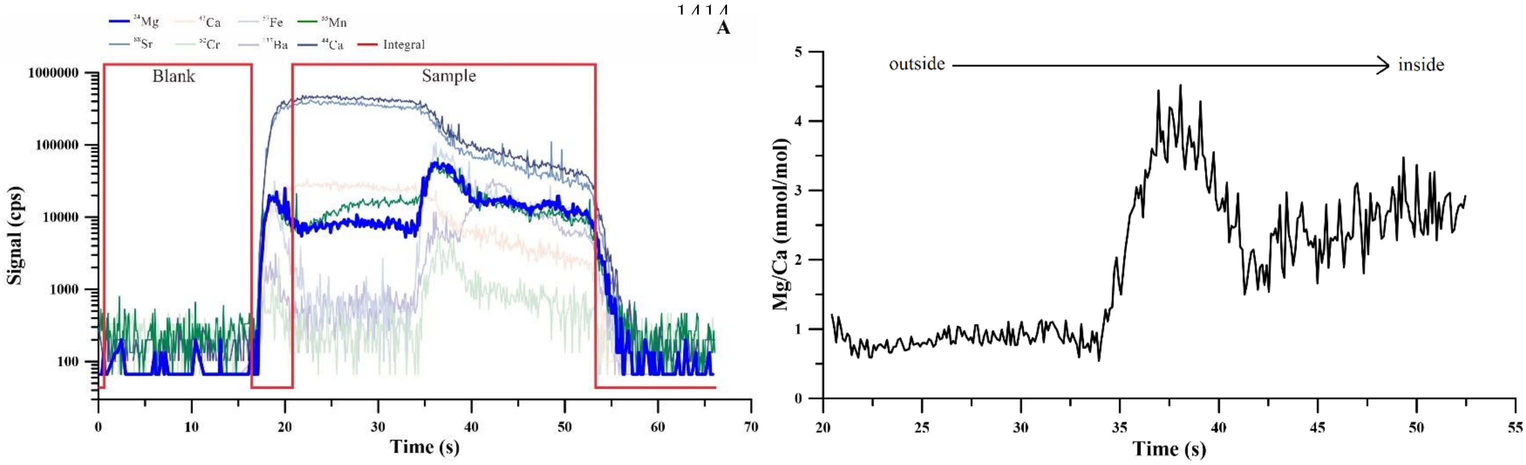


Figure R5. Example Mg/Ca depth profile for crusted specimen and uncrusted specimen[68]

Although the amount of Mg incorporated into the crust and ontogenetic calcite varies, the Mg/Ca ratios of both components are, overall, still primarily governed by the ambient water temperature[68]. Building on these considerations, our study applies LA-ICP-MS analyses to crusted *N. pachyderma* (s) specimens extracted from core IS-1B sediments, encompassing both the crust and the inner ontogenetic calcite of the shell. To clarify this, we have added an explanation in line 380 of the manuscript. Due to the frequent Mg/Ca fluctuations within both the crust and the ontogenetic calcite, and the absence of SEM micrographs for every individual shell, we did not precisely outline the portions of crust and ontogenetic calcite signals in Fig. 3R. Both our comparative tests and the results of Brittany et al. (2023) indicate that the Mg/Ca ratios among the four chambers in the individual shell of *N. pachyderma* (s) are very similar[68]. Therefore, the final chamber with a bulky and thick shell wall was selected to test the Mg/Ca, which is closer to the traditional approach of measuring the Mg/Ca of the whole shell.

The RQ ICP-MS instrument response was calibrated using SRM NIST610, 612, and 614 glasses and corrected for variations in the ablation yield and instrument drift by normalizing to the measured internal standard elements (^{88}Sr and ^{55}Mn) intensities during each mass spectrometer cycle. LA-ICP-MS data were processed using the ICPMSDataCal12.2[70,71] software. Data processing includes despiking and signal smoothing to remove outliers and removing average background counts from each data point. We manually determined the effective ablation signal interval for each sample by identifying the blank background period and the point at which the ablation fully penetrated the shell wall (Fig. R6). To ensure more stable and accurate reconstructions, we excluded the initial signal intervals characterized by elevated Mg, Fe, and Mn peaks, which are likely attributable to post-depositional diagenetic. The mean Mg/Ca for each ablation profile is then calculated by normalizing to the known trace element (^{88}Sr and ^{55}Mn) concentrations of the drift-corrected SRM standards.



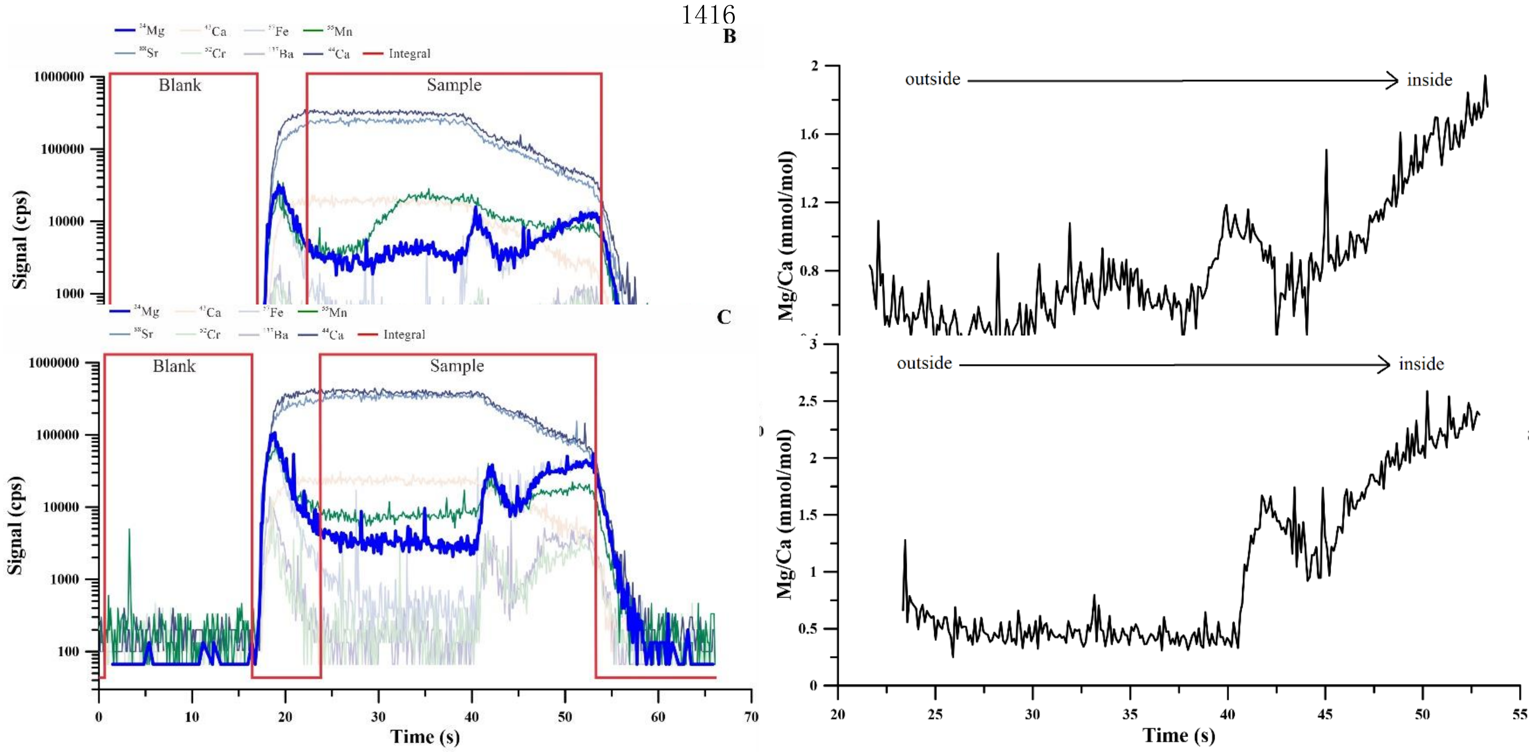


Figure R6. Typical profiles of LA-ICP-MS signals and Mg/Ca ratios. **A.** shell 32-33cm1b; **B.** shell 78-79cm 3a; **C.** shell 92-93cm 2a

We also very appreciate the reviewer's concern regarding why our Mg/Ca values are lower compared with the calibration datasets presented in Livsey et al. (2020) and Davis et al. (2017)[66,67]. The individual Mg/Ca values obtained in our tests mainly range between 0.5 and 2 mmol/mol, reflecting contributions both from the ontogenetic calcite, which exhibits higher Mg/Ca values, and from the crust, which exhibits lower Mg/Ca values. In addition, the modern summer temperature profile at our core site indicates relatively low temperatures within the calcification depth of N. pachyderma (s) (50–200 m), ranging from 3.26 to 0.57 °C.

In the study by Davis et al. (2017), N. pachyderma (s) was cultured in seawater across a temperature range of 6–12 °C, which is higher than the annual mean temperature at our core site[67]. Moreover, Davis et al. also noted that the Mg/Ca ratios in laboratory-cultured N. pachyderma were considerably higher than those in core-top or sediment-trap samples, owing to the absence of crust formation under culture conditions. This may explain why our reconstructed Mg/Ca values are lower than those reported by Davis et al.

In the study by Livsey et al. (2020), N. pachyderma (s) was collected across the Fram Strait and the Irminger Sea using plankton tows[66]. They observed that the Fram Strait specimens lacked an outer crust layer, and conducted the geochemical analyses exclusively on the ontogenetic calcite of the shell walls, which generally exhibits higher Mg/Ca ratios. This likely explains the higher Mg/Ca values reported in their Figure 3, and the markedly different Mg/Ca–temperature relationship in N. pachyderma ontogenetic calcite compared with previously published equations. For the Irminger Sea, the Mg/Ca ratios in crusted single N. pachyderma shells ranged between 0.70 and 1.95 (±0.54, 2SD) mmol/mol, which are only slightly higher than our reconstructed values. This difference may be attributed to the Irminger Sea's more southerly latitude than the Nordic Seas, where summer seawater temperatures are higher than at our core site.

By further comparing the Mg/Ca reconstruction results of crusted N. pachyderma (s) obtained from previous studies using both traditional methods (ICP-AES) and LA-ICP-MS[68,72,73], we consider that the Mg/Ca values obtained from the Nordic Seas in our study fall within a reasonable range.

• Comment on the calibration of Mg/Ca-derived temperature

3. Given these large uncertainties and lack of discussion, the choice of calibration (e.g. Nuernberg et al 1995) seems arbitrary. Simply because a core top dated to 1792 AD matches temperatures in 2018 at 50 m is not sufficient to validate a calibration equation especially since there is no supporting discussion around this and secondary controls on Mg/Ca and the issue of crust are ignored. The highly unreasonable SSS reconstructions of up to 38 psu for the Holocene further underline the need to reevaluate the calibration equation used. Furthermore, I recommend to include an explanation how uncertainties with reconstructed Mg/Ca are assessed.

Specifically, the impact of [CO3 −2] on Mg/Ca recorded in N pachyderma needs to be discussed and evaluated. Alternative calibration equations and their suitability need to be assessed. Some studies do not observe a sensitivity to [CO3 −2] (Davis et al 2017) while others do (or at least hypothesize it (Hendry, et al. 2009, Kozdon et al. 2009, Meland et al. 2006, Morley et al. 2024).

Response: We sincerely appreciate the reviewer's critical comments regarding our arbitrary choice of the Mg/Ca–temperature calibration method and our neglect of secondary controls on Mg/Ca ratios beyond temperature (in particular, the influence of $[CO_3^{2-}]$).

As noted by Morley et al. (2024), the Mg/Ca–temperature calibration for N. pachyderma (s) calcifying in SSTs below 4 °C remains under debate, as the challenge of reconstructing cold SSTs is compounded by the low sensitivity of the exponential

Mg/Ca–temperature relationship and the strong influence of secondary controls at low temperatures[74]. Indeed, many studies have observed that, in addition to temperature, seawater $[CO_3^{2-}]$ can exert a strong secondary control on Mg/Ca of N. pachyderma (s), particularly at the lower concentrations typically associated with cold SSTs[75,76] (Hendry,2009; Meland,2006). Given the relatively low summer surface temperatures at our core site, we therefore applied the method of Morley et al. (2024) to recalibrate our Mg/Ca–temperature in order to minimize the influence of $[CO_3^{2-}]$[74].

Morley et al. showed that the influence of seawater $[CO_3^{2-}]$ (Mg/Ca-$[CO_3^{2-}]$ sensitivity) on Mg/Ca-derived temperatures can be successfully quantified and isolated through integration of shell $\delta^{18}O_c$ and glacial–interglacial sea-level changes[74]. Here we use the commonly cited value of 0.11‰ per 10 m sea-level change for the correction of the glacial-interglacial sea-level variations on $\delta^{18}O_c$ [77–80].

$$\delta^{18}O_{Corr} = \delta^{18}O_c + 0.11‰ \times (SL/10) \quad \text{(Eq 1)}$$

Where SL is sea level changes derived from the curve of Lambeck et al[17]. The Mg/Ca-$[CO_3^{2-}]$ sensitivity (Mg/Ca$[CO_3^{2-}]$) for *N. pachyderma* (s) was then quantified using the Eq 2 and Eq 3[74].

$$[CO_3^{2-}] = (\delta^{18}O_{Corr} - 7.1367) / -0.0279 \quad \text{(Eq 2)}$$

$$Mg/Ca[CO_3^{2-}] = 3227.95 \times [CO_3^{2-}]^{-1.53} \quad \text{(Eq 3)}$$

The corrected Mg/Ca values (Mg/Ca$_{[Corr]}$) and corresponding Mg/Ca-based temperatures were calculated according to the suppl. Eq 4 and Eq 5, respectively[74].

$$Mg/Ca_{[Corr]} = Mg/Ca_{test} \ / \ Mg/Ca[CO_3^{2-}] \quad \text{(Eq 4)}$$

$$Mg/Ca_{[Corr]} = 0.372 \pm 0.014 \exp^{(0.0936 \pm 0.0045T)} \quad \text{(Eq 5)}$$

A comparison between the converted Mg/Ca temperature from our core-top samples and modern instrumental temperatures was conducted to evaluate the accuracy of our temperature reconstructions. This similar approach was also employed by Max et al. study to assess the most suitable calibration equation for their study site[10]. The reconstructed calcification depth of 50 m for modern *N. pachyderma* (s) at our core site yields a difference of 0.22 °C after calibration, indicating a reasonably good agreement and supporting its suitability for reconstructing temperatures from our downcore samples.

The comparison between the original and newly calibrated Mg/Ca-derived temperatures indicates that the overall pattern of temperature fluctuations during key climate events remains largely unchanged (Fig. R7). Moreover, after accounting for the influence of [CO_3^{2-}], the recalibrated Mg/Ca-derived temperatures display higher values at the peaks and lower values at the troughs, which likewise reflect the marine polar amplification in response to glacial–interglacial climate forcing[74].

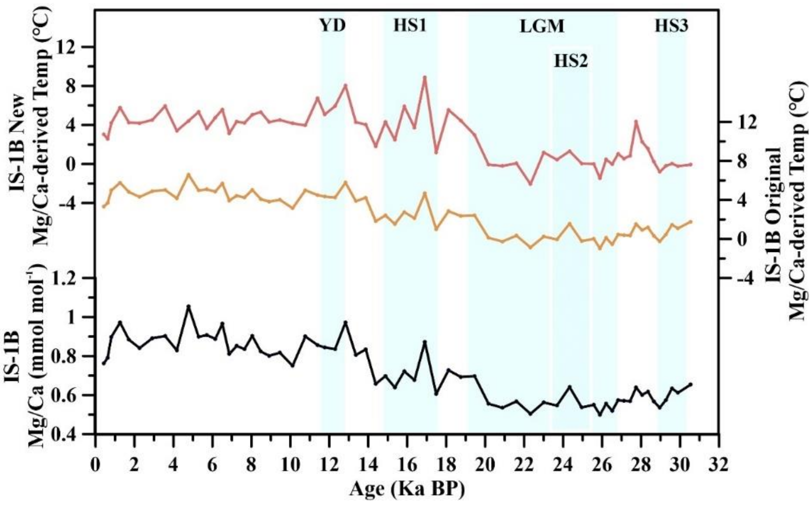


Figure R7. Comparison between the original (yellow) and newly (red) calibrated Mg/Ca-derived temperatures

• Comment on calcification depth of N pachyderma

4. Calcification depth of N pachyderma.

Authors state that N pachyderma thrive in deeper colder waters today and that therefore they must live at the cold sea surface in the past glacial ocean. This argument is faulty. Modern ecological observations of N pachyderma show that the main seasonal bloom (maximum abundance) occurs at the base of the chlorophyll max (Greco et al. 2019, Tell et al. 2022, Manno and Pavlov 2014), whether this is in the Arctic or subpolar North Atlantic. Previous hypothesis, that associate N pachyderma habitat with temperature or density are not based on modern ecological observations. Based on these modern observations, best estimates for the seasonal calcification depth of N. pachyderma is at 50m (Jonkers et al. 2022) regardless of temperature. It is therefore faulty to assume that N pachyderma is a surface ocean proxy in past glacial oceans.

Response: We are very grateful to the reviewers for pointing out our misunderstanding of the calcification depth of N. pachyderma (s). in the past glacial Nordic Seas. In recent decades, numerous studies have investigated the biological habits of N. pachyderma (s), including its hydrographic preferences, calcification depth, and bloom timing across different oceanic regions, based on laboratory cultures, plankton tows, and core-top specimens, with the aim of extracting more accurate paleoceanographic information from its shells[66,67,72,81,82]. However, there remains some controversy regarding the calcification depth of N. pachyderma (s) at present.

Simstich et al. reconstructed a west–east (~70°N) transect of modern N. pachyderma (s) δ^{18}O across the Nordic Seas and compared with the calculated δ^{18}Oc profile of the water column, showing that the species calcifies at depths of 70–250 m off the Norwegian coast beneath the Atlantic inflow, 20–50 m in the central Arctic domain, and 70–130 m in the western Nordic Seas under the influence of the East Greenland Current[82]. This means the water depth of *N. pachyderma* (s) chamber formation is related to the stratification/structure of the water column and is highly variable around the study area. In addition, some ecological observations suggest that the sea-ice presence and surface chlorophyll *a* concentration also strongly influence its vertical habitat[81,83]. *N. pachyderma* (s) shells collected by stratified plankton tow from the modern Arctic and the North Atlantic oceans show that the main seasonal bloom (maximum abundance) of this species occurs at the base of the chlorophyll max layer between 25 and 280m[81]. In order to understand the hydrographic information reflected by the biogeochemistry of *N. pachyderma* (s) shells in the core site, we reconstructed the calcification depth of *N. pachyderma* (s) in IS-1B core-top to compare with the upper ocean hydrographic structures in study area. Details of the specific methods are provided in the Supplementary Materials.

Our results indicate that the calcification depth of N. pachyderma (s) in our core-top sediments is around 50 m, consistent with the best estimates of its seasonal calcification depth reported by Jonkers et al. (2022), as noted by the reviewer[84]. Therefore, we consider that the geochemical signals preserved in *N. pachyderma* (s) calcite reflect the properties of summer shallow subsurface water mass in the Nordic Seas, and can also roughly reflect the changes in the upper part of the halocline in the study area[45,85–88].

Even though in our revised manuscript the water-mass signal represented by N. pachyderma (s) has been slightly adjusted (from surface cold water mass to shallow subsurface water mass), it still reflects the variability of the upper ocean in the Nordic Seas overall. Furthermore, considering the hydrographic structure of the modern and glacial Nordic Seas[45,87,88] together with the habitat of N. pachyderma (s)[82], the variations in Mg/Ca-derived temperature from N. pachyderma (s) in our study area can, to some extent, be interpreted as reflecting fluctuations in halocline stability and upper-ocean structure. Therefore, after redefining the hydrographic signal represented by N. pachyderma (s) on the basis of both the reviewers' comments and our core-top reconstruction of N. pachyderma (s) calcification depth, our original conclusion for the interval from the late LGM to HS1 remains unchanged: the pronounced increase in Mg/Ca-derived temperature reflects upper-ocean warming and a weakening of stratification, driven by enhanced northward oceanic heat transport and the shoaling of subducted Atlantic Water in the Nordic Seas.

• Other comments

5. The title doesn't really reflect the core interpretation proposed here. Wu et al. state that the timing for the observed change is supported by the model (e.g., increase in atmospheric circulation). However, only the time slice between 22-20 is shown and no information on the reference period is given. The presented data therefore doesn't preclude that atmospheric circulation changed earlier, for example between 24-22ka. A timeseries would be more convincing to support the proxy data.

Response: we have revised the title as "Insolation-forced oceanic changes drove the initial collapse of the Eurasian Ice Sheet and triggered the Last Termination."

The reviewer notes that our presented simulation results do not rule out the possibility that atmospheric circulation changes occurred earlier, for example between 24–22 ka BP. We would like to clarify that the atmospheric circulation changes (both at 850 hPa and at the surface) shown in Fig. 4B represent anomalies between 19 and 21 ka BP — that is, the differences in the U/V wind components at 19 ka BP relative to 21 ka BP. These therefore indicate *relative changes* within this specific interval, rather than the absolute state of atmospheric circulation throughout the 2 ka period. Accordingly, assessing whether an anti-clockwise like 850 hPa wind pattern existed during 24–22 ka BP is not directly relevant to the interpretation of Fig. 4B and may be therefore not necessary for our discussion.

Moreover, TraCE is a transient simulation conducted with the CCSM3 model, covering the period from 22 ka BP to the present. Therefore, TraCE is not suitable for investigating climate evolution prior to 22 ka BP. For this reason, we did not attempt to analyze the evolution of the Nordic Seas and the North Atlantic during 22–24 ka BP or the early LGM period.

6. 19-ff. All statements need to be carefully revised after the points raised above on chronology and Mg/ca temperature uncertainties are addressed.

Response: After recalibrating our age framework and the Mg/Ca-derived temperature record, we have thoroughly revised the corresponding statements throughout the manuscript.

7. 25-27. It is very unlikely that the NAO in its current Dipole is representative of glacial/deglacial boundary conditions. I would recommend refraining from using this terminology and instead refer to wind stress and or the position of the Jet stream in the model.

Response: We greatly appreciate the reviewer's concern regarding our use of the term of NAO to describe the phase of atmospheric modes during the LGM period.

In the modern Atlantic, the NAO is characterized by a north–south dipole structure, but under glacial boundary conditions the presence of large ice sheets substantially altered the position of the jet stream. Consequently, the existence and representativeness of the NAO during glacial periods remain uncertain[89].

Although many studies have employed the positive or negative phases of the NAO to characterize atmospheric modes during glacial and interglacial periods (e.g., Davis et al., 2009; Eldevik et al., 2014)[90,91], we have chosen to avoid potential controversy by removing all "NAO" in our manuscript. Instead, following the reviewer's suggestion, we describe the evolution of North Atlantic atmospheric modes in terms of the positioning of the westerly flow and associated wind stress.

8. 33: marked by significant…. "a" missing

Response: we added the "a" (line 35).

9. 33: "sawtooth" shape variation. Do you mean a sawtooth pattern?

Response: Yes, our intention is to describe the sawtooth-like pattern of Quaternary glacial–interglacial climate variability. Accordingly, we have revised the sentence to read: *"*a sawtooth pattern variation consistent with boreal summer insolation changes*"* (line 35).

10. 35: known as the "glacial terminations", remove "the"

Response: we have removed "the" (line 37).

11. 36: A commonly discussed hypothesis attributed the terminations - A commonly discussed paradigm attributes terminations to

Response: following reviewer's suggestion, we have revised the sentence to read: "A commonly discussed paradigm attributes terminations to the peaks of surface air temperature induced by the insolation maxima." (line 38)

12. 48: the onset of Last Termination – "the" missing

Response: we added "the". The sentence now is "the onset of the Last Termination". (line 53)

13. 50: factors – mechanisms?

Response: we changed the "factors" to "mechanisms" (line 54).

14. 55-56: through thermohaline circulation – "the" missing. But it reads awkward

Response: we added "the" (line 60).

15. 62: southern edge of EIS is situated - was situated downstream of the North Atlantic Current. Be more precise. The NAC crosses the NA ocean and turns in the Norwegian Current. The EIS would be on its Eastern border/margin.

Response: We appreciate the reviewer's suggestion to provide a more precise description of the surface current system in our Introduction. we have revised the sentence to: "The southern and western margins of the EIS were adjacent to the warm Norwegian Current, the downstream of the North Atlantic Current (NAC) and an integral part of the upper branch of the AMOC, thereby exposing the EIS to the impact from temperature fluctuations carried by Atlantic Water." (line 66-69) We have also supplemented the description of the surface hydrographic characteristics of our study area in line 84-88 and refined the representation of surface current pathways in Fig. 1.

16. 64: by the Atlantic warm water - by warm Atlantic water. The whole sentence is not clear. Clarify the use of "thereby" and "substantial impacts".

Response: we have revised the sentence to: "The southern and western margins of the EIS were adjacent to the warm Norwegian Current, the downstream of the North Atlantic Current (NAC) and an integral part of the upper branch of the AMOC, thereby exposing the EIS to the impact from temperature fluctuations carried by Atlantic Water." (line 66-69)

17. 69: under the orbital forcing. – remove "the"

Response: we have simplified the sentence to: "In the present study, we combined multi-geochemical proxy data from the southern Nordic Seas to reconstruct the evolution of upper ocean hydrography at the beginning of the Last Termination." (line 73-75)

18. 70: Processes responsible for inflow Atlantic Water (AW) oscillations – Mechanisms responsible for AW variability. AW can already be defined in line 64.

Response: According to reviewer's suggestion, we have revised the sentence to: "Mechanisms responsible for AW variability are discussed with aid of transient climate simulation TraCE, to better understand the role of insolation-driven northward oceanic heat transport on the early instabilities of the EIS." (line 75-77).

The abbreviation for Atlantic Water (AW) has been introduced earlier, at line 69.

19. 71 the role of the fluctuations of northward oceanic heat – the role of northward oceanic heat…

Response: See the revise sentence in Response 18.

20. 72: triggering the early instabilities of EIS during – remove "the" add "the"

Response: See the revise sentence in Response 18.

21. 72: during the transition period of Last Termination – prior to?

Response: See the revise sentence in Response 18. Because the previous sentence already specifies the time interval we focus in this study (line 73-75), we deleted "during the transition period of the Last Termination."

22. 73: Furthermore, we assessed the impacts of intermittent iceberg discharges from the EIS on the advanced slowdown of AMOC prior to Heinrich Stadial 1 (HS1). The evidence provided here is tenuous especially since the largest peak in IRD occurs during the YD and HS1 and not during the 19ka event

Response: We sincerely appreciate the reviewer's concern regarding our tenuous evidence supporting the hypothesis that iceberg and meltwater discharges from the EIS contributed to the early slowdown of the AMOC prior to HS1.

According to our newly calibrated age model, we infer that the peak meltwater release from the EIS occurred around 18 ka BP, as indicated by the rapid decline in upper-ocean temperature and salinity in the Nordic Seas reconstructed from our proxy records. The reviewer noted that the largest peak in our IRD record appears during HS1 rather than at 18 ka BP. However, we consider the difference between the IRD peaks at 18.5 ka BP and 16.8 ka BP to be relatively small (Fig. 2C). Moreover, the IRD concentrations display a persistent increasing trend from the late LGM onward, suggesting a progressive enhancement of meltwater input into the Nordic Seas in parallel with rising upper-ocean temperatures, and also represent the initiation retreat of the EIS from its maximum extent[19].

Further comparison of IRD and PIP25 records from adjacent stations in the southern Nordic Seas indicates that the remarkable IRD peak and the intense sea ice activity accompanied by the temperature drop all occurred around 18 ka BP (Fig. 3G, H ,I). In addition, newly published coupled transient climate simulations demonstrate that the first deglacial meltwater peak also took place at ~18 ka BP (Fig. 3E), closely linked to a surge event of the Fennoscandian Ice Sheet[1]. These lines of evidence further support our interpretation that the North Atlantic and Nordic Seas experienced substantial meltwater input from the EIS during this interval.

Therefore, we consider that we may have provided sufficient evidence for intermittent iceberg discharges from the EIS at the initiation of the Last Termination, and we can further evaluate their potential impact on the advanced slowdown of the AMOC prior to Heinrich Stadial 1.

23. 79: Faroe Current, the main branch of the warm AW inflow – is it the main branch? Please quantify Also, What about the EIC? Does it influence the site today? Potentially in the past?

Response: The North Atlantic Current crosses the North Atlantic Ocean and splits into three main branches that flow into the Nordic Seas through different gateways. The western branch, the North Icelandic Irminger Current, bypasses southern Iceland and enters the Nordic Seas through the eastern Denmark Strait; the middle branch, the Faroe Current (also referred to in some studies as the Norwegian Atlantic Front Current), flows across the Iceland–Faroe Ridge; the eastern branch, the Norwegian Current (also termed the Norwegian Atlantic Slope Current in some studies), enters through the Faroe–Shetland Channel[92–95]. Preliminary estimates based on modern instrumental observations indicate that the Atlantic Water inflow across the Iceland–Faroe Ridge amounts to approximately 3.3 Sv, with an associated heat transport of about 110 TW[94]. Consequently, Faroe Current is regarded as the main branch of the warm AW inflow.

As the reviewer questioned, does the East Icelandic Current influence our core site today, or potentially in the past? Instrumental and historical records indicate that the East Icelandic Current can occasionally affect the site when the East Greenland Current strengthens[92,96,97]. To provide a more precise description of the hydrographic setting at our core site, we have included the potential influence of the East Icelandic Current and revised the sentence as follows: "The core site lies beneath the path of the modern Faroe Current, the main branch of the warm AW inflow and an integral component of the upper branch of the AMOC. Instrumental and historical records indicate that the East Icelandic Current can also occasionally influence the surface water at this location, suggesting that IS-1B may serve as a sensitive archive for the past ocean dynamics in temperature and salinity" (line 86-88).

24. suggesting that IS-1B is a sensitive recorder of the past ocean dynamics in temperature and salinity – Rewrite to state explicitly that this is an assumption and that you are not considering an influence of the EIC contributing to the signal recorded.

Response: We have revised this sentence to clarify that it represents an assumption, following the reviewer's suggestion, and have also added the potential influence of the East Icelandic Current. The revised sentence can be found in Response 23.

25. The chronostratigraphic framework of core IS-1B was established by eight accelerator mass spectrometry radiocarbon (AMS 14C) ages spanning the past ~30 ka - refer to the method section and revise as suggested in main comment.

Response: We have recalibrated our Chronology framework follow the reviewer's suggestion. Details have been shown in Response 1.

26. as a proxy for the temperature of surface cold water mass (SSTcwm). – This

argument is faulty please also see comment above.

Response: We reconstructed the calcification depth of *N. pachyderma* (s) in our core-top sediment and, incorporating the reviewer's suggestion together with evidence from modern observations (laboratory cultures, modern plankton tows, and core-top specimens), redefined the Mg/Ca-derived temperature of *N. pachyderma* (s) as representing the temperature of the shallow subsurface water mass (SSTsub). Details have been shown in Response 4.

27. 88-90: a reference is needed to demonstrate that this approach is valid for NP at low temperatures. I have not seen this work successfully in any calibration attempt.

Response: The reconstruction of past ocean salinity using foraminifera has a long research history[98–100], and significant progress has also been achieved with N. pachyderma (s) in recent decades[82,101,102].

In this study, we used the method of Simstich et al. 2003 and Shackleton (1974)[82,103] to reconstruct the N. pachyderma (s)-derived salinity. The equilibrium relationship among the theoretical $\delta^{18}O$ values of inorganic calcite ($\delta^{18}O_c$), calcification temperature, and the $\delta^{18}O$ of ambient seawater ($\delta^{18}O_{sw}$)—originally established by O'Neil et al. (1969) and later formalized by Shackleton (1974)[103,104]—has been widely accepted and represents a good approximation in low temperature. Many studies have successfully applied this relationship to reconstruct seawater $\delta^{18}O_{sw}$ from foraminiferal $\delta^{18}O_c$ and Mg/Ca-based temperatures, for example, Meland et al. (2005) 's study[101] employed this approach to further reconstruct the LGM surface salinities in the Nordic Seas using N. pachyderma (s). Regarding the reconstructed $\delta^{18}O_{sw}$, Simstich et al. (2003) established its salinity transfer equation based on the relationship between seawater salinity across different regions of the Nordic Seas (including the Atlantic domains and Arctic domains in the central and eastern Nordic Seas, as well as the low-temperature western area influenced by the East Greenland Current), encompassing a broad range of temperatures and salinities[82]. Therefore, the method we used to reconstruct the N. pachyderma (s)-derived salinity is valid for N. pachyderma (s) at low temperatures.

28. 90-92: To further monitor the stability of surface cold water mass waters and the sensitivity of upper ocean structure in response to the oscillation in northward heat flux – what exactly is meant by upper ocean structure? Clarify. Oscillations refer to movement back and forth in a regular rhythm. I don't think that this is what is meant.

Response: The modern upper ocean of the Nordic Seas can be divided into three major oceanographic regions, separated by two oceanic fronts. The Arctic Front delineates the boundary between the Atlantic Domain, controlled by the North Atlantic Drift/Norwegian Current, and the transitional Arctic Domain. And the Arctic Domain is separated from the Polar Domain, which is under the influence of the East Greenland Current, by the Polar Front[88,105]. Therefore, the modern upper ocean of the Nordic Seas is primarily shaped by the interplay between warm Atlantic inflow and cold polar waters. It is noteworthy that, although both the Atlantic inflow (e.g., the Norwegian Current) and the East Greenland Current are surface currents, their influence and water-mass signals can extend well below the surface—to depths of over 600 m and 200 m, respectively—affecting the subsurface ocean[92,94]. For this reason, we use the term "upper ocean" to encompass the evolution of these water masses.

In the western Norwegian Basin, where our core site is located, the vertical structure of the modern upper ocean typically consists of surface mixed layer, Atlantic Water layer, thermocline, halocline, and the Norwegian Sea Arctic Intermediate Water[87,88,94]. During stadial periods, the upper ocean was additionally influenced by sea ice and meltwater, which introduced a fresh surface layer and a shallower and thicker halocline[106,107]. Even during the LGM, some studies suggest that the Atlantic Water layer subducted and persisted at subsurface depths beneath the cold halocline[108,109].

These observations indicate that the composition and vertical distribution of water masses in the upper Nordic Seas varied substantially over glacial–interglacial cycles, primarily controlled by the Atlantic Water, meltwater input, and the brine water from sea ice formation.

Overall, reconstructing the upper-ocean structure of the Nordic Seas provides valuable insights into fluctuations of Atlantic inflow and meltwater events, while also reflecting the degree of stratification, the intensity of surface convection and deep-water formation through the stability of the halocline.

Specifically, at the end of the LGM, the upper ocean structure of the Nordic Seas appears relatively stable, consisting of a fresh surface mixed layer, a halocline, a thermocline (which in some areas was absent or overlapped with the halocline), and an underlying Atlantic Water layer from shallow to deep. During the transition from the stadial to the interglacial stage, the upper ocean may additionally be influenced by Atlantic Water inflow and meltwater, leading to a less stable and more variable structure[45]. We also thank the reviewer for pointing out the inappropriate use of the term "oscillations." Accordingly, we have revised the sentence to read: "To further monitor the variability of shallow subsurface water mass and the stability of upper ocean structure in response to the fluctuations in northward heat flux." (line 96-98).

29. 92-94: If valid (see main comment) state uncertainties with reconstructions. Taking into account the high variability present in the dataset.

Response: we accepted reviewer's suggestion and recalibrated the Mg/Ca-derived temperature to remove the impact of seawater carbonate-ion with the method of Morley et al. 2024[74]. Therefore, we also recalculated the Mg/Ca temperatures of individual foraminiferal shells within each sampling layer and their standard deviation.

30. Fig 1: studied cores? Why plural?

Response: we have corrected "studied cores" to "studied core" in Fig. 1

31. 109: In the Nordic Seas, the interaction between inflowing warm AW and fresh meltwater through convection is closely linked to regional climate and sea ice behaviour. – interaction through convection? Please clarify exactly what is meant. Also, the reference (Ezat et al. 2014) does not support this statement.

Response: We sincerely thank the reviewer for highlighting our ambiguous expression in the discussion. We have revised the sentence as: "In the Nordic Seas, the evolution and spatial extent of inflowing Atlantic Water are closely linked to the sea-ice dynamics and meltwater input controlled by regional climate." (line 112-113) In addition, we have added a reference to Dokken et al. (2013) to support this statement[45].

32. 111: indicated that the AW never ceased – remove "the"

Response: we have removed the "the".

33. 113: below the thick halocline – change "the" to "a". Also, how do you know that there existed a thick halocline over the core site during the LGM? The references provided do not support this statement.

Response: In the modern western Nordic Seas, typically ice-covered regions are characterized by a strong halocline that separates the cold, relatively fresh surface water from the underlying intermediate water column[87]. During the LGM, the southern Nordic Seas were affected by intensive sea-ice activity, and numerous studies have revealed the development of a pronounced halocline and the ice cover blocked heat release and upward energy transport from the subsurface Atlantic Water to the surface and atmosphere[110,111]. Thus, the presence of a halocline above our core site during the LGM is well supported by previous work.

In response to the reviewer's suggestion, we have revised this sentence to: "the warm AW never ceased to flow into the Nordic Seas during the glacial period, but rather subducted to the subsurface and intermediate layers below the halocline and sea ice" (line 114-116). And we also added the reference of Rasmussen et al, (2004) and Dokken et al, (2013) to support this statement[45,110].

34. 113-114: Our new SSTcwm record from the core IS-1B show relatively low values over most of the LGM. – (1) Mg/Ca based SSTs frequently reach values below -2C which is impossible and raises the question whether the calibration equation used is adequate. (2) clearly defined uncertainties are essential. (3) These low SSTs also contrast with SSTs reconstructed from Mg/Ca in Ezat et al. 2017, that show SSTs near 4C prior to the termination. Similarly, Thornalley et al. 2011 report Mg/Ca based SSTs measured on NP of 4 C at (Southeast of Iceland at Rapid 15-4p) for the LGM. Neither study reports or supports a temperature increase after 21.1 ka.

Response: We sincerely appreciate the reviewer's concern regarding the relatively low Mg/Ca-derived temperatures during the LGM. Our Mg/Ca analyses of *N. pachyderma* (s), performed with the RQ ICP-MS, were calibrated using SRM NIST 610, 612, and 614 glasses to correct the variations in ablation yield as well as uncertainties associated with instrument drift by normalizing to the measured internal standard elements. Following this, we applied the method of Morley et al. (2024)[74] to recalculate Mg/Ca ratios into carbonate-ion-effect-corrected temperatures and the corresponding calibration uncertainties were also calculated from the calibration equation and are presented in Fig. 2F.

The reconstructed core top temperature estimate, reconstructed using the newly calibrated Mg/Ca-derived temperatures is consistent with the modern sub-surface temperature at the site. The newly calibrated record indicates that during the LGM, the average temperature was 0.23±0.43 °C, with only one extreme cold value below –2 °C, while most data points remain above 0 ℃. According to the recalibration work of Morley et al. (2024)[74], which compiled results from cold-temperature regions, many core-top, plankton tows, and sediment trap collections of *N. pachyderma* (s) show calibrated calcification temperatures below or close to –2 ℃. This suggests that our calibrated Mg/Ca-derived temperatures for the LGM are reasonable.

The reviewer notes that the Mg/Ca-derived temperatures reconstructed by Ezat et al. (2017) prior to the Last Termination are around 4 ℃[112], which are higher than our results. This difference may be explained by the core location: core JM11-FI-19PC (62°49′N, 03°52′W), located in the Faroe-Shetland Channel, lies further south than our site (65°36′N, 8°59′W), directly beneath the Norwegian Current. This current is stronger than the Faroe Current, making JM11-FI-19PC more strongly influenced by warm Atlantic waters. As for the Mg/Ca based temperature reported by the Thornalley et al. 2011[113], the core site of RAPiD-15-4P (62°17.58′ N, 17°08.04′ W) is in the south of Iceland and farther south than our core site. This location was likely less affected by extensive sea-ice activity and cold currents in the Nordic Seas, which may explain the relatively higher reconstructed temperatures. Furthermore, Thornalley et al. (2011) only reconstructed Mg/Ca temperatures since 21 ka BP (the later stage of the LGM), lacking the early to mid-LGM temperature variability. So, the potential differences with our results in the early and mid LGM are difficult to quantify [113].

The reviewer point out that neither of the two cited studies reports or supports a temperature increase after 21.1 ka. According to our newly calibrated age framework, the pronounced temperature rise recorded in our data began at ~20 ka BP. In the record of Ezat et al. (2017), however, a distinct warming peak is also observed around 20 ka BP, followed by a decline prior to HS1, which is broadly consistent with our results[112]. The Mg/Ca-based reconstructions of Thornalley et al. (2011) do not extend beyond 21 ka BP, making it difficult to assess whether temperatures during the later LGM were significantly higher than those of the early to mid-LGM[113].

Actually, multiple sea-surface temperature reconstructions from the Northeast Atlantic—along the pathway of the North Atlantic Current—and from sites on the eastern Nordic Seas margin near the Fennoscandian and Barents–Kara ice sheets also document this warming event (Fig. 3J, K, L, M). Taken together, these records indicate that the warming observed since the later stage of the LGM is traceable from the heat source in the North Atlantic to the sink in the Nordic Seas, lending our interpretation reliable.

35. 114: However, an intriguing temperature increase of 2.72°C after 21.1 ka BP – this statement needs to be revised after recalibrating 14C dates (see comment above) and uncertainties at the 2-sigma level for time references need to be shown. I suspect these revisions will place the onset of the warming very close or at least within uncertainties of the termination at 19-20 ka

Response: We sincerely appreciate the reviewer's concern regarding the uncertainties in both the timing and magnitude of the temperature increase described in this sentence. Following the reviewer's suggestion, we recalibrated the age framework and the Mg/Ca-derived temperatures. The results show that both the onset timing and the extent of the temperature rise were revised. Thus, we have modified the sentence as follows: "However, an intriguing temperature increase of 5.62 ± 0.57 ℃ after 20.16 ka BP marks a distinct shift from the previous interval characterized by weak fluctuations near the freezing point, indicating the onset of shallow subsurface warming in the southern Nordic Seas during the late stage of the LGM". (line 117-120)

36. 116: supporting the notion of upper ocean warming in the southern Nordic Seas during the late stage of the LGM. – (1) clarify what is meant by upper ocean warming. is this warming from the subsurface? Or from the surface? (2) given that NP are not a sea surface dweller even in the coldest environments (Grecco et al. 2019) the temperature recorded may not reflect the surface.

Response: Following the reviewer's suggestion, we reconstructed the calcification depth of our core-top *N. pachyderma* (s) (see details in Response 4) and redefined the Mg/Ca-derived temperature of *N. pachyderma* (s) as representing the temperature of the shallow subsurface water mass. The revised sentence is provided in Response 35.

37: 117: reconstructed SSS are outside the range of possible values especially for the Holocene with values reaching 38 psu! Similar to Mg/Ca this raises concern about the suitability of the methods and calibration equations used. Also, is it not misleading to show SSS without uncertainties? and to omit the variability per sample that would be introduced via the large range in Mg/Ca values from each individual?

Response: We appreciate the reviewer's concerns regarding the extremely high SSS reconstructions, particularly for the Holocene, as well as the lack of reported uncertainties in the SSS estimates. After recalibrating the Mg/Ca-derived temperatures, we used the updated temperature record to recalculate the $\delta^{18}O_{sw}$-derived salinities and redefined them as representing the salinity of the shallow subsurface water mass at our core site.

Our newly calibrated salinity results show that the reconstructed SSS_{sub} values are lower than the original SSS estimates during the Holocene, with the maximum salinity remaining below 36 PSU.

During the LGM, the SSS_{sub} values are generally lower, with some intervals falling below 32 PSU. Given the inherent limitations of $\delta^{18}O_{sw}$-derived salinity reconstructions and the uncertainties associated with applying this method across different ocean regions, some degree of uncertainty in the reconstructed values is unavoidable. Therefore, in our study, the reconstructed SSS_{sub} primarily serves to capture the variability and trends in salinity rather than to determine absolute values. In addition, we also calculated the uncertainties of the newly calibrated salinities, which are presented in Eq. 5.

For the salinity reconstruction of each sample layer, we used the mean Mg/Ca-derived temperature from 12 individual shells, after excluding abnormally high and low values, to calculate the $\delta^{18}O$ of ambient water. Thus, the reconstructed salinities are less affected by the variability of individual shell temperatures.

38. 118: revealed that the saltier surface cold water mass may have reduced the surface salinity gradient and stability of the halocline. (1) there is little evidence that NP would reside in the cold and fresh halocline given modern observations they thrive below this layer just below the Chl max. (2) warm water intrusions below the surface would destabilize the water column via cabbeling, which would also induce mixing.

Response: At present, there are multiple views regarding the preferred water-mass habitat of *N. pachyderma* (s). The reviewer notes that modern ecological observations show the main seasonal bloom of *N. pachyderma* (s) occurs at the base of the deep chlorophyll maximum layer, with the best estimates of its calcification depth being around 50 m. We fully agree with this interpretation, as it is supported by many recently published studies. The reviewer also argues that there is little evidence for *N. pachyderma* (s) residing within cold, fresh halocline waters. However, several investigations of *N. pachyderma* (s) habitats in the Nordic Seas suggest that this species often favors the nutrient-enriched and well-developed halocline beneath the East Greenland Current[82] and the strong halocline of the western Fram Strait[114].

Based on our reconstructed calcification depth of core-top *N. pachyderma* (s) and modern hydrographic profiles, we find that *N. pachyderma* (s) in our core-top samples calcified in the upper part of the halocline (Suppl. Fig 1). In fact, the two perspectives—that *N. pachyderma* (s) calcifies below the chlorophyll maximum layer and that it calcifies within the halocline in certain regions of the Nordic Seas—are not mutually exclusive. In many parts of the Nordic Seas, the upper water column develops a strong and thick halocline[87,88] the depth of which often overlaps with that of the chlorophyll maximum layer. Especially during the glacial period, which is the focus of this study, intensive sea-ice processes were typically associated with a shallower and thicker halocline[106,107]. By analogy with the modern Nansen Basin—where having a core of Atlantic Water overlain by 80-90% summer sea-ice coverage—a halocline up to ~200 m thick likely developed in the glacial Nordic Seas[23,115], encompassing both the chlorophyll maximum layer and the calcification depths of *N. pachyderma* (s). Consequently, the signals recorded by *N. pachyderma* (s) in the glacial Nordic Seas can, to a large extent, be interpreted as reflecting variations in the halocline.

In addition, the reviewer suggests that the warm water intrusions below the surface would destabilize the water column via cabbeling, which would also induce mixing. We agree with this view. The Cabbeling process occurring at the upper and lower interfaces of the halocline can enhance its mixing with the cold, fresh surface waters and the warm, salty Atlantic Water, which in turn reduces its thickness and stability. In certain regions of the glacial Nordic Seas, cabbeling-driven mixing produced denser waters that tended to sink, causing regional halocline disruption and promoting heat exchange between the cold surface layer and the underlying Atlantic Water (AW). Moreover, the strengthened vertical mixing can also reduce the salinity gradient of both side of halocline, consistent with our statement in the manuscript.

Furthermore, similar to the modern observation in the modern Eurasian Basin, subducted AW can also influence the halocline and overlying cold surface waters through double diffusion and turbulent mixing. Acting together with cabbeling, these processes may ultimately lead to regional or large-scale thinning or collapse of the halocline, promote AW shoaling, enhance vertical mixing and heat transfer, and initiate the upper ocean warming.

39. 136: This drastic increase in the degree of dispersion of temperature distribution indicates that temperature inside the water mass varied frequently over a wide range. (1) what is meant by “inside the water mass” (2) do you mean that NP habitat was characterized by a large Temperature gradient? If so, does this not contradict with l. 118 where authors imply a destabilized water column

and hence increased mixing. Would this not homogenize NP habitat range rather than increasing the T gradient?

Response: We sincerely appreciate the reviewer's concern that our statement regarding the standard deviation of single-shell Mg/Ca temperatures might lead to misunderstandings among readers.

In this study, we used the 12 individual shell Mg/Ca temperatures of every sampling layer to calculate the standard deviation of temperature in the sampling resolution (representing around 180-720 years). Based on our reconstruction of the calcification depth of *N. pachyderma* (s) and modern observations, as the reviewer previously noted, we consider the Mg/Ca-derived temperatures of *N. pachyderma* (s) to reflect the conditions of the shallow subsurface layer beneath the chlorophyll maximum, as well as variations in the upper part of the halocline. Since each individual shell Mg/Ca temperature reflects the ambient water conditions at the time and depth of calcification, the resulting standard deviation provides an overall measure of the variability and amplitude of temperature fluctuations in the shallow subsurface layer (approximately 50 m) within the sampling time interval.

Similar to early observations in the Eurasian Basin since the 1950s—showing no pronounced warm anomalies of inflowing AW until the 2000s, with a relatively fresh surface mixed layer and a stable cold halocline (50–150 m) overlying the AW[116,117]—the upper halocline and surface mixed layer of the glacial Nordic Seas were generally cold, with only weak temperature fluctuations[23]. Correspondingly, our reconstruction also shows that both the temperature and the standard deviation of single-shell Mg/Ca temperatures in the shallow subsurface layer were relatively low.

In the past two decades, instrumental observations have documented a phenomenon termed "Atlantification" in the eastern Eurasian Basin, characterized by a pronounced decline in sea ice, a weakening of the halocline, shoaling of the intermediate-depth AW layer, and enhanced winter ventilation[22,118,119]. This analogous situation with the thinning of the halocline and shoaling AW layer in the Nordic Seas, leading to the upper ocean warming, is also evident in our reconstruction since the late stage of LGM. Thus, the ongoing Atlantification of the Eurasian Basin provides a useful analogue for understanding changes of the water mass of the Nordic Seas during the late LGM.

Observations of moorings cross the eastern Eurasian Basin in the 2002–18 time series show that the base depth of halocline are continues increase from ~180m in 2002 to ~80m in 2018, suggests the rapid thinning (or even absence) of the Arctic cold halocline layer as the warm AW gradually shoaling[22,23]. Meanwhile, accompanied by the warming of the AW layer, the interannual and decadal scale temperature fluctuations in both the shallow subsurface layer (65-140m) and the surface mixed layer gradually intensified since the 1980s, along with rising summer peak temperatures[23,117]. This suggests that the intrusion of AW across the halocline and the associated upward heat transfer increased the temperature standard deviation of the upper halocline and shallow subsurface layer.

Between ~24 and ~20 ka BP, our reconstruction shows that both the temperature and the standard deviation of single-shell Mg/Ca temperatures in the shallow subsurface layer of the southern Nordic Seas remained relatively low, indicating that the upper halocline and surface mixed layer were comparatively stable (Fig. R8A). After ~20 ka BP, the shoaling and warming of AW led to halocline thinning and weakened stratification, exerting an upward influence on the shallow subsurface layer, as evidenced by increases in both temperature and temperature fluctuation (standard deviation)—a pattern resembling the modern evolution of the eastern Eurasian Basin (Fig. R8B). Nevertheless, even at the peak of this warming stage, IRD and IP_{25} records reveal that (Fig. 3G, H, I), unlike the modern eastern Nordic Seas, sea ice did not fully disappear, nor did the surface warm current entirely occupy the upper ocean and the surface remained only partially mixed (Fig. R8C). This indicates that persistent sea-ice activity and meltwater input remain continued to affect the upper ocean, together with the influence of AW, thereby sustaining relatively high temperature fluctuation (standard deviation) during the peak warming phase.

[Figure Redacted]

Figure R8. Cartoon of the upper ocean water masses evolution at IS-1B core stie Redrawn based on Polyakov et al., (2017, 2020)[22,23].

We also appreciate the reviewer's comment regarding our unclear description of the information conveyed by the standard deviation of shell temperatures. Indeed, our previous statement, particularly the phrase "inside the water mass," may be prone to cause to misunderstandings. As noted above, the temperature signals of *N. pachyderma* (s) in our core sediments primarily reflect conditions in the shallow subsurface layer (approximately 50 m), rather than within a broadly defined water mass spanning a large depth range. We have revised the sentence to "This drastic increase in the temperature dispersion indicates that temperature within the shallow subsurface layer varied more frequently and across a wider range on centennial timescales." (line 138-140).

Moreover, as the reviewer noted, we did not intend to suggest that the habitat of *N. pachyderma* (s) was characterized by a large temperature gradient. Rather, we consider that the increase in standard deviation reflects a shift from relatively narrow to wider ranges of temperature variability, accompanied by more frequent fluctuations, within the shallow subsurface layer (the habitat of *N. pachyderma* (s)). The increase in the temperature gradient would not produce strong temperature fluctuations; instead, it would indicate a greater temperature difference between the shallow subsurface layer and the underlying AW, as well as enhanced isolation from AW. By contrast, as shown in modern observations from the eastern Eurasian Basin, the upper halocline exhibits negligible vertical temperature gradients when Atlantification intensified, rather than pronounced gradients[23]. Thus, the absence of an enhanced temperature gradient is not contradictory to our earlier inference that the occurrence destabilized halocline in Nordic Seas, influenced by the underlying AW and promoted vertical mixing since the later stage of LGM.

40. 138: Compared to the growth of average temperature, the frequent variability of individual shell temperature signals most probably indicates instability of the cold water mass. (1) If the water column was instable and mixed throughout, then the temperature gradient would be getting smaller not larger. This does not explain the increase in Mg/Ca variability. (2) According to Ezat et al. 2014 AW never stopped flowing into the Nordic Seas during the LGM. Therefore, the T gradient would have been steep throughout the late glacial period with a cold surface and warm subsurface. Therefore, one might expect high Mg/Ca variability in this scenario. Not when warm waters upwell to the surface and homogenize the water column.

Response: we appreciate reviewer's comments about our interpretation of temperature variations and water-column evolution in the upper Nordic Seas. As we state in Response 39, we did not intend to suggest that the water layer of *N. pachyderma* (s) habitat was characterized by a large temperature gradient.

When heat began to accumulate in the AW layer, vertical mixing at the base of the halocline gradually intensified. This process progressively assimilated the lower halocline, making it warmer and saltier, and ultimately caused the halocline base to shoal (Fig. R8B). Then the upward transfer of heat gradually reached shallower depths, influencing the upper halocline and surface mixed layer, and leading to progressively stronger temperature fluctuations. Over time, this reduced the vertical temperature gradient. Such a process not only explains the increase in mean temperature within the shallow subsurface layer but also accounts for the intensified fluctuations and broader temperature variability reflected in the rising standard deviation.

To better illustrate this mechanism, we constructed hypothetical temperature-evolution curves (Fig. R9). Before AW shoaling and enhanced vertical mixing (Stage A in Fig. R9), the temperature in shallow subsurface layer exhibited only small variations in a narrow range, with a low temperature standard deviation (Fig. R8A; Fig. R9B, C). Once upward heat transfer and halocline thinning began (Stage B in Fig. R9), temperatures in the shallow subsurface layer fluctuated more strongly across a wider range, reaching higher peak values. This indicates a higher mean temperature in shallow subsurface layer and a reduced temperature gradient relative to the underlying AW (Fig. R8B; Fig. R9A–C). During this stage, the temperature standard deviation of the shallow subsurface layer rose sharply—contrary to the reviewer's concern that temperatures would not fluctuate significantly.

As also noted in Response 39, even though in the stage with the high temperature standard deviation prior to the HS1 in our sediment core records, active sea-ice processes persisted. This indicates that the upper ocean (the shallow subsurface and surface mixed layers) was not fully mixed with AW, nor was it completely dominated by the AW inflow. Therefore, the shallow subsurface layer was not characterized by relative homogenize AW (temperature variations in a warm and narrow range), but a layer with intense temperature fluctuations and alternately influenced by AW and by meltwater.

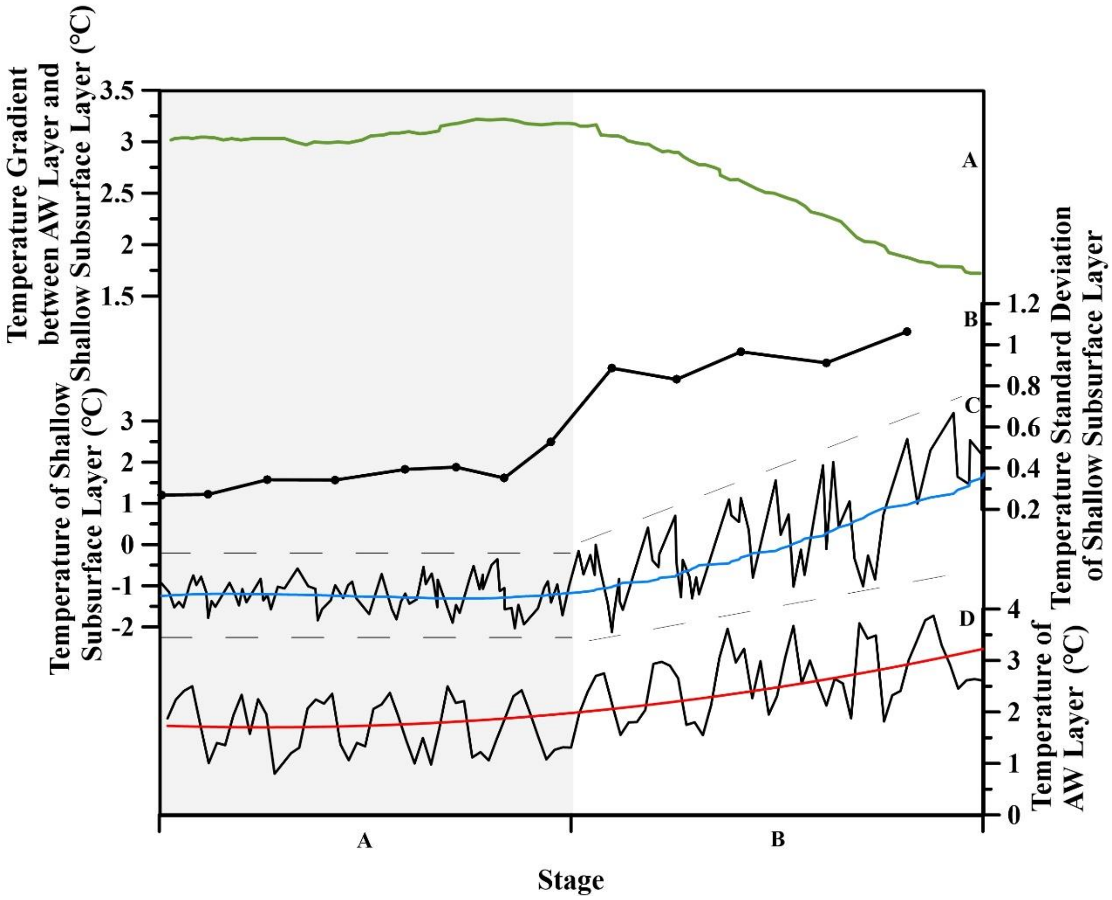


Figure R9. Hypothetical temperature evolution curves of AW layer and Shallow subsurface layer

The reviewer referred to the study by Ezat et al. (2014)[108], which revealed that the AW never stopped flowing into the Nordic Seas during the LGM. We fully agree with this viewpoint. Ezat et al. (2014) found that the intermediate-water temperatures of stadials were 2–5 °C higher than those of interstadials, based on benthic foraminiferal Mg/Ca reconstructions[108]. However, they did not reconstruct surface conditions. So, the reviewer's conclusion based on Ezat et al. (2014) study that a steep temperature gradient with a cold surface and warm subsurface must have prevailed throughout the late glacial period is not fully supported. Furthermore, Ezat et al. (2014) emphasized that vertical shifts in the position of warm AW were responsible for surface warming and the abrupt onset of interstadial conditions[108]. Together with the evidence of a pronounced decrease in B–A ventilation ages since 20 ka BP reported by Ezat et al. (2017)[112], we argue that the upwelling/shoaling of AW, accompanied by upper-ocean warming and intensified mixing, is both reasonable and substantiated by the evidence at hand.

41. 143-146: First, the general trend of temperature increase at the end of LGM is visible in surface North Atlantic(42), the eastern Nordic Seas margin close to the Fennoscandian Ice Sheet(38) (FIS) and the temperature profiles simulated by the TraCE model (Fig. 3J, K and Fig. 4A). – Reference 38 only marginally supports this statement. there is little change in SST (transfer functions) between 23.5 and 19 ka. Only afterwards there is a slight increase in SST.

Response: We appreciate the reviewer's point that the SST record cited from Rørvik et al. (2013) does not rise in parallel with our reconstructed temperature and therefore cannot directly support our statement of upper-ocean warming[7]. Indeed, the SST record of Rørvik et al. (2013) shows little increase between ~22 and ~21 ka BP[7]. However, after recalibrating our age model, the timing of the reconstructed upper-ocean warming shifts to ~20–18 ka. Coincidentally, the SST record of Rørvik et al. (2013) also displays a pronounced increasing trend during this interval, which provides complementary evidence for a warming signal in the eastern Nordic Seas near the margin of Fennoscandian Ice Sheet. For this reason, we still chose to cite the SST data of Rørvik et al. (2013) in our manuscript[7].

42. 146-148: This warming trend, although significant, was not as large as other pronounced amplitudes in the last deglacial period and Holocene. – What warming trend is referred to here and how was significance assessed?

Response: thanks for the reviewer's comment regarding our statement in line 146-148. The "warming trend" we refer to here denotes the synchronous increase in sea water temperature observed during the late stage of the LGM at multiple sites: in the eastern Nordic Seas margin close to the Fennoscandian Ice Sheet and Barents–Kara Ice Sheet, in the surface North Atlantic along the path of the North Atlantic Current, and at our core site, as we described in the front part of this paragraph (line 147-151). We consider that this warming trend is not so pronounced and shorter in duration compared to other amplitudes observed in the Bølling-Allerød period and Holocene. It is because the temperature increase at this stage is not as large as that observed during the Bølling–Allerød (8.23 °C) and Holocene (6.95 °C), and the duration of this warming is relatively short. For these reasons, we describe this warming trend is not so pronounced.

43. 148: the surface warming trend reported in dinocyst records (35) The age model in reference 35 does not comply with most recent advances in constraining marine researvoir ages in the polar and Arctic oceans (see also main comment).

Response: The reviewer claim that the age model used in Rigual-Hernández et al. (2017), from which we cited dinocyst-based temperature records, did not apply the most recent advances in constraining marine reservoir ages in the polar and Arctic oceans[120]. This implies that their age model may contain some uncertainties and may not be entirely accurate.

In Rigual-Hernández et al. (2017) study, they calibrated the AMS ^{14}C ages using the Marine09 dataset with a $\Delta R = 84 \pm 23$ obtained from the Marine Reservoir Correction Database in Calib 6.0 for the northwestern Barents Sea[120]. The Marine09 is indeed an older calibration curve and lacks precise constraints for high-latitude oceans

However, Rigual-Hernández et al. refined the Holocene portion of their chronology by aligning tie points with nearby regional stacks of palaeosecular variation and relative geomagnetic paleointensity. For sequences older than 10 ka BP, their age model was further constrained by correlations with other cores from adjacent areas and the stacked MS-chronology of Jessen et al. (2010), assuming linear sedimentation rates between dated/correlated horizons. Therefore, we consider their age model is relatively accurate and reliable, and the dinocyst temperature records to be valid and worth citing.

44. 169: The correlation with the consistent surface warming in the subpolar North

Atlantic is likely reflect a significant accumulation of ocean heat during the later LGM (42) (1) please fix grammar, (2) I'm perplexed by the choice of reference here since a "clear" warming is not apparent in Peck et al 2007 over the interval in question. Also, the omission of datasets closer to the core site (Ezat et al 2017 and Thornalley et al. 2011) seems odd. I conclude that the argument for a "consistent surface warming in the subpolar North Atlantic" is not supported by references.

Response: We appreciate the reviewer's comment regarding both the grammatical issue in the original sentence and the absence of an apparent warming signal in the temperature record we cited from Peck et al. (2007)[121] during the late stage of the LGM. First, we have revised the sentence for grammatical accuracy and readability. We have revised this sentence as: "The correlation with the consistent surface warming in the subpolar North Atlantic likely reflects substantial ocean heat accumulation during the late stage of LGM, which in turn accelerated the northward heat transport and facilitated the development of a subsurface heat reservoir in the Nordic Seas." (line 174-177).

The reviewer correctly noted that the warming trend in the Peck et al. (2007) record is not pronounced between 21–19 ka BP under our previous age framework[121]. However, in our newly calibrated chronology, the timing of the warming shifts to around 20–18 ka BP. Coincidentally, the Peck et al. (2007) record shows a clear surface temperature increase with a distinct peak during this interval[121] (Fig. 3L). Furthermore, their subsurface temperature record also indicates a synchronous increase, and the temperature difference between the surface and subsurface nearly diminishes to zero. This suggests that the upper ocean (including both surface and subsurface layers) in the northeastern Atlantic experienced a coherent warming, accompanied by intensified vertical mixing. This interpretation is consistent with our statement in the manuscript that warming along the path of the North Atlantic Current reflects enhanced northward heat transfer into the Nordic Seas. To further support this hypothesis, we also cite the surface temperature record from core SV23-81[122,123], located near the Peck et al. (2007) site, which likewise exhibits a pronounced temperature peak during this interval (Fig.

3J).

Secondly, regarding the temperature records from the nearby cores RAPiD-15-4P and JM-FI-19PC, studied by Thornalley et al. (2011) and Ezat et al. (2017), respectively, we did not include them in our manuscript[112,113]. For RAPiD-15-4P, Thornalley et al. (2011) reconstructed Mg/Ca-based temperatures only from 21 ka BP onward (the later stage of the LGM), without covering the early to mid LGM[113]. As a result, it is difficult to assess whether the temperature in later LGM has increased compared to early and mid LGM. Therefore, we chose not to cite this record. For JM-FI-19PC, the shallow subsurface temperature record does show a comparable warming trend beginning at 21 ka BP, with a peak between 20 and 19 ka BP[112]. However, during 25–23 ka BP, their record exhibits an anomalous warming peak, with mean temperatures over this 2 ka interval significantly higher than those of the early LGM and even exceeding Holocene values[112]. We interpret this anomaly as reflecting regional variability or the influence of HS2. Because this anomalous fluctuation is unusually strong and much more pronounced than the subsequent warming peak at 20–19 ka BP, we decided not to cite this record in our manuscript. Taken together with the temperature records in the Nordic Seas and the northeastern Atlantic during this period, we consider this warming event is reliable.

45. 171: accelerates the northward heat transport and contributes to the buildup of a subsurface heat reservoir in the Nordic Seas. – where is the evidence supporting the buildup of subsurface heat? According to Ezat et al. 2014, AW remained stable until HS1.

Response: We appreciate the reviewer's concern regarding the lack of evidence for our description in line 171 about the buildup of a subsurface heat reservoir.

As stated in the beginning of this sentence, "the buildup of a subsurface heat reservoir in the Nordic Seas" is a potential phenomenon that we have inferred based on the presence of subducted Atlantic Water in the glacial Nordic Seas and intensified northward heat transfer from late LGM to HS1. Importantly, our newly calibrated shallow subsurface temperature reconstruction shows a distinct warming trend, which provides direct support for this inference. The intensified northward heat transfer across the Iceland–Scotland Ridge and subducted together with the Atlantic Water, enhancing heat accumulation in the subsurface layer, activating the Atlantic Water layer, and ultimately leading to its shoaling and a stronger vertical mixing.

The reviewer notes that the AW temperature record reconstructed by Ezat et al. (2014) indicates that AW remained relatively stable until HS1[108]. Indeed, their benthic foraminiferal Mg/Ca-derived temperatures show persistently high values throughout the LGM, with limited fluctuations until the marked increase in the middle of HS1. First, it is important to emphasize that the benthic records of Ezat et al. (2014), derived from a water depth of 1179 m, primarily reflect intermediate-water conditions in the southern Nordic Seas[108]. These records do not likely to capture temperature variability in the upper part of AW or shallow subsurface layers, nor the enhanced subsurface heat accumulation associated with a stronger North Atlantic surface current.

Secondly, Ezat et al. (2014), along with Marcott et al. (2011) and He et al. (2020), pointed out that the thick polar-water layer (or freshwater input) overlying a strong halocline forced AW subduction into greater depths, thereby contributing to the intermediate-water warming during HS1[124,125]. This mechanism suggests that the HS1 warming trend in their record may be unrelated to the surface and subsurface processes we reconstructed.

Thirdly, unlike the temperature record, the benthic $\delta^{18}O$ record of Ezat et al. (2014) shows an earlier increase beginning around 20 ka. Moreover, Ezat et al. (2014) noted that temperature change can explain only ~20% of the $\delta^{18}O$ shift observed during the transition from the LGM to HS1[108]. We argue that intensified vertical mixing between the subsurface and surface layers, together with sea-ice melt induced by AW shoaling, contributed to vertical exchange with isotopically lighter $\delta^{18}O$ waters, thereby increasing $\delta^{18}O$ values at intermediate depths. This interpretation is broadly consistent with the inference presented in our manuscript.

46. 177: provides evidence from another angle for a causal role of subsurface AW upwelling in strengthening the surface mixing and warming. (1) Would upwelling not bring "old carbon" to the surface? (2) And would the decrease in benthic-atmosphere (B-A) 14C offset not suggest stronger deepwater formation (sinking of water)?

Response: We appreciate the reviewer's concern about whether the upwelling of AW could have caused an old-carbon effect or enhanced deepwater formation. In fact, as we state in our manuscript, the upwelling of AW actually represents the upward shift of the position of the AW layer, whereby the AW progressively intruded into the shallower depth across the halocline into the shallow subsurface layer and surface mixed layer. This was a long-term process, occurring over several hundred to several thousand years, and differs fundamentally from the continuous upwelling current near the coastal lines that transports bottom material directly to the surface. Consequently, we consider it unlikely that AW upwelling introduced "old carbon" to the surface ocean. To make the expression clearer and to avoid misleading readers, we have changed the statement of "upwelling of AW" by "shoaling of AW layer" throughout the manuscript.

Regarding the reviewer's question of whether the decrease in the benthic–atmosphere (B–A) ^{14}C offset suggests stronger deepwater formation, our answer is yes. The shoaling of AW, together with intensified vertical mixing, likely enhanced deepwater formation and contributed to a modest strengthening of the AMOC after 20 ka BP, as reflected in Pa/Th records reported by McManus et al. (2004) and Lippold et al. (2009)[52,126].

47: 178: the substantial heat emission from AW could facilitate the sea-air exchange – where is the evidence that the heat escaped the surface halocline/sea-ice covering AW. It is entirely possible that the heat remained below and NP occupied the 100-50m range as often shown in ecological data.

Response: We appreciate the reviewer's question regarding the mechanism of vertical heat transport and whether AW heat could reach the surface layer and influence sea ice.

Modern observations in the southern Norwegian Sea show that the halocline is typically found at ~50–150 m depth[85,88,92,127]. During stadial periods, the halocline tended to shoal and thicken[106,107]. Our Mg/Ca-derived temperature reconstruction reveals a pronounced warming in the shallow subsurface layer (~50 m; see the recalibration of calcification depth in Response 4), indicating that AW heat was transported upward through the lower halocline, affecting the upper halocline and the surface mixed layer. When combined with the synchronous decrease in IRD and the PIP25 record in the southern Nordic Seas in this time interval (Fig. 3G, H, I), this strongly suggests that AW-derived heat release contributed to the weakening of sea-ice activity.

Further compare to the analogous Atlantification observed process in the modern Eurasian Basin, the intensified AW shoaling has shoaled the AW layer to ~80 m depth—just below the winter surface mixed layer. This shoaling contributed to a substantial increase in upward oceanic heat flux during winter, from an average of 3–4 W m^{-2} in 2007–08 to >10 W m^{-2} in 2016–18, driving more than a twofold reduction in winter ice growth[23]. By analogy, the warming trend we reconstructed in the shallow subsurface layer at our core site demonstrates that heat released from AW shoaling could indeed influence the surface layer and sea ice, rather than being locked at depths of 50–100 m. And this warming trend also have been confirmed in the simulated ocean temperature anomalies above 50m shown in the Fig. 4A.

48. 183: Ocean response sensitive to the insolation changes

Response: we removed "the" in this subheading.

49. 184: From a broader perspective, the observed changes in surface and

subsurface Nordic Seas hydrography during the late stage of LGM – I'm confused. Is it surface or subsurface?

Response: we appreciate reviewers for raising their confusion about our statement on "changes in surface and subsurface". By this, we refer to the process of subsurface AW shoaling and the subsequent warming of the shallow subsurface layer, together with the associated decline in sea ice at the surface. In addition, the surface changes in the Nordic Seas also include the surface temperature variations we cited from the Spitsbergen shelf margin and the western Norwegian continental shelf shown in the previous section. Therefore, the "observed changes" we state here encompass both surface and subsurface evolution in the Nordic Seas.

50. 185: define what is meant by "local"

Response: We apologize for the confusing use of the term "local feedback." What we intended to convey was the regional feedback to large-scale climate forcing. Accordingly, we have revised "local" to "regional."

51. 186-188: Our proxy data of temperature standard deviations (Fig. 2E) indicate that the starting point of surface warming may be earlier than previous deductions from the studies in the Norwegian Sea and the area south of Iceland. – The argument here is confusing. Above the point was made that AW continues to inflow into the Nordic Seas throughout the LGM and that the warming signal recorded here is stemming from AW that are upwelling to the surface. This would reduce the T gradient in the top 200m not increase it – especially if the heat is venting to the surface and the halocline disappears.

Response: We appreciate the reviewer for pointing out the ambiguous and confusing statements in this sentence. With the recalibration of our age framework, both the warming trend at our core site and the onset of increasing temperature standard deviations are delayed by ~1 ka and showed no advance compare with previous studies in the Norwegian Sea and south of Iceland records. Therefore, we have removed the comparison with previous studies to avoid potential misunderstandings.

52. 190: radiocarbon dates at 22.7 ka BP and 18.6 ka BP, respectively, are systematically reliable independent of the age model uncertainties. – yes but they need to be recalibrated, which will result in younger Cal Age BP.

Response: We appreciate the reviewer's suggestion and have recalibrated the age model (see details in Response 1). Given the uncertainties in the age model, and to avoid potential misunderstandings in defining the exact timing of the boundary of this warming trend, we have removed this sentence.

53. 193: may begin earlier at ~22 ka BP – speculative.

Response: We also removed this sentence to improve the rigor of the manuscript.

54. 195: Moreover, the timing of the AW upwelling is concurrent with the significant recovery of 65N caloric summer insolation at around 22 ka BP (54,67) (Fig. 3A), which may reveal a close link between solar forcing and the heat accumulation in the AW layer of Nordic Seas. – Suggest changing to: concurrent with peak summer insolation at 65N at 22 ka.

Response: we appreciate the reviewer's suggestion to replace "65°N caloric summer half-year insolation" with "peak summer insolation at 65°N." We understand that the "peak summer insolation" referred to here likely corresponds to summer solstice insolation at 65°N, which represents the peak insolation index.

The caloric summer-half year insolation we mentioned here is the amount of insolation integrated over the caloric summer half of the year, defined such that any day of the summer half receives more insolation than any day of the winter half[31,49]. Near 65°N, the variance of this measure has almost equal contributions from climatic precession and the obliquity. Since ice-sheet growth and decay depend both on the intensity of incoming solar radiation and the duration over which it remains elevated[31,50], we use the caloric summer half-year insolation metric in our manuscript instead of "summer solstice insolation" to better represent variations in orbital energy input and their impact on Northern Hemisphere ice sheets. This metric has also been widely used in the latest research in recent years.

55. 197-199: the concurring initial weakening of latitudinal winter-insolation-gradient between 60N and 30N and the subsequent basin-wide surface warming trend – I see that the figure references the model output however there is little proxy evidence to support this statement.

Response: We sincerely thank the reviewer for pointing out that the reference we cited did not provide sufficient proxy evidence to support our statement in lines 197–199. Our original intention of writing this sentence here was to put forward a hypothesis. We observed that the concurrent weakening of the latitudinal winter insolation gradient (LIG) between 60°N and 30°N are synchronize with the warming trend in the Nordic Seas. Based on this, we hypothesize that variations in latitudinal insolation forcing may have triggered regional oceanic feedback processes.

In fact, the references we cited (e.g., Thornalley et al., 2011[113]) provide strong evidence that the LIG influenced the evolution of high-latitude wind-driven ocean circulation through changes in the solar heating difference between the tropics and the poles, as well as in the latitudinal temperature gradient (LTG). Moreover, Thornalley et al. (2009) also combined multiple proxy records to demonstrate the role of LIG in driving Atlantic atmospheric circulation and ocean gyres[128].

Specifically, Thornalley et al. (2009) tracked the LIG forcing on the main climate modes of North Hemisphere, based on the Northern Hemisphere LTG between the Scandinavian Arctic and the African subtropics reconstructed from pollen-derived temperature anomalies[128]. They showed that weakening of the LIG led to a reduced winter LTG during the mid-Holocene and the Eemian interglacial, which was associated with more positive NAO conditions. Changes in the NAO further altered wind stress across the North Atlantic, strengthening the northward advection of Atlantic waters along the Norwegian Current and enhancing the penetration of warm waters into the Nordic Seas. Although pollen records are missing for the late LGM to HS1, Thornalley et al. (2009) combined model-estimated LTG with alkenone-based temperature records from the California Current demonstrated that early weakening of the LIG would have an apparent influence on the subsequent poleward shift of the subtropical high-pressure system and the intensification of related ocean gyres in this interval[128].

Taken together, we consider that these studies provide sufficient proxy evidence to support a close link between LIG variations and regional oceanic feedbacks, such as the variation in subpolar/subtropical gyres and associated heat transport.

56. 202-204: During the cold conditions, the apparent instantaneous oceanic reaction to the growing solar insolation in the surface Nordic Seas can be shown in the reduced sea ice cover and lower ice-albedo feedback, as well as the in situ warming surface water affecting the hydrographic conditions. (1) Be more precise and define "the cold conditions". (2) define instantaneous and uncertainties associated with it.

Response: we appreciate the reviewer for pointing out our imprecise use of the term "cold conditions." As for the cold conditions, what we intended to describe were glacial periods characterized by relatively low surface temperatures, extensive sea-ice cover, and a strong ice–albedo feedback. The expression "cold conditions" lacks precision and does not clearly define the temporal or climatic boundaries. Therefore, we have revised it to "glacial conditions."

The term "apparent instantaneous oceanic reaction" was originally used to describe the immediate seasonal to interdecadal oceanic responses to insolation variations, representing a regional response distinct from the influence of large-scale ocean circulation or hemispheric scale long-term climate fluctuations. Our intention was to contrast these short-term, localized responses with the broader inter-basin scale insolation–atmosphere–ocean forcing mechanisms in the Nordic Seas discussed later in the manuscript. However, we realized that the phrase "apparent instantaneous oceanic reaction" could be misleading for readers, and therefore we have revised it to "apparent immediate oceanic response."

57. 205-207: However, the model simulation results revealed that the energy of local solar insolation are insufficient to drive large-scale surface warming associated with the ice sheet instabilities and major climate transitions in the north Hemisphere. (1) the use of however is confusing. Earlier in the manuscript the warming signal is interpreted to stem from AW that are upwelling to the surface. It was never suggested that the warming is driven by the atmosphere.

Response: We appreciate the reviewer's concern regarding our use of "however." The preceding sentence emphasized that the increase in local solar insolation could trigger regional responses such as enhanced heat transport and in situ surface-water warming. In contrast, the current sentence highlights that the energy provided by local solar insolation alone is insufficient to drive large-scale surface warming associated with ice-sheet instabilities and major climate transitions in the Northern Hemisphere. Therefore, we used "however" to connect these two statements and to emphasize this contrast (regional solar heat transport is insufficient).

58. 207-209: Our correlations between ocean feedback and solar forcing are most likely tied to the orbitally forced modulations of atmospheric-oceanic variability and the poleward flux of latent/sensible heat. Revise this statement and be more

precise. Refer to AW inflow directly.

Response: We sincerely thank the reviewer for the suggestion to make this statement more precise and to refer to the AW inflow directly.

Following this advice, we have revised the sentence to "The linkages between oceanic feedbacks in the Nordic Seas and solar forcing are most likely tied to the orbitally forced modulations of atmospheric-oceanic variability and the poleward flux of latent/sensible heat transported by AW." (line 210-212).

59. 213: The NAO is a winter atmospheric circulation pattern only significant over the past 30-40 years or so and cannot explain atmospheric variability beyond even over the observational period. And much less for the LGM, where the topography of Ice sheets vastly changed the positioning of the jet stream. I would suggest to remove all references to the NAO and stick to wind stress and the positioning of the Jet stream.

Response: We greatly appreciate the reviewer's concern regarding our use of the term of NAO to describe the phase of atmospheric modes during the LGM period. We have revised all the statement of NAO in our manuscript, details see in the Response 7.

60. 217-218: This enhanced wind stress in the cyclone direction can present a pronounced east west extension of subpolar gyre (SPG), resulting a stronger AMOC and NAC with higher latitudinal heat flux transport into the North-East Atlantic and the Norwegian Seas. – Fig 4B averages wind stress during 20-22ka. It does not however provide evidence or support for the onset of changes at 22 ka. The preceeding time slice (22-24 ka) might be useful to elucidate this difference.

Response: we greatly 11/20/2025appreciate the reviewer's concern that Fig. 4B may not provide sufficient evidence to support our statement regarding enhanced wind stress in the cyclonic direction in the manuscript.

We would like to clarify that the vectors shown in Fig. 4B represent wind velocity anomalies and the direction of the 850 hPa atmospheric circulation at 20 ka relative to 22 ka, rather than the average wind stress between 20–22 ka. After recalibrating our age model, we infer that the onset of instability in the shallow subsurface water and upper halocline layer, as well as the initial shoaling of AW, may have occurred as early as ~21 ka BP. Accordingly, we re-examined the TraCE simulation results with both full forcing and isolated orbital forcing, comparing 21 ka BP with the middle phase of warming peak at 19 ka BP. Compared with 21 ka BP, the 850 hPa wind field at 19 ka BP shows a pronounced intensification of wind velocity along the cyclonic direction, particularly over the North Atlantic, strengthening northeastward along the NAC pathway toward the Eurasian Ice Sheet (Fig. 4B). We consider these changes in atmospheric circulation can provide supporting evidence for the intensified subpolar gyre and enhanced northward heat transport.

61. 220-221: Meanwhile, the TraCE results revealed that the growing influence of insolation also strengthened 220 the winter surface winds in the Nordic Seas at the later stage of the LGM (Fig. 4B). – please explain how figure 4B reveals "the growing influence" of insolation Also explain the anomalies are calculated (what is the reference?) please add this to the method and figure caption 4.

Response: we appreciate the reviewer's comment regarding our missing explanation of how Figure 4 reflects the growing influence of insolation on the surface wind stress. We apologize for the clerical error: the correct phrasing should be "the influence of growing insolation," rather than "the growing influence of insolation."

In the revised Figure 4B, the shaded colors represent surface wind velocity anomalies at 19 ka BP relative to 21 ka BP simulated under the isolated orbital forcing. These anomalies were calculated by comparing the 10-year average winter wind speeds at 19 ka and 21 ka. The results indicate that, relative to 21 ka BP, surface wind velocity in the southern Nordic Seas increased significantly. We have added the calculation details to the Methods section and the Supplementary Materials following the reviewer's suggestion.

62. 222-223: Yes, increased wind stress and surface mixing would favour upwelling. However, the question is when wind stress intensified which is not covered by the model.

Response: we sincerely thank the reviewer for raising the concern about whether the timing of the surface wind stress intensification simulated by TraCE is consistent with the timing of AW shoaling. In fact, the simulated results shown in Figure 4B (shaded colors) represent surface wind velocity anomalies at 20 ka BP relative to 22 ka BP. After recalibrating our age framework, we resimulated changes in surface wind velocity at 19 ka BP relative to 21 ka BP, which cover the interval from the onset of AW shoaling to the subsequent warming period in the shallow subsurface layer. Consequently, we consider that the simulated time window of surface wind stress adequately captures the period of AW shoaling and intensified surface mixing.

63. 223-227: We thus infer that the heat accumulation in the AW layer and the observed surface warming in Nordic Seas are mainly triggered by the atmospheric-oceanic circulation process underlying the orbital forcing mechanism. – This should be simplified. Basically: AW is upwelling due to enhanced wind forcing during summer insolation maxima. Also, there is no evidence for the accumulation of heat at this time.

Response: we appreciate the reviewer's suggestion to simplify our description of how insolation-driven atmospheric–oceanic circulation triggered upper ocean warming in the Nordic Seas, as well as to reconsider the phrasing of "heat accumulation" in this context. The reviewer emphasized the role of summer insolation maxima in enhancing wind forcing and AW shoaling. However, the winter latitudinal insolation gradient also played an important role in strengthening poleward heat transport[90,129], thereby contributing to the upper-ocean warming in the Nordic Seas. To avoid ambiguity, we have therefore refined the term “orbital forcing mechanism” and specific it to “insolation forcing mechanism.”

In addition, Reviewer 2 pointed out that the warming trend in the upper Nordic Seas—accompanied by AW shoaling and halocline thinning—resembles the Atlantification process currently observed in the modern Eurasian Basin. Accordingly, we have added relevant comparisons with the Atlantification process in the revised manuscript and replaced the phrase “the accumulation of heat” to “intensified Atlantification-like process” in this sentence. The revised sentence now reads: “We thus infer that the observed upper-ocean warming in the Nordic Seas and the intensified Atlantification-like process were primarily driven by coupled atmospheric－oceanic circulation changes that were initiated by orbitally forced reductions in the LIG.” (line 230-233)

64. 233-235 The onset of the deglacial epoch in the Nordic Seas is interpreted to have a chilled surface conditions at around 19 ka BP in the wake of the anomalous warming event in the late LGM, as evidenced by our N. pachyderma (s) Mg/Ca temperature record and the enhanced IRD concentration in core IS-1B (Fig. 3I, M). (1) The cooling at 19 ka is very tenuous and could simply reflect noise (or less noise) in the data. (2) an “event” usually refers to a distinct time period, whereas here the record shown would support a gradual and continuous increase in SST from 22-18 ka (4 ka) rather than an event. (3) The IRD data is tenuous as it doesn’t change much over this period.

Response: we sincerely thank the reviewer for pointing out that the cooling trend around 19 ka BP is tenuous without additional supporting evidence and may represent only a noise signal in the data.

（1）After recalibrating our age framework, the timing of this cooling stage shifted to around 18 ka BP. Furthermore, by recalibrating the Mg/Ca-derived temperatures and removing the carbonate-ion effect according to the reviewer’s suggestion (see details in Response 2), the cooling became more pronounced (the decreased value has dropped from 1.84 to 3.94°C). It is worth noting that the temperature record of 1.19 °C during this cooling period was calculated from the average of 12 single-shell Mg/Ca-derived temperatures. These individual shell temperatures ranged from –2.09 to 3.37 °C, with a standard deviation of 1.53. This indicates that the single-shell temperatures cluster closely around the mean with relatively narrow variability, and all show a significant decrease compared with the preceding data point (5.55 °C). Therefore, we infer that this cooling signal reflects a real variation in the shallow subsurface layer rather than noise. Moreover, surface temperature records from relevant sites in the northern Nordic Seas also show a decrease at around 18 ka BP (Fig. 3J, K, L, M), suggesting that this cooling trend was widespread across the Nordic Seas and not the noise of our dataset.

（2）The reviewer further noted that the gradual warming trend reconstructed from our core proxy records and nearby sites represents a continuous warming process initiated in the later stage of the LGM, rather than a stable warm phase. Accordingly, the reviewer suggested that this interval should not be termed a “warming event.” We have therefore revised the terminology from “warming event” to “warming stage” in line with the reviewer’s recommendation.

（3）Although the IRD record of core IS-1B does not show a sharp change comparable to those at the onset of the LGM or the Younger Dryas (periods that may have been strongly affected by tephra shards from the Vedde Ash and FMAZ II), it nevertheless exhibits a notable increase around 18 ka BP compared with the late LGM (rising from 1.72% to 5.52%) and forms a peak before the HS1. Furthermore, the IRD record from the adjacent site Enam93-21 also shows a significant peak at ~18 ka BP, while the PIP25 record from JM11-F1-19PC displays relatively high values at the same time (Fig. 3G, H, I). Taken together, these records indicate strong meltwater input and active sea-ice activities in the southern Nordic Seas during this period.

65. 236-243: I do not see how the data supports an increase in IRD that corresponds to a 19ka MWP event.

Fig. 5 The cartoon gives the impression of colder SSTs during the 19 ka MWP event than during the early LGM which is not supported by the data shown here.

Response: we sincerely thank the reviewer for pointing out that the IRD records cited in Fig. 3 cannot adequately support our interpretation of strong meltwater input from the Eurasian Ice Sheet corresponds to a 19ka MWP event.

After recalibrating our age framework, the timing of the peak of meltwater input shifted to around 18 ka BP. Accordingly, we removed the IRD record from core MD99-2249 and instead included the IRD record from JM11-F1-19PC, which is geographically closer to our study site. In addition, following the suggestion of Reviewer 1, we also cited the global net freshwater input record simulated by Mikolajewicz et al. (2024), which shows a significant meltwater peak consistent with our interpretation in the earlier part of this section[1].

The newly redrawn cartoon in Fig. 5 illustrates the evolution of the upper ocean structure in the Nordic Seas from the early LGM to 18 ka BP. The positions and thickness of the surface mixed layer, halocline, and AW layer were revised with reference to the modern and stadial Nordic Seas structure, as well as the evolution of modern Eurasian Basin[22,23,45,92].

In the figure, solid lines delineate the different water layers, with their vertical positions indicating depth and distribution, while colors represent the temperature of each layer and its associated trend. Compared with the early LGM (Fig. 5A), the surface mixed layer and halocline in Fig. 5C (18 ka BP) do not appear significantly colder based on the color shading. The strong meltwater input around 18 ka BP led to rapid cooling of the surface and shallow subsurface layers and resulted in the formation of a stronger halocline. This enhanced halocline effectively isolated the surface mixed layer from the underlying AW, forcing the AW to reside at greater depths. Therefore, Fig. 5C primarily reflects the impact of meltwater input and a thicker halocline, and doesn't indicate a substantially colder SST compared with the early LGM.

66. 249-254. This sentence needs a reference. subsurface warming triggering ice sheet collapse has been hypothesised many times before.

Response: we appreciate the reviewer's comment regarding the lack of references supporting the hypothesis of subsurface warming as a trigger for ice-sheet collapse. Accordingly, we have added the relevant references at the end of the sentence.

67. 259-261: However, the classic AMOC signals of mid latitude Atlantic remain invigorated in 19ka BP, and its successive decrease trend came hundreds of years after that of the north. (1) It is not clear what is meant by "classic AMOC signals" (2) change "in" to "at". (3) reformulate the last part of the sentence to clarify.

Response: we sincerely thank the reviewer for pointing out that our description of AMOC signal fluctuations unclear and the grammatical mistake in this sentence.

After recalibrating our age framework, we found that the timing of the reconstructed large-scale meltwater input and the associated collapse of the Eurasian Ice Sheet occurred at around 18 ka BP, approximately 1 ka later than previously inferred. This timing coincides precisely with the initial decline of AMOC prior to HS1, as indicated by the Pa/Th record[52,126], in contrast to our earlier results that suggested a ~1 ka lag between meltwater input and AMOC reduction. Therefore, the hypothesis of a two-step release of freshwater and icebergs from the Nordic Seas presented in our previous manuscript is no longer valid. The inference that the meltwater input in this interval is synchronized with 19 ka Melt Water Pulse is also questionable. Consequently, we have removed these statements (line 256-269 in the original manuscript version), including this sentence, in the revised version of the manuscript.

68. 261: We hypothesized that the freshwater pulses. Plural? Which meltwater pulses exactly are referred to here? So far only one was mentioned at 19ka.

Response: we appreciate the reviewer's comment regarding our misleading use of the plural term "pulses." Since the timing of the meltwater input we reconstructed does not align with the 19 ka Melt Water Pulse (details provided in Response 67), we have revised "freshwater pulses" to "freshwater release."

69. 261: these sequential slowdowns of AMOC. What sequential slowdowns? It is not clear what is meant here. Please clarify. Also, how can the AMOC slow down at different latitudes. It doesn't make sense.

Response: we appreciate the reviewer for pointing out our inaccurate statement regarding the slowdowns of the AMOC.

Initially, we hypothesized a two-step freshwater release in the Nordic Seas that could have led to AMOC declines at different time intervals (18 and 19 ka BP), as recorded by Pa/Th in the northwest Atlantic[52,126] and in the northeast Atlantic[130], respectively. However, after recalibrating our age framework, we found that the timing of the reconstructed meltwater input aligns well with the widely accepted AMOC records from McManus et al. (2004) and Lippold et al. (2009) [52,126]. Therefore, this two-step hypothesis is no longer valid, and we have removed the corresponding statements, including this sentence, from the revised manuscript.

70. 262: and the delay of mid latitude can be interpreted as a two-stage process of the meltwater release in Nordic Seas. What delay at mid-latitudes? This part of the discussion is very confusing as it lacks time references and actual values.

Response: we have removed the hypothesis in this part in the revised manuscript (details see in the Response 67, 69).

71. 263-264: As soon as the rapid cooling in the Nordic Sea, due to the fast cooling at 19ka BP, the occurrence of thick and perennial sea ice limited the southward transport of massive icebergs collapse from the EIS into mid latitude Atlantic. (1) word missing after the first comma (2) where is the evidence/reference that support thick and perennial sea ice at 19 ka? (3) Where is the reference/evidence for southward transport of ice bergs or that it was stopped? (4) Reference 63 proposes perennial sea during Heinrich events only. As is this sentence seems very speculative.

Response: we sincerely thank the reviewer for raising doubts about the hypothesis we proposed. We have removed the hypothesis in this part in the revised manuscript (details see in the Response 67, 69).

72. 269: leading to the meltwater intrusions in mid latitude and the substantial collapse of North Atlantic Deep Water (NADW) formation. – (1) reference needed. (2) change in to at (3) are you referring to HS1 here? If so, please state exactly.

Response: We have removed the hypothesis in this part in the revised manuscript (details see in the Response 67, 69).

73. 272: caused by a precursor event. Be more concise and state specifically what event you are referring to. Also support this assertion with a reference.

Response: we sincerely appreciate reviewer's comment about our unspecific statement about the precursor event which caused the initial weakening of the AMOC.

By "precursor event," we originally intended to refer to the oceanic processes that contributed to the initial weakening of the AMOC in connection with freshwater release prior to the large-scale collapse of the Laurentide Ice Sheet, coinciding with evidence of additional European-origin IRD flux predating the Laurentide Ice Sheet surges within the Heinrich layer[131]. The associated release of meltwater, sea ice, and icebergs into the North Atlantic via the Nordic Seas at around 18 ka BP likely reduced NADW formation and contributed to the initial weakening of the AMOC recorded in the Pa/Th records [52,126].

However, since we have removed the hypothesis of a two-step freshwater release (details see in the Response 67, 69), this paragraph has been completely revised to clarify the relationship between the initial slowdown of the AMOC and the freshwater release at 18 ka BP. To ensure consistency and readability, we have also removed the reference to a "precursor event." The revised section can be found in line 283-294.

74. 288: reference is incomplete

Response: we have corrected and completed the formatting of this reference.

75. 288-293: not sure where this is coming from. CO2 should have been removed as a possibility earlier in the discussion and not in the concluding paragraph.

Response: we appreciate the reviewer's comment about the appropriateness of discussing the influence of rising CO_2 on the initiation of the Last Termination in the concluding paragraph.

Numerous studies have shown that other global-scale forcings, such as the increase in atmospheric CO_2, also played a critical role in completing the termination process. Therefore, it is necessary to discuss and compare the contributions of CO_2 rise to the initiation of the Last Termination in the manuscript.

However, placing this discussion of the impact of rising CO_2 concentrations on the onset of the Last Termination in the concluding paragraph somewhat disrupted the logical flow of the article. So, we removed this part into the discussion paragraph following the reviewer's suggestion. he revised section can be found in line 306-314.

76. 357-361. Equation 2 is the equation of O'Neil et al 1969 and Shackleton 1974. Nyland et al 2006 did not modify it they just show the equation solved for d18Osw.

Response: we sincerely appreciate the reviewer for pointing out our inaccurate description and citation of Equation 2.

Upon reviewing Nyland et al. (2006)[72], we confirmed that they had only cited the equations of O’Neil et al. (1969) and Shackleton (1974)[103,132], rather than modifying them. Accordingly, we have revised our description of Equation 2.

77. 372: different sea areas. Explain what is meant here

Response: the “different sea areas” mentioned here refer to the various ocean regions where *N. pachyderma* (s) inhabits, such as the high-latitude Atlantic and the Southern Ocean. Regarding the shell diameter (or size) and thickness of *N. pachyderma* (s), numerous studies have demonstrated that these characteristics vary across different oceanic regions, geological periods, and under differing water parameters[133,134].

78. 380: … and the summer-winter temperature gap of ambient water. Explain and rephrase to clarify.

Response: we appreciate the reviewer’s comment regarding the unclear description in this sentence.

Here, the term “summer–winter temperature gap of ambient water” refers to the summer-winter temperature difference at the calcification depth of *N. pachyderma* (s) at our core site. We have revised this sentence follow the reviewer’s suggestion. The revised version now reads: “This variation is much smaller than both the temperature amplitudes observed in our time series and the summer − winter temperature difference of the ambient water.”

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Reply to the reviewers' comments on "Insolation-forced oceanic changes drove the initial collapse of the Eurasian Ice Sheet and triggered the Last Termination" by Wu et al.

Content

Responses to the suggestions and comments by Reviewer 1

Thank you for your constructive comments, which helped to improve our manuscript substantially. Please see below for our point-by-point responses.

• Comment on the need to place greater emphasis on the Mikolajewicz et al. simulations when discussing the meltwater source and the interlinked interaction between EIS/LIS.

1. While it is true that the authors "[…] did not use the TraCE results to investigate ocean–ice sheet interactions or to simulate the behavior of the EIS", the interlinked interaction between EIS/LIS and the ocean is a big focus of the paper. So it's perhaps ok to not explicitly mention the absence of coupled ice sheet in TraCE, but in our opinion more emphasis should be given to the simulation from Mikolajewicz et al. when discussing the main conclusion of the paper, that the melt water pulse that initially slows down the AMOC occurs prior to HS1 and originated from the EIS, not the LIS.

Response: We sincerely thank the reviewer for the suggestion to place greater emphasis on the simulations of Mikolajewicz et al. (2025)[1] which indeed provide important context for discussing the identification of the meltwater pulse responsible for the initial weakening of the AMOC and its source.

We apologize for not incorporating dedicated sensitivity experiments to separately quantify the detailed contributions of ocean warming to EIS instability or to fully resolve the coupled interactions between the EIS and LIS. This reflects the scope of this work, which is primarily focused on proxy-based reconstructions of oceanic processes and the oceanic response to meltwater discharge from the EIS, rather than on explicitly simulating ice-sheet dynamics. In future work, we plan to conduct targeted sensitivity experiments to better constrain the role of oceanic/atmospheric forcings to the EIS dynamics. And we would very much welcome the opportunity for further discussion on ice-sheet evolution and to learn more from your valuable insights.

According to the simulations of Mikolajewicz et al.,[1] the first pronounced deglacial meltwater peak occurred at around 18 ka BP and was associated with a surge event of the Fennoscandian Ice Sheet, leading to massive iceberg discharge into the Norwegian Sea. This interpretation is consistent with our proxy-based reconstruction, which indicates significant surface cooling and substantial meltwater input into the Nordic Seas related to the abrupt collapse of the EIS. Accordingly, we cite the simulated net meltwater flux from Mikolajewicz et al.[1] in Fig. 3f and emphasize the timing of the first deglacial meltwater peak in Lines 245–248 to substantiate the occurrence of this meltwater event and its Eurasian source. Moreover, Mikolajewicz et al.[1] further demonstrated that this meltwater release into the Nordic Seas contributed to surface cooling in the North Atlantic and a weakening of the AMOC. This is in line with our inference that this meltwater pulse initiated the early slowdown of the AMOC prior to HS1. Therefore, in addition to the proxy-based reconstruction of AMOC strength (Pa/Th)[2,3], we incorporate the transient climate model-based interpretation of AMOC evolution from Mikolajewicz et al.[1] to complement and strengthen the discussion of the early AMOC weakening in Line 290-293.

In addition, we have incorporated the 2021 study by the Mikolajewicz group (Kapsch et al. (2021)[4], which examines variations in ice-sheet volume and the response of the AMOC to ice-sheet dynamics, providing additional evidence for the acceleration of EIS retreat and concurrent AMOC weakening at around 17.8 ka BP (Lines 288–290). Taken together, these model-based results help to place our proxy-based interpretation into a broader context, which lends further support to our main conclusions. We sincerely thank the reviewer for prompting us to include these additional references and helped to improve the clarity and robustness of our discussion.

- **Comments on including additional TraCE simulation results in the supplementary materials and on revising the definition and citations related to the timing of the Last Termination.**

2. Comment 3/Figure 4: Due to space limitations, the individual figures for each isolated forcing experiment are not included. Is there perhaps space in the supplementary materials?

Comment 16 (L46): The "widely accepted time interval" that is cited describes the deglaciation itself, but not the actual Last Glacial Termination (Termination I), which refers to the transition from full glacial to full interglacial climates as defined by the Marine Isotope Stages. See Broecker & van Donk (1970), which Denton (2010) is referring to. Clark et al. (2009) is cited as well, but this article is about the LGM. Dyke & Prest (1987) are cited, but with the wrong publication date. It should be 1987, not 2008 (which is when it was made available online).

Response: We appreciate the reviewer's comment regarding the lack of figures presenting the results of the individual isolated forcing experiments in TraCE. In response, we have added these results to the supplementary materials (Supplementary Fig. 2).

We also thank the reviewer for pointing out the inaccurate description of the timing of the Last Termination and the miscitation in the reference list. We fully agree with the reviewer's comment and adopt the definition of glacial terminations as relatively rapid transitions from full glacial to interglacial climates, as defined by marine isotope stages according to the study of Broecker & van Donk. (1970)[5]. In the original framework proposed by Broecker & van Donk (1970)[5], Termination I was identified based on the mid-point of the sharp decrease in benthic $\delta^{18}O$ in deep-sea cores occurring at around 11 ka BP, and this timing also falls in the middle of the major deglaciation interval. Moreover, accurate radiocarbon-dated records across different ocean basins suggest asynchronous changes in benthic $\delta^{18}O$, revealing the onset of Termination I varies by region (for instance, the North Atlantic at ~17.5 ka BP and deep Indian at ~14.5 ka BP[6]). Lisiecki et al. (2005)[7], constructing the global benthic $\delta^{18}O$ stack, "LR04" stack, determined the termination timing based on the temporal midpoint between the onset and conclusion of rapid $\delta^{18}O$ shifts, placing Termination I at ~14 ka BP.

Beyond that, Broecker & van Donk (1970)[5] also emphasized the close correspondence between changes in global ice volume, sea level, and benthic $\delta^{18}O$ during the last deglaciation. Denton et al. (2010)[8] further demonstrated that Termination

I coincided with the rapid retreat of Northern Hemisphere continental ice sheets. Most recent studies have increasingly regarded terminations not merely as isotopic shifts representing transitions from full glacial to interglacial climates, but are also linked to stages of massive meltwater release from global glaciers and the associated deglaciation processes (considered as the end of a glacial cycle, the glacial terminations)[9,10]. Thus, Termination I is increasingly recognized as a protracted process and a critical time interval of large-scale deglaciation following long-term glacial expansion.

In the revised manuscript, we follow the reviewer's recommendation and adopt the definition of Broecker & van Donk (1970)[5], defining terminations based on the interval from the onset of systematic global benthic $\delta^{18}O$ decline to the establishment of a stable interglacial low $\delta^{18}O$ state. Based on the LR04 stack, we constrain the time interval of the Last Termination (Termination I) to approximately 20–8 ka BP, with a mid-point of rapid $\delta^{18}O$ shifts at ~14 ka BP[5,7,10]. Throughout the manuscript, the term "Last Termination" is also used to denote the dynamic process of large-scale Northern Hemisphere ice-sheet collapse in last deglaciation.

We also thank the reviewer for identifying the incorrect publication date of Dyke & Prest (1987)[11], which has now been corrected in the revised manuscript.

• Other comments

3. L51: Missing space, 'ocean-ice sheet'

Response: We have corrected the term "ocean-icesheet" to "ocean-ice sheet" (Line 52).

4. L55: I would remove systematically.

Response: We have removed the term "systematically" in Line 56.

5. L117-118, L243, L283: Missing uncertainties on the 14C dates

Response: We sincerely thank the reviewer for pointing out that the uncertainties of the ^{14}C age points were not explicitly indicated in the manuscript. We would like to clarify that, in many published studies in paleoceanography and paleoclimatology, uncertainties of calibrated AMS ^{14}C ages are typically reported in the chronology section (results part) rather than being annotated alongside individual age points in the discussion (e.g. Max et al., 2022; Hoff et al., 2016; Müller et al., 2009; Justwan et al., 2008[12–15]).

In the present study, owing to space limitations, the calibrated AMS ^{14}C ages for all dating points and their associated uncertainties are provided in the supplementary materials (Suppl. Table 2). In addition, following the reviewer’s helpful suggestion, we have now explicitly included the age uncertainties for all ^{14}C age points referenced in the manuscript in the supplementary materials (Suppl. Table 3), thereby improving the clarity and precision of the study.

6. L258: "weakening" instead of "weaking LIG"

Response: We have replaced the “weaking LIG” by “weakening LIG” (Line 258).

7. L272: typo, ‘deglaciation’.

Response: We have corrected the spelling of “deglaciation” (Line 272).

8. L276: "weakened the buttressing of ice shelves", by melting sea ice? Or melting of ice shelves reduced buttressing of the marine-terminating glaciers and increased ice discharge?

Response: We apologize for the ambiguous description of the processes by which oceanic warming triggers melting of marine-terminating glaciers and ultimately contributes to the initial collapse of the EIS. In this sentence, “ice shelves” refer to large floating ice platforms attached to the coastal margins of continental ice sheets and extending into the ocean[16]. Ice shelves play a critical role in ice-sheet stability by providing buttressing to upstream grounded ice, thereby slowing the flow of land-based ice into the ocean[17].

In response to the reviewer’s concern in the comment, “the weakening of ice-shelf buttressing” does not result from the melting of sea ice, but rather from basal melting of the submarine portions of ice shelves, which leads to their thinning, destabilization, and retreat. In the original sentence, the processes of “melting of marine-terminating glaciers” and “weakening of ice-shelf buttressing” were intended to represent two parallel responses within the EIS system.

To clarify this mechanism and improve readability, we have revised the sentence to: “we presume that the extensive upper ocean warming in Nordic Seas and the strengthened ocean heat transport to the grounding line promoted sustained melting of the marine-terminating glaciers and led to a progressive weakening of ice-shelf buttressing.” (Line 274-277)

9. L317: Missing "the" in front of "Last Termination"

Response: We have added the missing “the” in front of “Last Termination”.

10. L320: "solar activity" implies short-term activity (such as sunspots), I assume you refer to the orbitally controlled "solar insolation" instead

Response: We sincerely appreciate the reviewer's comment regarding our imprecise use of the term “solar activity.” We now have replaced it with “solar insolation” follow the suggestion of reviewer (Line 323).

11. L324-325: "the Last Termination of Northern hemisphere ice sheets", perhaps " the driving mechanisms for the Last Termination and the initial collapse of NH ice sheets"

Response: We sincerely thank the reviewer for this constructive suggestion, and we have revised the sentence to: “Our findings expand the climatological evolutionary chain of the driving mechanisms for the initial collapse of Northern hemisphere ice sheets to an earlier period.” (Line 327-328)

Responses to the suggestions and comments by Reviewer 5

We are grateful to the reviewer for the constructive comments and for raising thoughtful concerns regarding the extent to which ocean warming is attributed a dominant role in triggering the initial retreat of the EIS during the Last Termination. These comments also prompted us to more carefully reconsider the relative importance of oceanic processes and surface atmospheric warming in driving instabilities of Northern Hemisphere ice sheets, and have helped to improve the scientific robustness and thoroughness of this study. Please see below for our point-by-point responses.

• Comments on the representation of surface atmospheric temperature evolution over the Eurasian Ice Sheet and differences in reconstructed temperatures across various models and ice-core record.

1. Line 280-282: surface air temperature reconstructions show no significant warming over the EIS between ~21 and 18 ka BP, suggesting the contribution of surface mass balance in this meltwater pulse was likely limited (Osman et al., 2021)

Paleoclimate data assimilation by Osman et al. (2021) indicates that atmospheric temperatures remained roughly constant between 21 and 18 ka over EIS. However, presenting this information in the manuscript as such may be an oversimplification. Depending on the selected age model, simulations performed with iCESM1.2 and iCESM1.3 show that while some runs suggest negligible changes in mean annual atmospheric temperatures over the British－Irish Ice Sheet (BIIS) and the Fennoscandian Ice Sheet (FIS), others indicate warming of up to ~2 °C. Averaging across all age models, Osman et al. (2021) report a mean increase of approximately 1 °C in annual air temperature over the BIIS and FIS between 21 and 18 ka (see attached figures). This atmospheric warming is even more pronounced over regions corresponding to present-day Great Britain.

More generally, other studies also point toward early atmospheric warming during the last deglaciation. Bouttes et al. (2023), using transient simulations of the last deglaciation with the iLOVECLIM model, report a several-degree increase in surface air temperature over the Eurasian Ice Sheet beginning at the

onset of deglaciation. In addition, $\delta^{18}O$ reconstructions from the GRIP ice core indicate a marked atmospheric warming between 21 and 18 ka. Furthermore, recent simulations by Mikolajewicz et al. (2024) also explore atmospheric temperature changes over the EIS, though these results are not discussed in the present manuscript. And what about TraCE simulations?

I understand the argument presented by the author, but all these studies collectively highlight that significant uncertainties still exist regarding temperature evolution. Consequently, it is extremely difficult to use this argument alone to dismiss the role of the atmosphere in the retreat of the EIS.

Moreover, these uncertainties on atmospheric temperature are even more significant due to the extreme sensitivity of the EIS to atmospheric warming, with an increase of 1 or 2 °C relative to the LGM being sufficient to induce substantial ice loss, particularly at the connection between the BIIS and FIS (Van Aalderen et al., 2024).

Response: We sincerely appreciate the reviewer for pointing out our imprecise interpretation of the reconstructed surface air temperatures over the EIS from Osman et al. (2021)[18]. We are particularly grateful for the reviewer's meticulous re-evaluation of the temperature anomalies from Osman et al. (2021)[18] across different age models, which identified a mean annual warming of approximately 1 °C over the British – Irish Ice Sheet (BIIS) and FIS between 21 and 18 ka. As shown in your provided figures, although this warming is less pronounced than the trend from late HS1 to the B/A period, it nonetheless suggests a discernable surface warming trend in the southern sector of the EIS during this interval.

We also thank the reviewer for referring us to the study of Bouttes et al. (2023)[19], which investigates changes in air temperature and ocean circulation using transient simulations with the iLOVECLIM model. This study indeed identifies a surface warming trend over Northern Hemisphere ice sheets at the onset of the last deglaciation. However, it should be noted that the simulations in Bouttes et al. (2023)[19] investigates climate sensitivity by forcing the model with prescribed ice sheet reconstructions. The inputs consist of two existing ice sheet reconstructions, ICE-6G_C and GLAC-1D, while the outputs are the resulting climate responses, with analyzing changes in global and regional temperatures, sea ice, and AMOC. In this experimental design, temperature fluctuations are the resulting climate responses to ice-sheet variations, rather than the product of unconstrained natural forcing. Consequently, these simulations may be less suitable for directly evaluating the relative contributions of atmospheric warming and oceanic processes to the initial melting of Northern Hemisphere ice sheets.

In addition, we also thank the reviewer for highlighting the atmospheric temperature changes between 21 and 18 ka BP inferred from Greenland ice-core records. As shown in the GRIP temperature reconstruction, there is indeed a warming tendency during this interval, with fluctuations on the order of ~0.3–3.3 °C[20] (Fig. R1). However, compared to the much larger temperature variations exceeding 10 °C that occurred around ~22 ka BP and during earlier glacial stages, this warming remains relatively modest (Fig. R1). This raises the question of why more pronounced surface atmospheric warming during these intervals was not accompanied by large-scale collapse of the Eurasian and Greenland ice sheets. By comparison, the NGRIP record over the same interval (21–18 ka BP) exhibits only minor fluctuations and does not show a pronounced warming trend[21,22] (Fig. R1). The differences between the GRIP and NGRIP temperature evolutions likely reflect regional variability within Greenland and possibly differences in site-specific sensitivity to atmospheric circulation changes. Consequently, we argue that the temperature variations at the GRIP site may not be spatially representative to infer the broader surface temperature changes across Northern Hemisphere ice sheets.

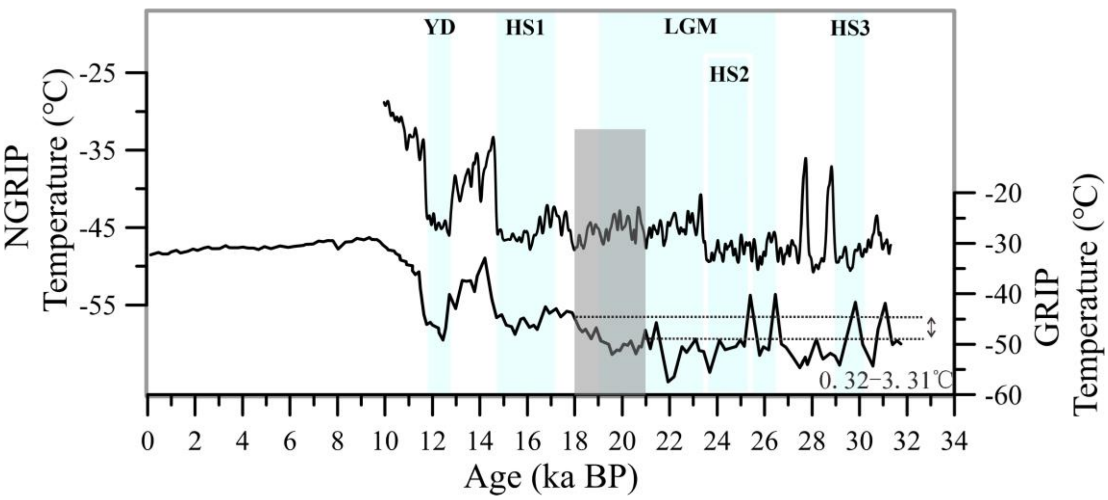
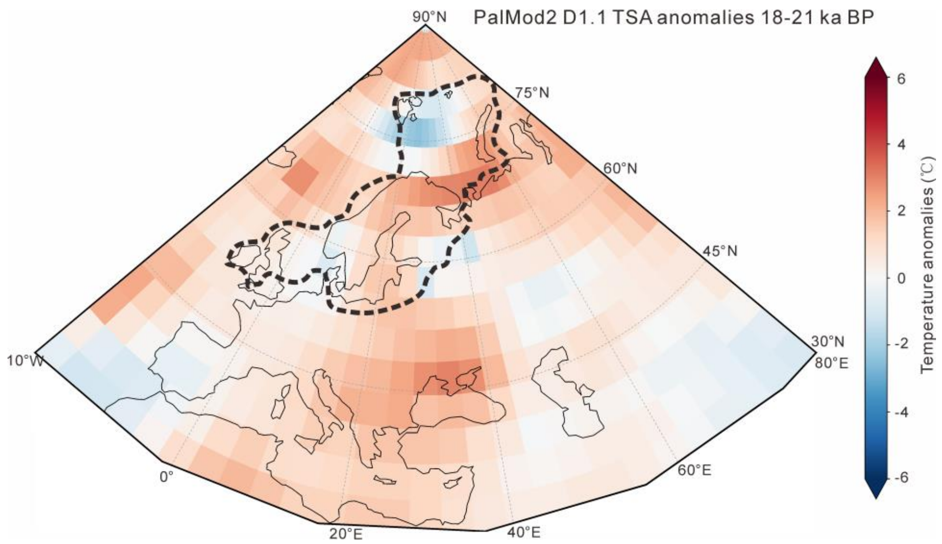
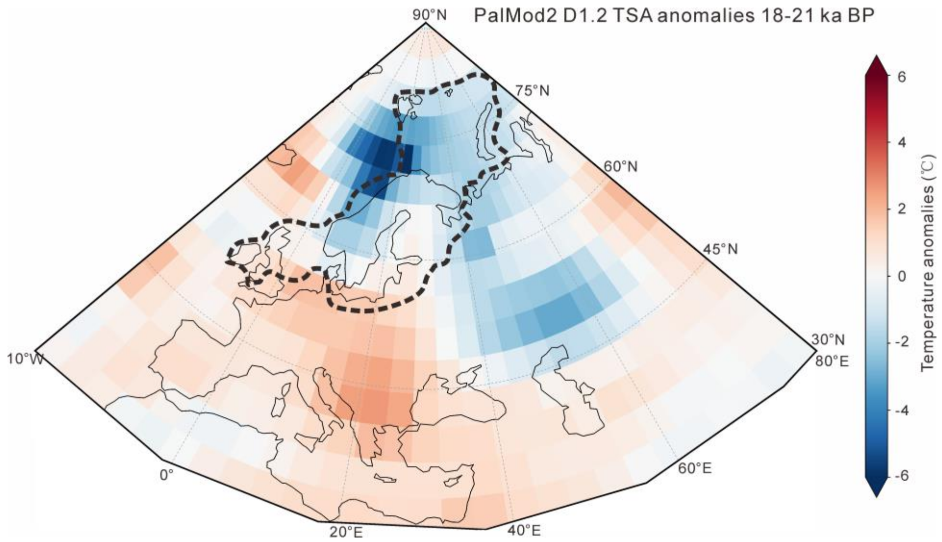


Figure R1. Greenland temperature reconstructions from GRIP[20] and NGRIP[21,22].

Regarding the recent transient climate simulations by Mikolajewicz et al. (2024)[1], we have already cited their results on meltwater flux peak and the inferred sources in our manuscript (Line 245-248). However, we had not previously examined their simulated surface atmospheric temperature fields in detail. In response to the reviewer's suggestion, we have now extracted the surface air temperature outputs from the different sub-ensemble simulations and visualized them (Fig. R2). We further calculated the temperature anomalies over the Eurasian Ice Sheet region for the interval 21–18 ka BP to provide a more comprehensive assessment of atmospheric temperature evolution during this period (Tabel. R1).




PalMod2 D1.2 TSA anomalies 18-21 ka BP
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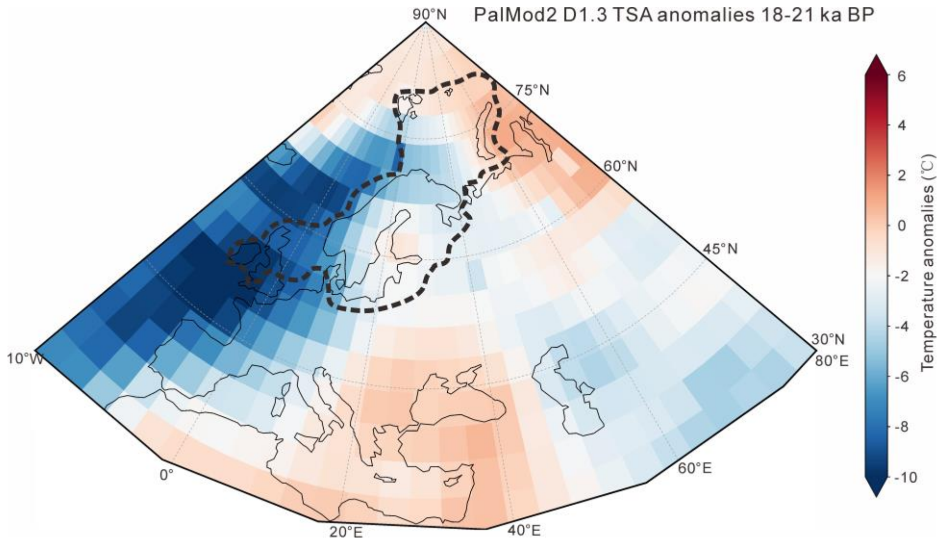
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-8
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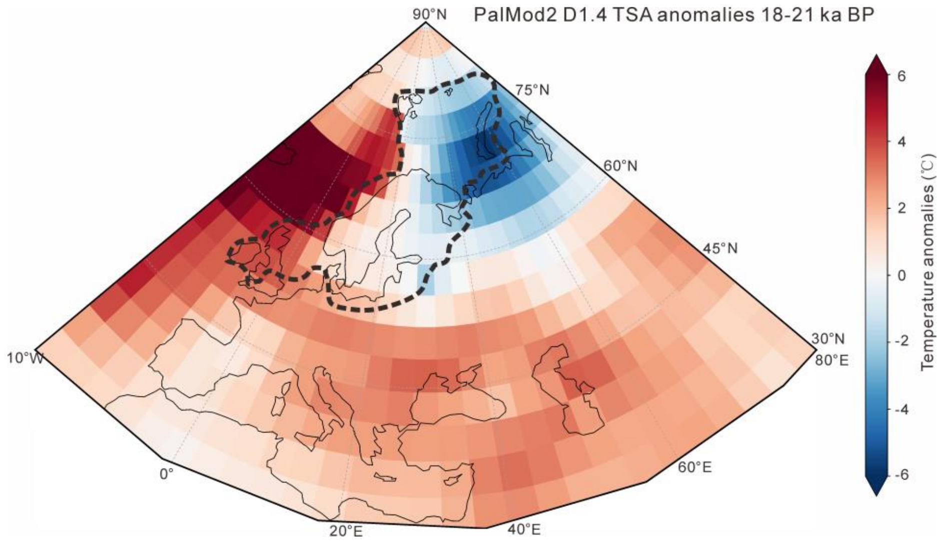
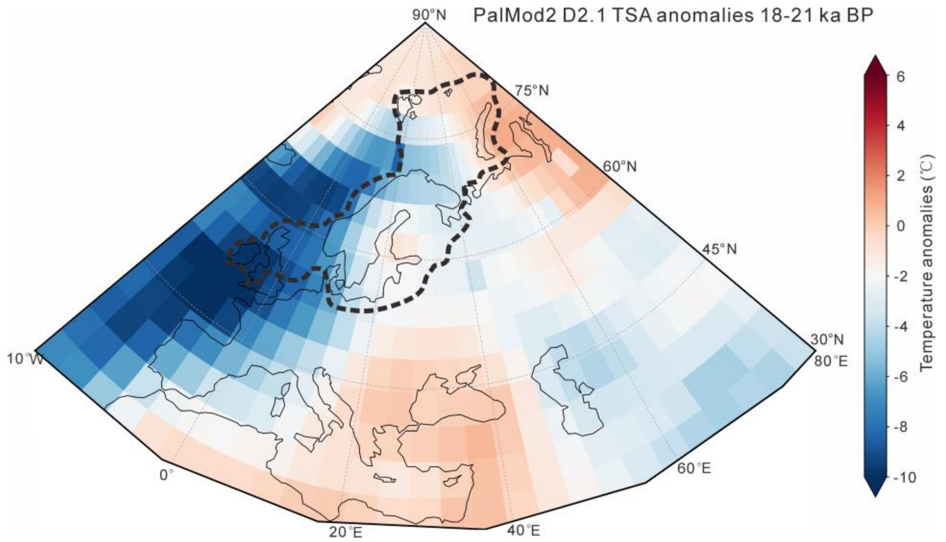

PalMod2 D1.4 TSA anomalies 18-21 ka BP
90°N
75°N
60°N
45°N
30°N
10°W
0°
20°E
40°E
60°E
80°E
Temperature anomalies (℃)
6
4
2
0
-2
-4
-6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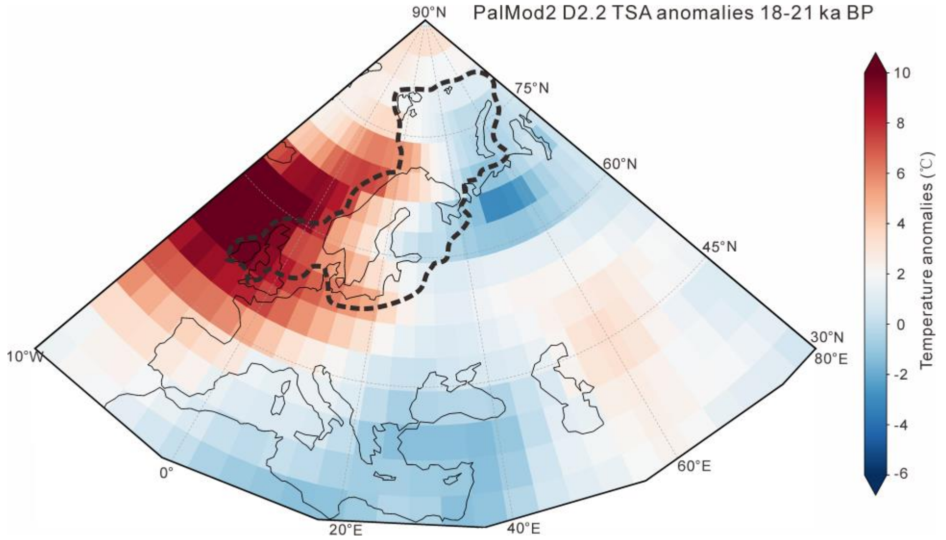
75°N
60°N
45°N
30°N
10°W
0°
20°E
40°E
60°E
80°E
Temperature anomalies (℃)
6
4
2
0
-2
-4
-6
-8
-10


PalMod2 D2.2 TSA anomalies 18-21 ka BP
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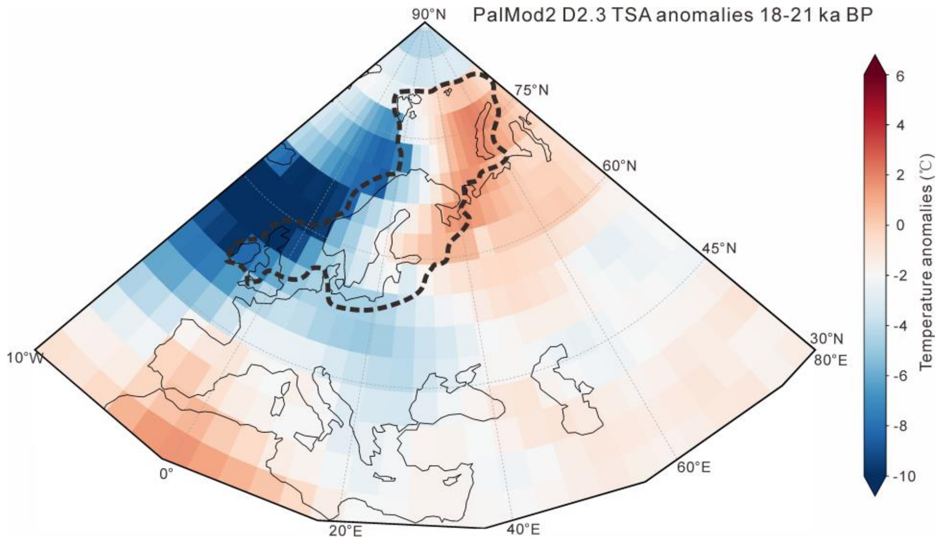
75°N
60°N
45°N
30°N
10°W
0°
20°E
40°E
60°E
80°E
Temperature anomalies (℃)
10
8
6
4
2
0
-2
-4
-6


PalMod2 D2.3 TSA anomalies 18-21 ka BP
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75°N
60°N
45°N
30°N
10°W
0°
20°E
40°E
60°E
80°E
Temperature anomalies (℃)
6
4
2
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-2
-4
-6
-8
-10

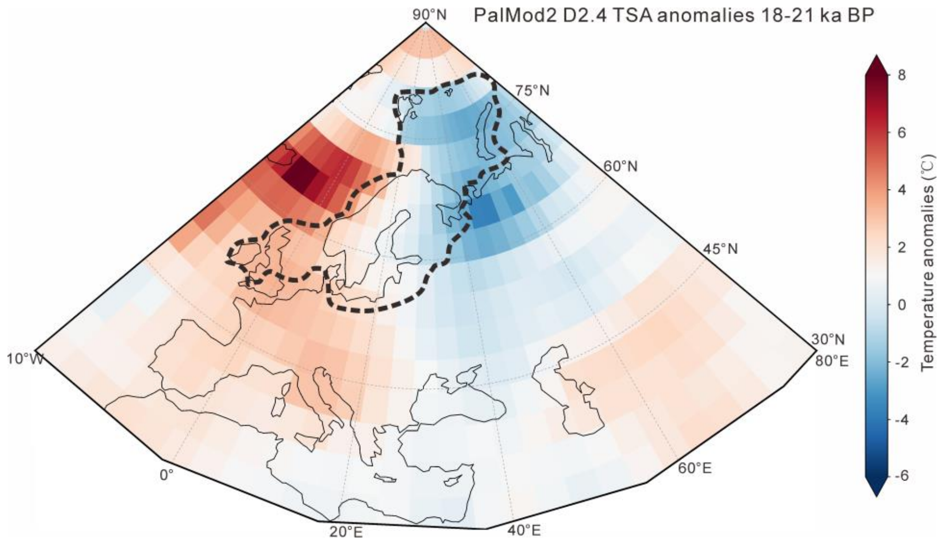

Figure R2. Surface atmospheric temperature (TSA) anomalies (18-21 ka BP) of EIS simulated by MPI-ESM model with different ensemble members[1].

Table R1. Surface atmospheric temperature anomalies (18-21 ka BP) of EIS simulated by MPI-ESM model.

Ensemble member	**Maximum anomaly**	**Minimum anomaly**	**Average anomaly**
D1.1	3.052	-2.428	0.706
D1.2	1.579	-7.066	-1.438
D1.3	0.759	-9.837	-2.986
D1.4	4.642	-5.859	-0.919
D2.1	0.759	-9.837	-2.986
D2.2	11.164	-1.239	2.339
D2.3	2.0388	-12.722	-2.026
D2.4	3.989	-2.585	-0.194
Average	3.497	-6.446	-0.938

The spatial extent of the EIS was reconstructed following Hughes et al. (2016)[23].

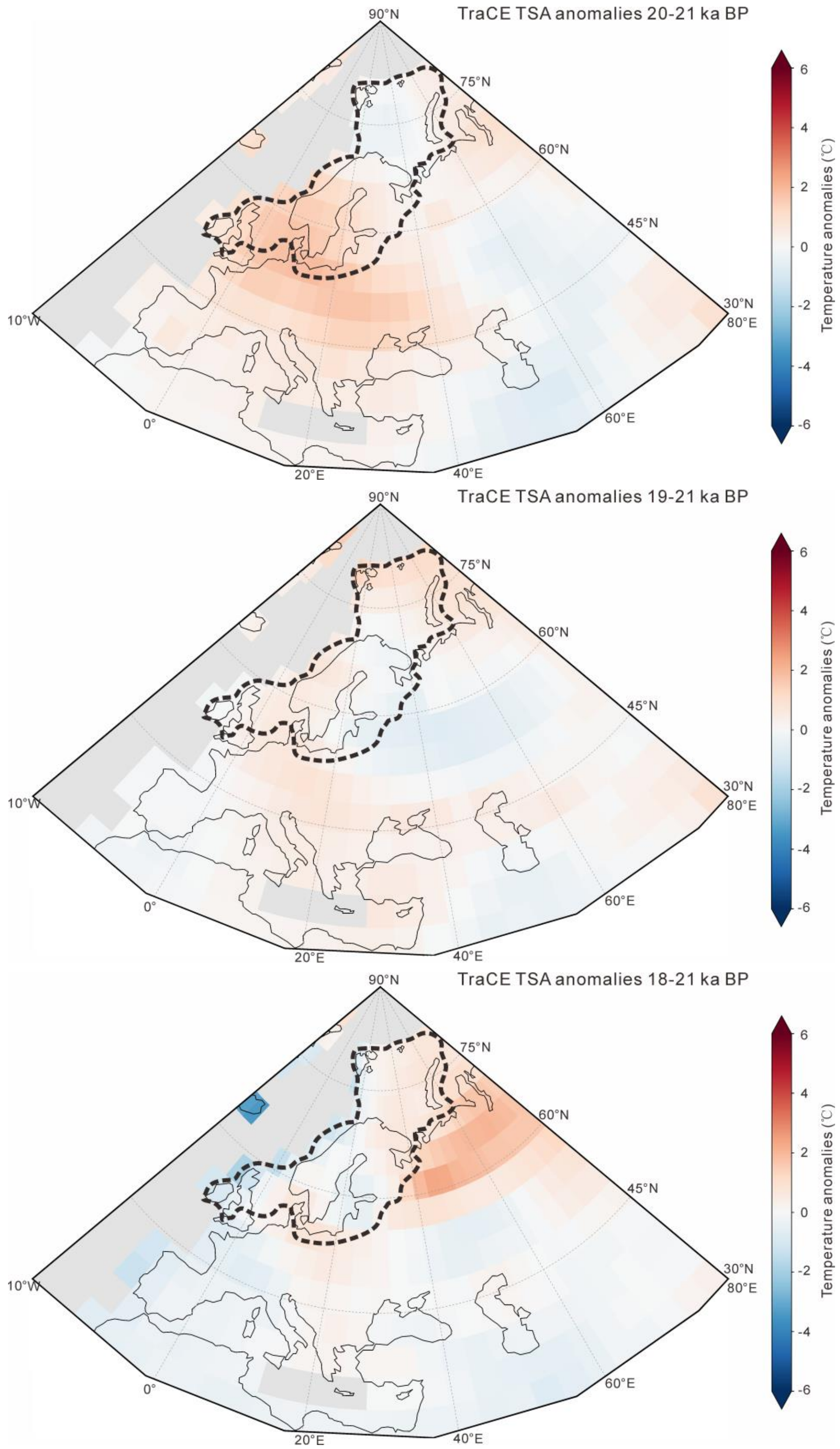

Figure R3. Surface atmospheric temperature (TSA) anomalies (20-21 ka BP, 19-21 ka BP, 18-21 ka BP) of EIS simulated by TraCE under all forcings.

Table R2. Surface atmospheric temperature anomalies of EIS simulated by TraCE under all forcings.

Time interval	Maximum anomaly	Minimum anomaly	Average anomaly
20-21 ka BP	1.726	-0.376	0.402
19-21 ka BP	1.345	-0.532	0.384
18-21 ka BP	1.886	-1.853	0.230

The spatial extent of the EIS was reconstructed following Hughes et al. (2016)[23].

The surface atmospheric temperature anomalies simulated by MPI-ESM show noticeable differences among ensemble members and across regions. The significant warming trend is confined to limited regional domains, such as ensemble D1.1 over the eastern sector of the Barents–Kara Sea Ice Sheet (BKIS) and ensembles D1.4 and D2.2 over Great Britain. The mean temperature anomaly averaged across the EIS region for all eight ensemble members is -0.938 °C. These results suggest that the transient simulations conducted by Mikolajewicz et al. (2025)[1] do not indicate a widespread or large-scale warming trend over the EIS during the 21–18 ka BP period.

Similarly, surface atmospheric temperature anomalies simulated by TraCE also do not reveal a coherent and broad warming trend over the EIS during 21–20, 21–19, or 21–18 ka BP, with mean anomalies remaining below 0.5 °C (Fig. R3; Table R2). The discrepancies among the MPI-ESM, iLOVECLIM, and TraCE simulations, as well as the paleoclimate data assimilation by Osman et al. (2021)[18], suggest that temperature variations during this interval were governed by internal climate variability and regional processes, reflecting pronounced spatial heterogeneity and model-response uncertainty. Consequently, the reconstruction of surface atmospheric temperature over the EIS from the late LGM to the early deglaciation exhibits non-uniqueness, and the existence of a sustained warming trend during 21 – 18 ka BP is still controversial.

Therefore, we concur with the reviewer that the reconstruction of surface atmospheric temperature over the EIS at the onset of deglaciation remains subject to considerable uncertainty. We also acknowledge that our previous reliance solely on the relatively limited temperature increase to dismiss a potential atmospheric contribution to the retreat of the EIS is premature and does not provide a comprehensive assessment of the available evidence.

As noted by the reviewer, sensitivity experiments conducted by Van Aalderen et al. (2024)[24] suggest that the EIS may be highly sensitive to atmospheric warming, with even modest temperature increases capable of triggering substantial ice loss, particularly in the region connecting the BIIS and the FIS. However, in this study, Van Aalderen et al. pointed out that, for the BKIS, substantially larger temperature perturbations (at least 3–5 °C) would be required to initiate its retreat[24]. This threshold is considerably higher than the surface air temperature reconstructions from Osman et al. (2021)[18] under all the age models. Moreover, Van Aalderen et al.[24] also acknowledged that the simulated sensitivity of the EIS to atmospheric warming contrasts with results from other modeling studies (e.g., Petrini et al. (2018, 2020); Alvarez-Solas et al. (2019)[25–27], which instead emphasize basal melting and oceanic processes as the dominant drivers of EIS deglaciation. Van Aalderen et al. attribute these discrepancies among model results to differences in the implementation of climatic forcing (e.g., absolute climatic fields, anomalies, or climatic indices), the procedures used to construct the initial state of the EIS, and the presents of confined versus unconfined ice shelves at the LGM[24]. Therefore, we consider that substantial uncertainties still exist in current ice-sheet modeling studies regarding the driving mechanisms of the initial collapse of the EIS, as well as the relative contributions of atmospheric warming and oceanic processes.

We sincerely appreciate the reviewer for highlighting the potential importance of atmospheric warming in triggering the initial retreat of the EIS by referring to recent model-based reconstructions. However, we remain cautious regarding the conclusion of Van Aalderen (2024)[24] that basal melting played only a minor role. While numerical simulations are invaluable for investigating underlying mechanisms, they inevitably rely on parameterizations and boundary condition assumptions. Specifically, our reconstructions along with other proxy-based ocean temperature records, are grounded in direct geological evidence and provide essential empirical constraints that may more faithfully capture the actual evolution of oceanic processes and ice-sheet responses.

Drawing upon our proxy reconstructions and analogous studies concerning the mechanisms initiating the collapse of the Laurentide Ice Sheet[12], we consider that the contributions of oceanic warming and basal melting remain critical and should not be overlooked. Therefore, as suggested by the reviewer, we have revised the manuscript to adopt a more balanced perspective that takes into account the respective roles of both atmospheric and oceanic drivers of the EIS retreat during the Last Termination, as well as their potential synergistic interplay in accelerating ice-sheet mass loss.

In the Abstract, we have revised the wording to change the description of oceanic forcing from the sole driving force to the main driving force of the initial collapse of Northern Hemisphere ice sheets (Lines 22–25). In addition, we have adopted a more cautious description of the role of the warming trend, using "this warming accelerated the disintegration" instead of stronger causal terms like "drove" to moderate the strength of the causal interpretation (Line 27-29).

In the Introduction, we have refined the wording of oceanic forcing, describing it as a key driving force rather than the sole one. Additionally, we have removed the statement suggesting that oceanic forcing responded earlier to insolation forcing than atmospheric warming, and instead present the two mechanisms in parallel to avoid implying a hierarchical relationship (Line 50-53).

In the Discussion section, we focused our revisions on the third section. Firstly, we change the subtitle from "Upper ocean processes triggered the termination of EIS and the deglacial climate" to "A pivotal role of upper ocean processes in triggering EIS termination and deglacial climate" in order to moderate the interpretation of oceanic forcing, reframing it from the sole driving factor to one of the key triggers (Line 239). Then, we have moderated the wording in this section to adopt a more cautious tone and to avoid overstating the comparative strength of oceanic forcing (Line 266-269, 278-279, 330-331). Lastly, we removed the previous statement suggesting a faint or negligible contribution of surface mass balance changes driven by atmospheric warming to EIS instability, which was originally based on the surface temperature reconstruction of Osman et al. (2021). Instead, we now adopt a more balanced perspective by acknowledging the potential contribution of atmospheric warming to EIS retreat and emphasizing its role in amplifying the sensitivity of the EIS to oceanic warming and associated basal melting (Line 278-285).

• Comments on whether ocean warming alone could have driven the initial collapse of the EIS and the potential contribution of sea-level changes to EIS retreat.

2. The hypotheses proposed regarding the behavior of the EIS rely primarily on reconstructed subsurface ocean temperature fluctuations derived from the sediment core, without explicitly assessing whether the magnitude of this ocean warming is sufficient, by itself, to trigger ice-sheet retreat.

The authors report an increase of 5.62 ° C in subsurface temperatures (SSTsub) after ~20.16 ka and associate this warming with enhanced melting of the EIS. However, Van Aalderen et al. (2024) identify a threshold behavior in their simulations, whereby a minimum subsurface ocean warming of approximately 7.5 ° C is required to initiate the first retreat of the EIS. While this threshold likely depends on the modeling framework and experimental design, the discrepancy raises the question of whether the reconstructed ocean warming presented by the authors is quantitatively sufficient to trigger ice-sheet retreat.

Response: We sincerely thank the reviewer for the insightful comment pointing out that our hypothesis of an oceanic-warming–driven mechanism, primarily supported by proxy records, did not explicitly assess whether the magnitude of ocean warming was sufficient to trigger EIS retreat.

Actually, our hypothesis is mainly based on the precise temporal synchronization between large-scale ocean warming in the Nordic Seas along the EIS margin, and the subsequent massive meltwater release from the EIS, as evidenced by our proxy records. This represents a hypothesized causal linkage inferred from close temporal correspondence, a line of reasoning commonly adopted in paleoclimate and paleoceanography studies, where proxy records provide robust temporal constraints but can not provide direct process quantification. Such synchronization is further supported by the recorded fluctuations in deep-water ventilation in the southern Nordic Sea (Fig. 3E), which we interpret as a direct consequence of the oceanic thermal forcing and meltwater input from EIS. Moreover, we also have cited the sensitivity experiments of EIS from Alvarez-Solas et al. (2019)[27] and Petrini et al. (2020)[26] in support of our hypothesis, as their simulations indicate a major role of oceanic process in regulating EIS dynamics during the last deglaciation. In this context, our study provides crucial proxy-based empirical support for the proposed oceanic driving mechanism.

As noted by the reviewer, Van Aalderen et al. (2024) identified a threshold, suggesting that a minimum ocean warming of approximately 7.5 °C is required to initiate the first retreat of the EIS[24]. In comparison, our reconstructed temperature increase of 5.62 ± 0.57 °C indicates that ocean warming alone may not have been sufficient to reach this threshold and trigger the initial EIS collapse and associated meltwater release during the early Last Termination. However, the results of Petrini et al. (2020) suggest a substantially lower sensitivity[26]. Their simulations show that an increase in ocean temperature of ~2.3 °C above the freezing point in the Barents Sea can lead to a ~65% increase in grounding-line discharge and nearly double the rate of sea-level rise. Notably, our reconstructed shallow subsurface temperature reaches 5.54 °C at 18.11 ka BP in the southern Nordic Seas, exceeding the threshold proposed by Petrini et al. (2020)[26]. Moreover, the dinocyst-based summer temperature of 12.06 °C at ~18 ka BP[28] (Fig. 3M), obtained south of Svalbard near the Barents Sea, also substantially exceeds the temperature increase required in the simulations of Petrini et al. (2020)[26], and represents an increase of 9.90 °C (21.91–18.06 ka BP), surpassing the threshold suggested by Van Aalderen et al. (2024)[24].

The differences in threshold values among these model studies may partly arise from variations in their spatial focus. For example, Van Aalderen et al. (2024) emphasize that the southern sector of the EIS, particularly the region connecting the BIIS and FIS, may have been more sensitive to atmospheric warming[24]. In contrast, Petrini et al. (2018, 2020) suggest that the northern and central sectors of the EIS were more susceptible to oceanic warming and associated basal melting[25,26]. Furthermore, these discrepancies may also reflect differences in modeling frameworks, prescribed boundary conditions, and experimental design. Therefore, the magnitude of ocean warming required to independently trigger the initial collapse of the EIS during the onset of the Last Termination remains controversial in current simulation studies. Nevertheless, oceanic warming and the associated basal melting have been widely recognized as key mechanisms contributing to instabilities in the marine-based sectors of the EIS, and their role in triggering rapid and substantial ice-sheet retreat should not be overlooked.

As we did not conduct sensitivity experiments to quantitatively assess whether the magnitude of warming identified in our reconstructions alone could have triggered the initial collapse of the EIS, we have revised our discussion to ensure a more cautious and balanced interpretation. Specifically, we have removed our previous statement suggesting that atmospheric warming and the resulting negative surface mass balance played only a minor role in EIS deglaciation. In addition, we have moderated our characterization of oceanic warming, revising the description from “the primary driving force” to “a key driving force” in order to avoid overstating our conclusions while acknowledging the potential multi-factor drivers involved in the EIS retreat.

In addition, Petrini et al. (2020), using ice-sheet model simulations of the Barents – Kara Ice Sheet (BKIS), show that an initial retreat occurs in the Bjørnøyrenna region between 20 and 18 ka even when ocean forcing remains constant or slightly cooler than during the LGM. This result suggests that other mechanisms, such as rising sea level or atmospheric warming, may have contributed to the initiation of ice-sheet retreat.

Response: We also sincerely appreciate the reviewer for drawing our attention to the modeling study of BKIS deglaciation by Petrini M. et al. (2020)[26] and for highlighting the initial retreat of the BKIS in the Bjørnøyrenna region between 20 and 18 ka, when ocean forcing was described as remaining constant or slightly cooler than during the LGM. Although the reviewer did not provide the full citation, based on the reviewer’s description, we believe the referenced work is: *Petrini M, Colleoni F, Kirchner N, et al.*

Simulated last deglaciation of the Barents Sea Ice Sheet primarily driven by oceanic conditions[J]. Quaternary Science Reviews, 2020, 238: 106314. After carefully re-examining this paper, however, we were unable to identify statements consistent with the reviewer's interpretation that no oceanic warming occurred between 20 and 18 ka BP or that oceanic forcing played only a minor or negligible role in the initial retreat of the BKIS in the Bjørnøyrenna region.

According to Petrini et al. (2020)[26], the onset of BKIS retreat indeed occurred in the Bjørnøyrenna Ice Stream at the western margin of the ice sheet, where an area loss exceeding 0.11 Mkm^2 was simulated, making it the most rapidly retreating sector during the early phase of deglaciation. Importantly, the authors attribute this retreat to a rapid increase in sub-shelf melting driven by relatively strong ocean thermal forcing and the presence of warm subsurface Atlantic Water. This mechanism is broadly consistent with the ocean-driven processes emphasized in our study. Furthermore, in their simulations, both the annual and July mean air temperatures over the Barents and Kara seas during this interval remain close to LGM values, and the reconstructed surface mass balance of the BKIS remains positive. These results may suggest that the interpretation that atmospheric warming predominantly triggered the initial retreat of the BKIS appears to be less supported in Petrini et al. (2020) framework[26].

Furthermore, Van Aalderen et al. (2024) demonstrate that the sensitivity of the EIS to ocean warming is strongly amplified when atmospheric temperatures increase simultaneously. A combined effect of oceanic and atmospheric warming therefore represents a plausible mechanism for initiating EIS retreat. Finally, global sea level is estimated to have varied by approximately ±20 m between the LGM (~26–21 ka) and 18 ka, and Van Aalderen et al. (2024) also highlight the sensitivity of the Eurasian Ice Sheet to sea-level changes.

Taken together, these results raise a key question that is not addressed in the manuscript: is the reconstructed ocean warming quantitatively sufficient to initiate EIS retreat on its own, or does it represent one contributing factor among several? The paper does not provide a clear answer to this question.

Response: We also thank the reviewer for highlighting the perspective presented in Van Aalderen. et al. (2024), which emphasizes that the sensitivity of the EIS to ocean warming may be strongly amplified when atmospheric temperatures rise simultaneously[24]. In their sensitivity experiments, the combined effects of oceanic and surface air temperature warming indeed result in a more rapid retreat of the EIS and greater mass loss. Van Aalderen et al. (2024) further propose that the surface melting associated with pronounced atmospheric warming can alter the geometry of grounded ice and floating ice shelves, thereby increasing the ice sheet's vulnerability to basal melting and amplifying its dynamic response to oceanic forcing[24]. In light of these results, we are more inclined to agree with the reviewer's assessment that the initiation of EIS retreat was likely driven by a plausible mechanism involving the combined effects of oceanic and atmospheric processes, rather than being attributed solely to one forcing component. At the same time, this interpretation does not diminish the important role of ocean warming, but instead suggests that its impact may have been modulated or amplified under concurrent atmospheric warming conditions. Accordingly, we have revised the manuscript to better reflect this integrated mechanism by adding a short section that discusses the potential contribution of atmospheric warming to EIS retreat and emphasizes its role in amplifying the sensitivity of the EIS to oceanic warming and associated basal melting (Line 278-285).

In addition to atmospheric warming, we appreciate the reviewer's insightful comment regarding the potential influence of sea-level changes on the instability of deglaciation EIS. We fully agree that sea-level fluctuations play a critical role in the Termination processes of Northern Hemisphere ice sheet cycles. During the prolonged cooling of the last glacial cycle, Northern Hemisphere ice sheets expanded to their maximum extent, leading to a pronounced global sea-level minimum during the LGM[10,29]. At that time, the EIS accumulated a large ice volume and experienced maximum isostatic depression, developing vast marine-based ice sheet sectors within marine basins and along the continental slope[8,9]. Such a geometric configuration likely increased the sensitivity of marine-based sectors to subsequent rapid sea-level rise, as enhanced buoyancy forcing may destabilize the grounding zone between grounded ice and floating ice shelves[9]. Furthermore, rising sea levels typically induce grounding-line retreat, which expands the contact area between the ice sheet base and the ocean, potentially strengthening oceanic thermal transport, further enhancing basal melting and grounding-line discharge[26,29].

In fact, in addition to oceanic warming, we had already emphasized in the original manuscript (Lines 263–265) the role of sea-level changes in contributing to ice-sheet instability during deglaciation. We considered sea-level rise together with oceanic warming as an integrated oceanic forcing component, which we identified as a principal driver of the initial retreat of the EIS. To further clarify this mechanism and in response to the reviewer’s valuable suggestion, we have now included a description that sea-level rise enhances ice-sheet sensitivity, thereby increasing its vulnerability to oceanic thermal forcing (Line 281-285).

However, we must acknowledge that we are unable to directly address the reviewer’s question regarding whether the reconstructed oceanic processes were quantitatively sufficient, on their own, to initiate the retreat of the EIS. Since our study does not involve dedicated sensitivity experiments, we cannot quantify the extent to which oceanic forcing alone could have triggered the initial collapse. Accordingly, we have revised the discussion to adopt a more cautious and balanced interpretation, presenting oceanic processes as a key contributing factor rather than as the sole trigger. Detailed revisions are provided in Response 1.

• Comments on the relative importance of basal melting in triggering the EIS retreat.

3. Lines 265－268 : Modeling studies showed that basal melting, induced by oceanic temperature fluctuations, is considerably more efficient than surface mass balance changes in leading the higher rates of iceberg discharge along the EIS margins and the meltwater peaks on the multi-millennial evolution.

Van Aalderen et al. (2024) show that the methodologies employed in some of these modeling studies tend to overestimate the role of ocean forcing in the

destabilization of the EIS. When accounting for these methodological biases, the relative importance of ocean-driven basal melting may be significantly smaller than suggested by the cited studies, calling into question the strong emphasis placed on ocean forcing in this context.

Response: We sincerely thank the reviewer for pointing out that some modeling studies may overestimate the role of ocean forcing in the destabilization of the EIS, particularly in light of methodological differences, as highlighted by Van Aalderen et al. (2024)[24]. The reviewer's comment prompted us to reconsider whether basal melting may have been overemphasized in our interpretation.

Van Aalderen et al. (2024) provided a valuable and detailed modeling investigation, employing comprehensive sensitivity experiments to explore the relative impacts of atmospheric forcing, ocean temperature changes, and sea-level perturbations on EIS dynamics[24]. Their results indeed suggest that atmospheric warming may have played a stronger role in the early retreat of EIS compared to oceanic forcing, which contradict with several previous modeling studies. Van Aalderen et al. attribute this discrepancy to the way climate forcing is implemented (e.g., absolute climatic fields, anomalies, or climatic indexes) and the procedure followed for building the initial state of EIS, as well as to the presence of confined or unconfined ice shelves at the LGM[24].

In the modeling framework presented by Van Aalderen et al. (2024), the relatively coarse spatial resolution (20 km grid size) may constrain the accurate representation of grounding-line ice thickness, especially within narrow Norwegian fjords and major trough systems[24]. Although subgrid treatment of ice velocity at the grounding line are included, but they did not incorporate different values of elevation. This means an underestimate of the ice thickness variations across the grounding line, hence may potentially lead to an underestimation of marine ice sheet retreat. Moreover, it is also important to note that their model setup lacks certain dynamic climate feedbacks that are crucial for capturing the full complexity of EIS evolution. Specifically, the simulation does not account for the ice-climate feedback loops, such as the impact of freshwater forcing on the AMOC and the subsequent redistribution of oceanic heat. And the feedback from a retreating ice sheet on atmospheric circulation patterns and surface temperatures is not fully integrated. Given these omissions, the simulated results may represent a simplified response that may not fully capture the inherent complexities of the actual EIS deglaciation.

Overall, the relative contributions of atmospheric and oceanic forcing remain subject to considerable uncertainty across current modeling studies. Differences in experimental setup and methodological approaches can lead to the potential overestimation or underestimation of specific forcing components. For instance, studies such as Alvarez-Solas et al. (2019) and Petrini et al. (2020) suggest that atmospheric forcing alone may produce modest iceberg discharge, and the role of oceanic forcing has historically been underestimated in certain studies[26,27]. Given the discrepancies among modeling studies, non-simulated evidence like independent proxy records can provide an important constraint for evaluating the relative contributions of different drivers.

From a proxy perspective, direct reconstruction of surface air temperature over the former EIS is not possible (unlike the Greenland Ice Sheet with records from ice cores), as the ice sheet has completely disappeared. Terrestrial biological proxies near the former ice margin primarily reflect regional conditions and cannot represent the integrated surface temperature of the entire ice sheet. In contrast, marine sediment records provide relatively robust reconstructions of ocean temperature variability, including evidence for significant shallow subsurface warming preceding large-scale EIS retreat, as presented in our manuscript. In particular, the high-resolution sediment record of Rørvik et al. (2010), cited in our study, shows that from the late LGM to the onset of deglaciation, at decadal resolution, most of the multiple IRD maxima and plumite deposition layers on the Norwegian shelf occurred shortly following peaks in ocean temperature[30]. This evidence highlights a close temporal link between oceanic warming and meltwater pulses, suggesting that oceanic forcing likely played an important role in the long-term melting and large-scale retreat of the EIS during the Last Termination.

Given the continuing uncertainty across current studies regarding the relative dominance of atmospheric warming versus oceanic forcing, we have revised our manuscript to adopt a more cautious interpretation. Specifically, we have removed the statement that "oceanic temperature fluctuations are considerably more efficient than surface mass balance changes in leading the higher rates of iceberg discharge" in Line 268.

• Comments on the precision of the title and the robustness of the conclusions.

4. In conclusion, the current scientific literature remains characterized by active debate regarding the processes responsible for the retreat of the EIS during the last deglaciation.

The results presented in this paper constitute an important contribution to our understanding of oceanic changes along the margins of the Eurasian Ice Sheet. However, the hypotheses concerning ice-sheet dynamics rely primarily on interpretations of sediment-core data, without the support of new ice-sheet model simulations that directly incorporate the reconstructed subsurface ocean temperatures.

In the absence of such simulations, it remains difficult to robustly assess the response of the Eurasian Ice Sheet to the reconstructed ocean forcing and to identify the dominant mechanisms responsible for its initial retreat. Several key questions therefore remain unresolved: was the increase in ocean temperatures sufficient, on its own, to trigger EIS retreat between 20 and 18 ka? Did rising sea level contribute to destabilization in this region? Did atmospheric warming play a significant role? These questions are either only partially addressed or not discussed in the manuscript, limiting the ability to identify the primary drivers of the initial EIS collapse.

Finally, the experiments presented in this study do not allow for a clear quantification of the relative roles of atmospheric, oceanic or sea-level forcing in driving the initial retreat of the EIS.

Response: We sincerely appreciate the reviewer for pointing out that our reconstruction of the EIS deglaciation process is not supported by dedicated ice-sheet model simulations that directly incorporate the reconstructed subsurface ocean temperatures. In the absence of such simulations, it is indeed challenging to robustly assess the response of the EIS to the reconstructed ocean forcing or to quantitatively evaluate the role of atmospheric warming in its initial retreat.

We acknowledge that we did not perform individual ice-sheet model experiments to examine changes in ice volume and geometry of the EIS under different climatic forcings. Our reconstruction of EIS evolution is primarily based on proxy evidence, which provide the basis for interpreting the timing, as well as the associated oceanic and climatic conditions preceding and following its retreat.

Nevertheless, in our original manuscript we also refer to current modeling studies, including those by Alvarez-Solas et al. (2019) and Petrini et al. (2020), which all conducted dedicated ice-sheet simulations of the EIS incorporating reconstructed subsurface ocean temperatures[26,27]. These studies employed hybrid, three-dimensional, thermo-mechanical ice-sheet models derived from the GRISLI framework, the same framework also employed by Van Aalderen et al. (2024), capable of simulating inland ice, ice streams, and floating ice shelves, and separately simulated the evolution of the overall EIS and the BKIS under transient atmospheric and oceanic forcings[24]. Both two simulation results consistently emphasize the key role of ocean-driven basal melting and enhanced grounding-line discharge, driven by oceanic thermal changes, in triggering the EIS instability and initiating retreat. In particular, Alvarez-Solas et al. (2019) highlighted that atmospheric forcing exerted a modest influence on EIS relative to oceanic forcing during stadial–interstadial transitions, as significant surface mass balance changes were only confined to limited areas[27].

Moreover, we drew an analogy with simulations investigating the mechanisms of iceberg discharge during Heinrich events of the last glacial period for the Laurentide Ice Sheet[31,32]. These studies similarly demonstrate that the basal melting and the breakup of ice shelves at Hudson Strait and Labrador, triggered by ocean temperature oscillations, were the primary drivers of rapid ice-sheet collapse. While these lines of evidence may not definitively quantify the precise extent to which oceanic warming triggered the EIS retreat between 21 and 18 ka BP, nor to determine whether oceanic forcing alone was sufficient to induce its large-scale collapse, the combined proxy evidence and parallel findings across different ice-sheet simulations nevertheless reinforce the plausibility of our proposed oceanic forcing as a key mechanism contributing to the initial retreat of the EIS.

Regarding the overall scope of our study, we focus not only on documenting the retreat of the EIS and its response to ocean temperature changes, but we also aimed to elucidate how oceanic heat transport and circulation systems responded to increasing boreal insolation, as well as the potential teleconnections between the EIS and the Laurentide Ice Sheet mediated by oceanic processes. A central focus of our study is to understand why Northern Hemisphere ice sheets retreated so rapidly during the Last Termination compared to their slow ice accumulation during the last glacial period.

We propose a hypothesis in which increasing boreal insolation modified the westerly wind system, thereby enhancing poleward oceanic heat transport into the Nordic Seas. This oceanic warming, in turn, accelerated basal melting and massive freshwater release from the EIS, which contributed to the early weakening of the AMOC and basin-wide subsurface warming in the North Atlantic, eventually triggering the destabilization of the LIS and the freshwater surges of Heinrich Stadial 1. This framework emphasizes the sensitivity of oceanic processes to insolation forcing and highlights their role as a primary carrier that efficiently conveys insolation signals to Northern Hemisphere ice-sheet systems. Within this evolutionary chain of positive climate feedbacks, we believe it is essential to demonstrate the critical role of oceanic forcing in contributing EIS retreat and accelerating meltwater release. However, we maintain that quantifying whether oceanic warming alone could trigger the initial retreat, or determining the exact threshold required for such a trigger, may extend beyond the primary scope of this proxy-based study.

I acknowledge that implementing ice-sheet model simulations can be complex, and I do not suggest that such experiments are strictly required. However, a more

balanced and deeper discussion of the different mechanisms potentially contributing to ice-sheet destabilization would have been desirable. In this context, the title of the paper "Insolation-forced oceanic changes drove the initial collapse of the Eurasian Ice Sheet and triggered the Last Termination" may be perceived as potentially misleading. A title more focused on the presentation of new subsurface ocean temperature reconstructions, rather than on Eurasian Ice Sheet dynamics, would appear more appropriate.

Response: We thank the reviewer's understanding that the complexity of implementing ice-sheet model simulations and recognizing that such experiments may extend beyond the primary scope of this study. Furthermore, we fully agree with the suggestion that the manuscript requires a more balanced and in-depth discussion of the different mechanisms potentially contributing to ice-sheet destabilization. Accordingly, we have revised the discussion to adopt a more cautious and balanced interpretation, presenting oceanic processes as a key contributing factor rather than as the sole trigger. Detailed revisions are provided in Response 1.

Finally, we sincerely appreciate the reviewer for pointing out that the original title of the manuscript may be perceived as potentially misleading, particularly the description of "oceanic changes drove the initial collapse of the Eurasian Ice Sheet" could have overstated oceanic forcing on the EIS dynamics. We also thank the reviewer for the suggestion to place greater emphasis in the title on the new subsurface ocean temperature reconstructions.

We would like to clarify that, in addition to ocean temperature changes, our study highlights broader oceanic processes during the Last Termination, including freshwater discharge that isolated upper–deep ocean convection, weakened North Atlantic Deep Water formation, and reduced AMOC strength. These interconnected oceanic processes constitute an integral component of the termination process and are directly linked to adjacent ice-sheet dynamics, encompassing more than temperature changes alone. Therefore, we consider the term "oceanic changes" to more comprehensively reflect the scope of our findings. Nevertheless, in response to the reviewer's concern, we have adopted a more balanced title: “Insolation-forced oceanic changes as a key trigger for the initial collapse of the Eurasian Ice Sheet and the Last Termination.”

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Insolation-triggered Eurasian Ice Sheets collapse initiates the Last Termination

What are the noteworthy results?

- In this manuscript, Wu et al. present a Mg/Ca based sea surface temperature reconstruction based on the foraminifera N. pachyderma collected from a sediment core from the northeast of Iceland. The focus of the manuscript is placed on a warming trend in SSTs at the end of the last glacial maximum but preceding a meltwater pulse at 19ka BP. Wu et al. propose that this warming trend initially forced by insolation, triggered a meltwater pulse at 19ka (from the Eurasian Ice sheet), which led to the initial AMOC weakening/collapse which in turn led to Heinrich event 1.

  However, there are significant issues with the chronology and method of reconstructing sea surface temperature, which call into question this interpretation and the validity of the significance claimed. The absence of the warming in other well dated proxy records nearby and further south further questions the validity of the chronology and method used. As is, I cannot recommend this manuscript for publication in Nature Communications.

Will the work be of significance to the field and related fields? How does it compare to the established literature? If the work is not original, please provide relevant references.

- The significance of the presented work lies (1) in the timing and (2) in the SST reconstructions. Both need to be reanalysed and revised (see details below), and I expect the chronology/ timing to change significantly and therefore the cause-and-effect interpretation to be invalid.

Does the work support the conclusions and claims, or is additional evidence needed?

- This is not clear as methods and the chronology need to be revised and the data reanalysed significantly. It is difficult to state whether this will lead to the desired conclusions.

Are there any flaws in the data analysis, interpretation and conclusions? Do these prohibit publication or require revision?

- Yes, details of the issues and recommended revisions are outlined below. As is the data is not suitable for publication

Is the methodology sound? Does the work meet the expected standards in your field?

- No, the method and chronology need to be revised/reanalysed.

Is there enough detail provided in the methods for the work to be reproduced?

- The argument for reproducibility needs to be clearly made in the manuscript and is part of the recommendations below.

Major Issues:

1. **Chronology**

Correct age modelling over glacial interglacial periods for the Nordic Seas is not trivial. Fortunately, calibration curves have recently been updated by the Marine20 radiocarbon (14C) age calibration curve and further refined by Heaton et al (2023) for polar regions. This is important, because Heaton et al. (2023) suggest applying much larger reservoir corrections for the LGM and weaker AMOC stages such as Heinrich events. Please see also Table A2 in Heaton et al (2023) where they list estimates of the latitudinal increase in ΔR values to be applied to modern day ΔR values while still using the Marine20 curve.

This is relevant in the context of the interpretation of the data presented by Wu et al. because larger offsets will lead to younger calibrated ages and thereby place the onset of warming very close to the 19ka-MWP at 19.6-18.8 ka and even H1 (within uncertainties) rather than at the end of the LGM. Specifically, the date at cm 80-82 calibrates to 19.87-20.39 ka (2 σ) instead of 21.44-22.02 ka (using Calib 8.20 and Marine20) and therefore the onset of the observed warming is in range of 19ka-MWP and very similar to observations made in Max et al. 2022. As a result, the cause-and-effect interpretation doesn't hold.

Recommended revisions:

- A detailed discussion using the most recent literature on polar marine reservoir ages (the references provided e.g. Austin et al. 1995 and Klitgaard-Kristensen et al 1998 are outdated) and their uncertainties (at the 2-sigma range) is required to assess the timing of changes in the proxy record.
- Recalibrate all ages using Marine20 calibration curve and the additional reservoir corrections for polar waters as suggested by Heaton et al. 2023.
- Report calibrated ages at the 2 σ range.
- Given the uncertainties associated with marine reservoir corrections in the Nordic Seas an independent chronological marker (ash layers) or tie points to a well dated core nearby (e.g., JM11-FI-19PC) would further support the validity of the proposed age model.

2. **Mg/Ca proxy on N. pachyderma**

There is a long history in determining the temperature signal recorded in N pachyderma Mg/Ca values. Many authors have hypothesized and demonstrated that there are clear secondary controls that affect Mg/Ca values especially at low temperatures (Meland et al. 2006, Kozdon et al 2009, Hendry et al. 2009). Specifically, for N. pachyderma, Mg incorporation appears to be sensitive to seawater carbonate-ion concentration $[CO_3^{-2}]$, pH, and/or salinity which often covary in the surface ocean making it difficult to isolate the sensitivity to SST particularly in cold environments where $[CO_3^{-2}]$ and pH are low (Russel et al. 2004, Evans et al. 2016, Kisakürek et al. 2008, Gray et al 2018, Morley et al. 2017, Hendry et al 2009). The sensitivity and impact of $[CO_3^{-2}]$ on Mg/Ca recorded in N pachyderma specifically has recently been demonstrated in Morley et al. 2024.

Recommended revisions:

- Authors state that *"At present, there is no consensus regarding Mg/Ca-temperature calibration methods for N. pachyderma (s) in previous related research on the Nordic Seas (Kozdon et al. 2009)."*

  There may not be consensus, however, there has been a lot more work published on *N pachyderma* since 2009 that is relevant to the data presented here. In particular, it would be important to discuss findings of Livsey et al 2020 and Davis et al. 2017, since both studies attempted to calibrate Mg/Ca values to temperatures using laser ablation, the same methods as used here. Their results are not very conclusive, which raises question about the application of laser ablation for temperature reconstructions on this species. Notably Davis et al 2017 state: "*A marked improvement is made in reconstructions using N. pachyderma when crust calcite is excluded, and temperature is derived from Mg/Ca ratios in ontogenetic calcite alone, using our culture-based relationships*".

  I would recommend that authors discuss these constraints and apply or test the recommendations made in Davis et al 2017. I would also recommend that authors clearly outline how the Mg/Ca signal was processed and integrated for each measurement. For example, it would help to show the Mg/Ca profiles and clearly outline the portions of crust and ontogenetic calcite used for signal integration (as in Davis et al 2017, Livsey et al. 2020). Also please compare the processing strategy with Livsey et al 2020 and Davis et al. 2017 to elucidate why average Mg/Ca values shown here are so much lower (!) when compared to the calibration datasets shown in both Livsey et al. 2020 and Davis et al. 2017.

- Given these large uncertainties and lack of discussion, the choice of calibration (e.g. Nuernberg et al 1995) seems arbitrary. Simply because a core top dated to 1792 AD matches temperatures in 2018 at 50 m is not sufficient to validate a calibration equation especially since there is no supporting discussion around this and secondary controls on Mg/Ca and the issue of crust are ignored. The highly unreasonable SSS reconstructions of up to 38 psu for the Holocene further underline the need to reevaluate the calibration equation used. Furthermore, I recommend to include an explanation how uncertainties with reconstructed Mg/Ca are assessed.

- Specifically, the impact of $[CO_3^{-2}]$ on Mg/Ca recorded in N pachyderma needs to be discussed and evaluated. Alternative calibration equations and their suitability need to be assessed. Some studies do not observe a sensitivity to $[CO_3^{-2}]$ (Davis et al 2017) while others do (or at least hypothesize it (Hendry, et al. 2009, Kozdon et al. 2009, Meland et al. 2006, Morley et al. 2024).

3. **Calcification depth of N pachyderma.**

Authors state that N pachyderma thrive in deeper colder waters today and that therefore they must live at the cold sea surface in the past glacial ocean. This argument is faulty. Modern ecological observations of *N pachyderma* show that the main seasonal bloom (maximum abundance) occurs at the base of the chlorophyll max (Greco et al. 2019, Tell et al. 2022, Manno and Pavlov 2014), whether this is in the Arctic or subpolar North Atlantic. Previous hypothesis, that associate N pachyderma habitat with temperature or density are not based on modern ecological observations. Based on these modern observations, best estimates for the seasonal calcification depth of N. pachyderma is at 50m (Jonkers et al. 2022) regardless of temperature. It is therefore faulty to assume that N pachyderma is a surface ocean proxy in past glacial oceans.

Line by line comments:

Title

The title doesn't really reflect the core interpretation proposed here. Wu et al. state that the timing for the observed change is supported by the model (e.g., increase in atmospheric circulation). However, only the time slice between 22-20 is shown and no information on the reference period is given. The presented data therefore doesn't preclude that atmospheric circulation changed earlier, for example between 24-22ka. A timeseries would be more convincing to support the proxy data.

Abstract

19-ff. All statements need to be carefully revised after the points raised above on chronology and Mg/ca temperature uncertainties are addressed.

25-27. It is very unlikely that the NAO in its current Dipole is representative of glacial/deglacial boundary conditions. I would recommend refraining from using this terminology and instead refer to wind stress and or the position of the Jet stream in the model.

Introduction

33: marked by significant…. "a" missing

33: "sawtooth" shape variation. Do you mean a sawtooth pattern?

35: known as the "glacial terminations", remove "the"

36: A commonly discussed hypothesis attributed the terminations - A commonly discussed paradigm attributes terminations to

48: the onset of Last Termination – "the" missing

50: factors – mechanisms?

55-56: through thermohaline circulation – "the" missing. But it reads awkward

62: southern edge of EIS is situated - was situated downstream of the North Atlantic Current. Be more precise. The NAC crosses the NA ocean and turns in the Norwegian Current. The EIS would be on its Eastern border/margin.

64: by the Atlantic warm water - by warm Atlantic water. The whole sentence is not clear. Clarify the use of "thereby" and "substantial impacts".

69: under the orbital forcing. – remove "the"

70: Processes responsible for inflow Atlantic Water (AW) oscillations – Mechanisms responsible for AW variability. AW can already be defined in line 64.

71 the role of the fluctuations of northward oceanic heat – the role of northward oceanic heat...

72: triggering the early instabilities of EIS during – remove "the" add "the"

72: during the transition period of Last Termination – prior to?

73: Furthermore, we assessed the impacts of intermittent iceberg discharges from the EIS on the advanced slowdown of AMOC prior to Heinrich Stadial 1 (HS1). The evidence provided here is tenuous especially since the largest peak in IRD occurs during the YD and HS1 and not during the 19ka event

79: Faroe Current, the main branch of the warm AW inflow – is it the main branch? Please quantify Also, What about the EIC? Does it influence the site today? Potentially in the past?

81: suggesting that IS-1B is a sensitive recorder of the past ocean dynamics in temperature and salinity – Rewrite to state explicitly that this is an assumption and that you are not considering an influence of the EIC contributing to the signal recorded.

82: The chronostratigraphic framework of core IS-1B was established by eight accelerator mass spectrometry radiocarbon (AMS 14C) ages spanning the past ~30 ka - refer to the method section and revise as suggested in main comment.

85: as a proxy for the temperature of surface cold water mass (SSTcwm). – This argument is faulty please also see comment above.

88-90: a reference is needed to demonstrate that this approach is valid for NP at low temperatures. I have not seen this work successfully in any calibration attempt.

90-92: To further monitor the stability of surface ~~cold water mass~~ **waters** and the sensitivity of upper ocean structure in response to the oscillation in northward heat flux – what exactly is meant by upper ocean structure? Clarify. Oscillations refer to movement back and forth in a regular rhythm. I don't think that this is what is meant.

92-94: If valid (see main comment) state uncertainties with reconstructions. Taking into account the high variability present in the dataset.

Fig 1: studied cores? Why plural?

109: In the Nordic Seas, the interaction between inflowing ~~warm~~ AW and fresh meltwater through convection is closely linked to regional climate and sea ice behaviour. – interaction through convection? Please clarify exactly what is meant. Also, the reference (Ezat et al. 2014) does not support this statement.

111: indicated that the AW never ceased – remove "the"

113: below the thick halocline – change "the" to "a". Also, how do you know that there existed a thick halocline over the core site during the LGM? The references provided do not support this statement.

113-114: Our new SSTcwm record from ~~the~~ core IS-1B show relatively low values over most of the LGM. – (1) Mg/Ca based SSTs frequently reach values below -2C which is impossible and raises the question whether the calibration equation used is adequate. (2) clearly defined uncertainties are essential. (3) These low SSTs also contrast with SSTs reconstructed from Mg/Ca in Ezat et al. 2017, that show SSTs near 4C prior to the termination. Similarly, Thornalley et al. 2011 report Mg/Ca based SSTs measured on NP of 4 C at (Southeast of Iceland at Rapid-15-4p) for the LGM. Neither study reports or supports a temperature increase after 21.1 ka.

114: However, an intriguing temperature increase of 2.72℃ after 21.1 ka BP – this statement needs to be revised after recalibrating 14C dates (see comment above) and uncertainties at the 2-sigma level for time references need to be shown. I suspect these revisions will place the onset of the warming very close or at least within uncertainties of the termination at 19-20 ka

116: supporting the notion of upper ocean warming in the southern Nordic Seas during the late stage of the LGM. – (1) clarify what is meant by upper ocean warming. is this warming from the subsurface? Or from the surface? (2) given that NP are not a sea surface dweller even in the coldest environments (Grecco et al. 2019) the temperature recorded may not reflect the surface.

117: reconstructed SSS are outside the range of possible values especially for the Holocene with values reaching 38 psu! Similar to Mg/Ca this raises concern about the suitability of the methods and calibration equations used. Also, is it not misleading to show SSS without uncertainties? and to omit the variability per sample that would be introduced via the large range in Mg/Ca values from each individual?

118: revealed that the saltier surface cold water mass may have reduced the surface salinity gradient and stability of the halocline. (1) there is little evidence that NP would reside in the cold and fresh halocline given modern observations they thrive below this layer just below the Chl max. (2) warm water intrusions below the surface would destabilize the water column via cabbeling, which would also induce mixing.

136: This drastic increase in the degree of dispersion of temperature distribution indicates that temperature inside the water mass varied frequently over a wide range. (1) what is meant by "inside the water mass" (2) do you mean that NP habitat was characterized by a large Temperature gradient? If so, does this not contradict with l. 118 where authors imply a destabilized water column and hence increased mixing. Would this not homogenize NP habitat range rather than increasing the T gradient?

138: Compared to the growth of average temperature, the frequent variability of individual shell temperature signals most probably indicates instability of the cold water mass. (1) If the water column was instable and mixed throughout, then the temperature gradient would be getting smaller not larger. This does not explain the increase in Mg/Ca variability. (2) According to Ezat et al. 2014 AW never stopped flowing into the Nordic Seas during the LGM. Therefore, the T gradient would have been steep throughout the late glacial period with a cold surface and warm subsurface. Therefore, one might expect high Mg/Ca variability in this scenario. Not when warm waters upwell to the surface and homogenize the water column.

143-146: First, the general trend of temperature increase at the end of LGM is visible in surface North Atlantic(42), the eastern Nordic Seas margin close to the Fennoscandian Ice Sheet(38) (FIS) and the temperature profiles simulated by the TraCE model (Fig. 3J, K and Fig. 4A). – Reference 38 only marginally supports this statement. there is little change in SST (transfer functions) between 23.5 and 19 ka. Only afterwards there is a slight increase in SST.

146-148: This warming trend, although significant, was not as large as other pronounced amplitudes in the last deglacial period and Holocene. – What warming trend is referred to here and how was significance assessed?

148: the surface warming trend reported in dinocyst records (35) The age model in reference 35 does not comply with most recent advances in constraining marine researvoir ages in the polar and Arctic oceans (see also main comment).

169: The correlation with the consistent surface warming in the subpolar North Atlantic is likely reflect a significant accumulation of ocean heat during the later LGM (42) (1) please fix grammar, (2) I'm perplexed by the choice of reference here since a "clear" warming is not apparent in Peck et al 2007 over the interval in question. Also, the omission of datasets closer to the core site (Ezat et al 2017 and Thornalley et al. 2011) seems odd. I conclude that the argument for a "consistent surface warming in the subpolar North Atlantic" is not supported by references.

171: accelerates the northward heat transport and contributes to the buildup of a subsurface heat reservoir in the Nordic Seas. – where is the evidence supporting the buildup of subsurface heat? According to Ezat et al. 2014, AW remained stable until HS1.

177: provides evidence from another angle for a causal role of subsurface AW 177 upwelling in strengthening the surface mixing and warming. (1) Would upwelling not bring "old carbon" to the surface? (2) And would the decrease in benthic-atmosphere (B-A) 14C offset not suggest stronger deepwater formation (sinking of water)?

178: the substantial heat emission from AW could facilitate the sea-air exchange – where is the evidence that the heat escaped the surface halocline/sea-ice covering AW. It is entirely possible that the heat remained below and NP occupied the 100-50m range as often shown in ecological data.

183: Ocean response sensitive to ~~the~~ insolation changes

184: From a broader perspective, the observed changes in surface and subsurface Nordic Seas hydrography during the late stage of LGM – I'm confused. Is it surface or subsurface?

185: define what is meant by "local"

186-188: Our proxy data of temperature standard deviations (Fig. 2E) indicate that the starting point of surface warming may be earlier than previous deductions from the studies in the Norwegian Sea and the area south of Iceland. – The argument here is confusing. Above the point was made that AW continues to inflow into the Nordic Seas throughout the LGM and that the warming signal recorded here is stemming from AW that are upwelling to the surface. This would reduce the T gradient in the top 200m not increase it – especially if the heat is venting to the surface and the halocline disappears.

190: radiocarbon dates at 22.7 ka BP and 18.6 ka BP, respectively, are systematically reliable independent of the age model uncertainties. – yes but they need to be recalibrated, which will result in younger Cal Age BP.

193: may begin earlier at ~22 ka BP – speculative.

195: Moreover, the timing of the AW upwelling is concurrent with the significant recovery of 65N caloric summer insolation at around 22 ka BP (54,67) (Fig. 3A), which may reveal a close link between solar forcing and the heat accumulation in the AW layer of Nordic Seas. – Suggest changing to: concurrent with peak summer insolation at 65N at 22 ka.

197-199: the concurring initial weakening of latitudinal winter-insolation-gradient between 60N and 30N and the subsequent basin-wide surface warming trend – I see that the figure references the model output however there is little proxy evidence to support this statement.

202-204: During the cold conditions, the apparent instantaneous oceanic reaction to the growing solar insolation in the surface Nordic Seas can be shown in the reduced sea ice cover and lower ice-albedo feedback, as well as the in situ warming surface water affecting the hydrographic conditions. (1) Be more precise and define “the cold conditions”. (2) define instantaneous and uncertainties associated with it.

205-207: However, the model simulation results revealed that the energy of local solar insolation are insufficient to drive large-scale surface warming associated with the ice sheet instabilities and major climate transitions in the north Hemisphere. (1) the use of however is confusing. Earlier in the manuscript the warming signal is interpreted to stem from AW that are upwelling to the surface. It was never suggested that the warming is driven by the atmosphere.

207-209: Our correlations between ocean feedback and solar forcing are most likely tied to the orbitally forced modulations of atmospheric-oceanic variability and the poleward flux of latent/sensible heat. Revise this statement and be more precise. Refer to AW inflow directly.

213: The NAO is a winter atmospheric circulation pattern only significant over the past 30-40 years or so and cannot explain atmospheric variability beyond even over the observational period. And much less for the LGM, where the topography of Ice sheets vastly changed the positioning of the jet stream. I would suggest to remove all references to the NAO and stick to wind stress and the positioning of the Jet stream.

217-218: This enhanced wind stress in the cyclone direction can present a pronounced east-west extension of subpolar gyre (SPG), resulting a stronger AMOC and NAC with higher latitudinal heat flux transport into the North-East Atlantic and the Norwegian Seas. – Fig 4B averages wind stress during 20-22ka. It does not however provide evidence or support for the onset of changes at 22 ka. The preceeding time slice (22-24 ka) might be useful to elucidate this difference.

220-221: Meanwhile, the TraCE results revealed that the growing influence of insolation also strengthened 220 the winter surface winds in the Nordic Seas at the later stage of the LGM (Fig. 4B). – please explain how figure 4B reveals "the growing influence" of insolation Also explain the anomalies are calculated (what is the reference?) please add this to the method and figure caption 4

222-223: Yes, increased wind stress and surface mixing would favour upwelling. However, the question is when wind stress intensified which is not covered by the model.

223-227: We thus infer that the heat accumulation in the AW layer and the observed surface warming in Nordic Seas are mainly triggered by the atmospheric-oceanic circulation process underlying the orbital forcing mechanism. – This should be simplified. Basically: AW is upwelling due to enhanced wind forcing during summer insolation maxima. Also, there is no evidence for the accumulation of heat at this time.

233-235 The onset of the deglacial epoch in the Nordic Seas is interpreted to have a chilled surface conditions at around 19 ka BP in the wake of the anomalous warming event in the late LGM, as evidenced by our *N. pachyderma* (s) Mg/Ca temperature record and the enhanced IRD concentration in core IS-1B (Fig. 3I, M). (1) The cooling at 19 ka is very tenuous and could simply reflect noise (or less noise) in the data. (2) an "event" usually refers to a distinct time period, whereas here the record shown would support a gradual and continuous increase in SST from 22-18 ka (4 ka) rather than an event. (3) The IRD data is tenuous as it doesn't change much over this period.

236-243: I do not see how the data supports an increase in IRD that corresponds to a 19ka MWP event.

Fig. 5 The cartoon gives the impression of colder SSTs during the 19 ka MWP event than during the early LGM which is not supported by the data shown here.

249-254. This sentence needs a reference. subsurface warming triggering ice sheet collapse has been hypothesised many times before.

259-261: However, the classic AMOC signals of mid latitude Atlantic remain invigorated in 19ka BP, and its successive decrease trend came hundreds of years after that of the north. (1) It is not clear what is meant by "classic AMOC signals" (2) change "in" to "at". (3) reformulate the last part of the sentence to clarify.

261: We hypothesized that the freshwater pulses. Plural? Which meltwater pulses exactly are referred to here? So far only one was mentioned at 19ka.

261: these sequential slowdowns of AMOC. What sequential slowdowns? It is not clear what is meant here. Please clarify. Also, how can the AMOC slow down at different latitudes. It doesn't make sense.

262: and the delay of mid latitude can be interpreted as a two-stage process of the meltwater release in Nordic Seas. What delay at mid-latitudes? This part of the discussion is very confusing as it lacks time references and actual values.

263-264: As soon as the rapid cooling in the Nordic Sea, due to the fast cooling at 19ka BP, the occurrence of thick and perennial sea ice limited the southward transport of massive icebergs collapse from the EIS into mid latitude Atlantic. (1) word missing after the first comma (2) where is the evidence/reference that support thick and perennial sea ice at 19 ka? (3) Where is the reference/evidence for southward transport of ice bergs or that it was stopped? (4) Reference 63 proposes perennial sea during Heinrich events only. As is this sentence seems very speculative.

269: leading to the meltwater intrusions in mid latitude and the substantial collapse of North Atlantic Deep Water (NADW) formation. – (1) reference needed. (2) change in to at (3) are you referring to HS1 here? If so, please state exactly.

272: caused by a precursor event. Be more concise and state specifically what event you are referring to. Also support this assertion with a reference.

288: reference is incomplete

288-293: not sure where this is coming from. CO2 should have been removed as a possibility earlier in the discussion and not in the concluding paragraph.

Method:

The main comment on the methods is summarized above at the beginning of this review. Here are some additional small items.

357-361. Equation 2 is the equation of O'Neil et al 1969 and Shackleton 1974. Nyland et al 2006 did not modify it they just show the equation solved for d18Osw.

372: different sea areas. Explain what is meant here

380: ... and the summer-winter temperature gap of ambient water. Explain and rephrase to clarify.

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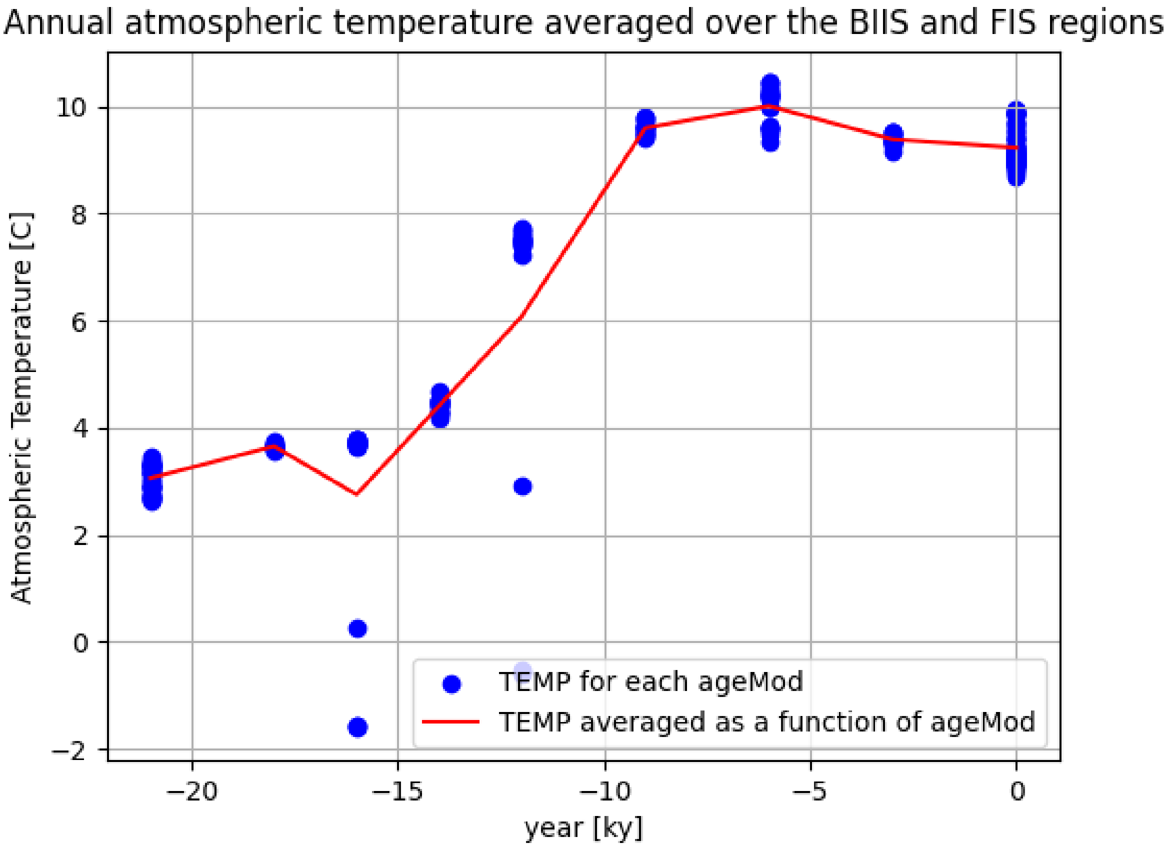
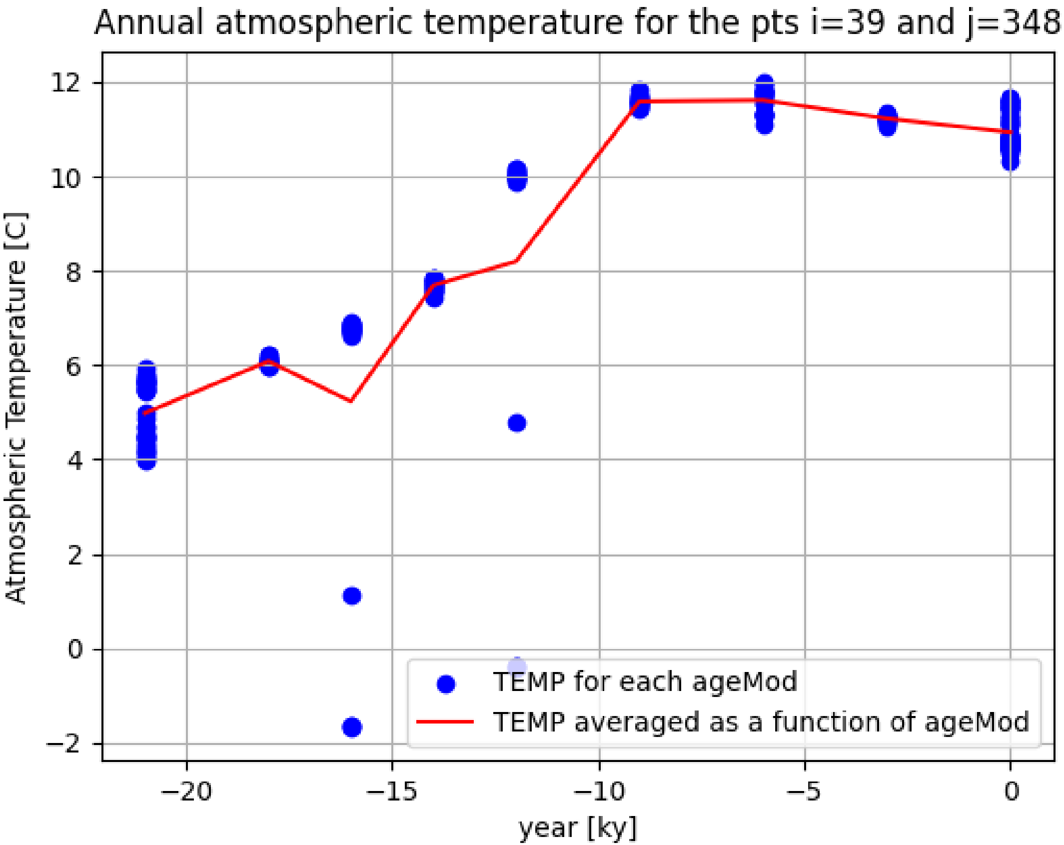


Annual atmospheric temperature averaged over the BIIS and FIS regions
Atmospheric Temperature [C]
year [ky]
10
8
6
4
2
0
−2
−20
−15
−10
−5
0
TEMP for each ageMod
TEMP averaged as a function of ageMod



Annual atmospheric temperature for the pts i=39 and j=348
Atmospheric Temperature [C]
year [ky]
12
10
8
6
4
2
0
−2
−20
−15
−10
−5
0
TEMP for each ageMod
TEMP averaged as a function of ageMod