Author response to editor comments and implemented revisions of egusphere-2026-2414: "Sea ice variability and links to East Siberian permafrost carbon remobilization during the last glacial-interglacial transition"

Reference: https://egusphere.copernicus.org/preprints/2026/egusphere-2026-2414/

Albin Eriksson, Junjie Wu, Matt O´Regan and Örjan Gustafsson

We are grateful to the referees and the editor for providing detailed comments and feedback on the manuscript. The constructive feedback has helped improved the scientific presentation and discussion of our results.

All reviewer and editor comments are included below in *black italic font*. Citations of our implemented changes in the revised manuscript are formatted as indented *italic blue text*.

Editors comments

Dear authors, thank you for your replies to the two reviewers. The reviewers gave positive reviews overall, and made several constructive comments which you have addressed or responded to in your reply. I invite you to submit the revised version of the manuscript so that the impact of these responses on the overall document can be viewed.

Here, I re-iterate several of the reviewer comments that I agree will improve some of the strengths and nuances of your interpretations, and ask that these are incorporated into the revised manuscript.
• Reviewer 1 queried the absence of a sea level curve given that you emphasise the role of sea level during the deglaciation. Whilst you indicate that this is not planned, your abstract emphasises the role of sea level, and it is included as an important component in the discussion (although not in your conclusions). I agree with Reviewer 1 that a sea level curve would ensure that this important contribution to your interpretation is visible. MWP1a and MWP1b annotated on this plot would tie more closely to the arguments made in the text. Figure 5 seems the most obvious place to show this.

> Figure 5a in the revised manuscript now contains the rate of sea level change (m kyr^{-1}) throughout the studied time-period (Lambeck et al., 2014). MWP-1A and MWP1-B are annotated in figure 5a. The following figure caption for panel a has been added in revised manuscript lines 288 to 289:
>
> *"a) The rate of sea level change (m kyr^{-1}) throughout the studied time-period (Lambeck et al., 2014)."*

• Original lines 244 and remaining paragraph. I'm seeking clarification on how you have responded to the reviewers in the edited manuscript, as you indicate that there was more seasonal sea ice (and elevated terrestrial biomarker inputs) during HS-1 in the Laptev Sea than at your site. Can you clarify that in the revisions to this paragraph you have moved away from this being evidence of reduced coastal erosion, and instead about the transport / export of eroded coastal material to the Lomonosov Ridge, which is what I think your reply is indicating? (as well as the reply to the line 275 comment provided by Reviewer 1). I agree with the reviewer that there was a conflict with the interpretation of less coastal erosion given the regional differences but if your argument is that the perennial sea ice prevented transport to your site then this aligns more clearly.

> In the original manuscript lines 244-254, as well as now, we want to highlight the importance of two processes. 1) The perennial sea ice serving as a barrier for transport of terrestrial matter and 2) the limited coastal erosion close to our study site on the Lomonosov Ridge (not the Laptev Sea). The distance from shore is calculated based on the global sea level at the time from Lambeck et al. (2014) and the coring location.
>
> During the HS-1, the coastline was ~75 km away from 31-PC, meaning that the core should document coastal erosion and terrestrial export of organic matter from the coast. We argue that a combination of 1) and 2) is possible during HS-1 around the Southern Lomonosov Ridge. At the location of 31-PC, the existence perennial sea ice suggests that a barrier for terrestrial organic matter export existed, and less coastal erosion was likely when less open water was available to erode the shelf. However, in the Eastern Laptev Sea, terrestrial biomarkers were present at ~4x higher concentrations compared to 31-PC during HS-1, with indications of seasonal sea ice (Hörner et al., 2016). These regional differences in both sea ice cover and terrestrial organic matter export are the basis for our interpretation that

perennial sea ice acts as a barrier for organic matter transport, and can also limit coastal erosion since both sites are close to the coastline at the time.

In order to clarify this, the following sentences are included in the revised manuscript, lines 271 to 279:

"*While limited coastal erosion is evident at the Southern Lomonosov Ridge, a seasonal sea ice cover in the Western Laptev Sea resulted in a stronger input of terrestrial organic matter (Hörner et al., 2016) compared to 31-PC. During the HS-1, 31-PC was rather close to the coastline (75 km), indicating that the core should provide a record of coastal erosion and terrestrial export of organic matter. However, low fluxes of OC and terrestrial biomarkers were present in 31-PC (Fig. 2&5). In the Western Laptev Sea, β-sitosterol and campesterol concentrations were around four times higher during the HS-1 (Hörner et al., 2016) compared to 31-PC. This highlights the role of perennial sea ice in limiting terrestrial carbon export during the HS-1 in 31-PC. Additionally, the proximity to the coastline of both cores captures how seasonal sea ice in the Eastern Laptev Sea permitted higher terrOC input through coastal erosion. Conversely, perennial sea ice cover over the Southern Lomonosov Ridge limited erosion of the nearby coastline."*

• *Reply to reviewer 1 line 284: your suggested edit here seems to lean on the "less coastal erosion" argument again, but here you suggest "limits erosion in the vicinity of the 31-PC site" which seems to contradict the previous bullet point here on reducing transport. I'm confused about which coastline is being eroded "close to 31-PC" – it could be interpreted as meaning erosion of the Lomonosov Ridge, but this is several hundred m below the sea surface so doesn't seem likely?*

We thank the editor for requesting a clarification here. At the end of MWP-1A (13.5 kyr B.P.), the location of 31-PC was ~140 km from the coastline. We expected to see more coastal erosion products from the paleo-coastline than what we observe in our record during the B/A. As clarified in response to the point raised above, we suggest that the low OC/biomarker fluxes in 31-PC result from 1) perennial sea ice serving as a barrier for transport of organic matter and 2) limited coastal erosion close to our study site on the Lomonosov Ridge, where perennial sea ice cover would also limit wave exposure. We have clarified this in revised manuscript lines 320 to 329:

"This suggests a significant role of the permanent sea ice limiting coastal erosion during B/A close to the Southern Lomonosov Ridge. Indeed, the OC-fluxes were much larger in 31-PC when seasonal sea ice was present after DO-3 and the PB (Fig. 2&5). Additionally, nearby cores in the Eastern (PS2458; Fahl and Stein, 2012) and Western Laptev Sea (PS51-154/11; Hörner et al., 2016) display open water/seasonal sea ice conditions concurrent to input of pre-aged OC/n-alkanoic acids, but in several orders of magnitude higher fluxes than in 31-PC (Fig. 5; Nicolas et al., 2025). This suggests that the presence of perennial sea ice at the Southern Lomonosov Ridge, similar to the HS-1, served as a barrier to limit the transport of terrestrial derived material to the core location, and that coastal erosion was limited close to the site. In contrast, regional variability was evident in the Laptev Sea, where seasonal sea-ice permitted substantially greater transport of terrestrial organic carbon, partly due to reduced sea-ice cover facilitating enhanced coastal erosion (Fahl and Stein, 2012; Hörner et al., 2016).

• In response to the queries above, please ensure that the arguments are consistent with what is stated in the abstract and conclusions as well when you make your final revisions.

> We thank the editor for the suggestions and have made sure that the arguments are consistent in the abstract and conclusions.

• Reviewer 1 line 309-311: I think the reviewer is asking here to tone down the proposal that there was "a substantial increase" given that only 1 data point has the oldest age referred to in the text. I would also adjust the "likely linked" statement to "mobilization could be linked to…", because you also have some variability in your sea ice biomarkers at this time.

> The sentence has been rephrased in revised manuscript lines 351 to 353 as:
>
> "*Source* apportionment *of OC suggest that most of the OC in 31-PC came from ICD at the time (Fig. 5; Martens et al., 2020). Due to cross shelf-transport times of 0.6 to 2.6 kyr of the OC (Martens et al., 2020), the mobilization could be linked to coastal erosion driven by sea-level rise during the MWP-1B (Lambeck et al., 2014)."*

• Reviewer 2 asks for age model clarification. The proposed new text partially addresses this comment, but it would be helpful to say what parameter was correlated (tephra horizons, physical properties?).

> The development of the age-model and correlation parameters are clarified in the revised manuscript lines 102 to 108 as:
>
> *"The chronology of core 31-PC was developed through a combination of bulk radiocarbon dating and stratigraphic correlation with Greenland ice-core records, resulting in an age-model covering the last 27 kyr (Muschitiello et al., 2020; Martens et al., 2020). The age model was developed using a Markov Chain Monte Carlo algorithm to probabilistically align variations in sediment porosity with $\delta^{18}O$ records from Greenland ice cores, with eight bulk radiocarbon ages as maximum-age constraints. The age-model development is described in detail in Muschitiello et al. (2020). Sedimentation rates in 31-PC were on average 41±24 cm kyr^{-1}, apart from the past 9 kyr where they were lower (15 cm kyr^{-1}; Muschitiello et al., 2020)."*

• Both reviewers queried the "DO-3" relationship (lines 197-199). Given the wide range of ages, the authors could consider showing these age uncertainties on figure 3 for this data point, or in the text state the median but also the 2-sigma range for the calibrated ages. It still reads as a stretch to suggest that this is DO-3 or related to it, when figure 3 looks clearly offset from that event, and the impact of age uncertainty isn't discussed elsewhere except for the cross-shelf transport noted above. Would the latter play a role here too?

> The age offset in the core bottom is now implemented in figure 2 (used to be figure 3, but was change to figure 2 in the revised manuscript according to referee suggestion). The cross-shelf transport (CST) at the time was only ~0.2 kyr (Martens et al., 2020). Since the age-model uncertainty is much larger than the CST it is therefore not further discussed. To further clarify the age offset in the core bottom we have in the revised manuscript lines 215 to 217 included the uncertainty ranges as:

"Given the age model uncertainty, the DO-3 event (28-27.5 kyr B.P), or the aftermath of it, is likely captured at the base of 31-PC (spanning 27.2 ± 2.3/-1.4 kyr B.P, 90 % confidence interval; Muschitiello et al., 2020; Fig.2)."

• *Remember to change the terminology of orbital forcing in your Conclusion section as well (c.f. reviewer comments on lines 305-306 plus 325-326). The term is also used in line 351.*

Orbital forcing has been rephrased as "*solar insolation"* throughout the manuscript.

Anonymous referee #1: https://doi.org/10.5194/egusphere-2026-2414-RC1

The manuscript „Sea ice variability and links to East Siberian permafrost carbon remobilization during the last glacial-interglacial transition“ by Eriksson et al. addresses the important aspect of coastal erosion contributing organic carbon to the Arctic Ocean during time intervals of climate warming. The authors specifically investigate sea ice conditions recorded in a sediment core from the southern Lomonosov Ridge throughout the past 27 ka and how these may have impacted the stability of permafrost in the Laptev Sea region. By means of the sea ice biomarker IP25, phytplankton and terrestrial biomarkers, the authors provide a continuous record documenting periods of permanent sea ice cover during peak glacial times versus reduced sea ice cover during the Holocene and a variable organic matter export from land reaching the core site. Drawing on previously published data from the sediment core, the authors interpret this input of terrigenous organic matter in the context of coastal and permafrost erosion driven by changes in sea-ice cover and/or sea level rise. The manuscript is generally well written and the data a valuable contribution to the assessment of ice-ocean-land interactions and I hence recommend publication. However, the manuscript also exhibits some weaknesses, regarding the interpretation of own but also of previously published data. In several instances, relevant literature has not been adequately taken into account, and some published findings are represented inaccurately or interpreted in a manner that is not fully consistent with the original reports (see detailed comments below). Addressing these issues would further strengthen the manuscript and improve its scientific rigor. Regarding the interpretation if either sea ice or sea level change drove coastal erosion and organic matter export during the deglacial, it would, for example, be helpful to include a sea level record in one of the figures and to also discuss local records investigating deglacial sea level rise in the Laptev Sea (e.g. Bauch et al., 2001; Taldenkova et al., 2013). More specific comments are listed below.

> Figure 5 now contains a sea level rise curve in accordance to the referee suggestion. Additionally, the important references provided (Bauch et al., 2001; Taldenkova et al., 2013) have been included in the revised manuscript.

Line 10: something is wrong with the wording here

> This has been rewritten in the revised manuscript lines 11-12 as:
>
> *“Sea ice plays a central role in the polar climate system, and its rapid decline in the Arctic over recent decades has altered surface albedo and the exchange of heat and gases between the ocean and atmosphere.”*

Line 11: please correct here and elsewehre B.P. (not B.P)

> This has been corrected, here and elsewhere throughout the manuscript.

Line 15: (Ice Proxy 25)

> “Ice-proxy 25” has been changed to “*Ice Proxy 25”* in revised manuscript line 17.

Line 21: delete ‘over’

> “Over” has been deleted.

Line 35: delete ‘with’

> “With” has been deleted.

Line 39: here, you could also mention the role played by expanded sea ice in shifting the westerly winds southward (and thus reducing the impact of warm/moist air on permafrost) – see e.g. Vandenberghe et al. (2012)

This is now implemented in revised manuscript lines 43-44 as:

"*An extended sea ice cover can also reduce permafrost stability by reducing heat and moisture transport by shifting westerlies southward (Vandenberghe et al., 2012).*".

Lines 40-41: Vaks et al. (2020) is maybe not the best reference regarding "past periods of abrupt climate change" as their study rather deals with the onset of perennial sea ice cover affecting moisture transport 400 kyrs ago

"Abrupt climate change" has now been removed from the sentence. To clarify that it is not during periods of abrupt climate change the sentence is now clarified in revised manuscript lines 41-43 as:

"*Paleoclimate evidence implies that this could have been important in past climates with reduced sea ice conditions enhancing permafrost thawing, leading to increased greenhouse gas release (Vaks et al., 2020).*"

Line 53: (Ice Proxy with a 25 carbon atom skeleton; Belt et al., 2007)

The suggestion from referee #1 has been implemented in revised manuscript line 56 as:

"*(Ice Proxy with a 25 carbon atom skeleton; Belt et al., 2007)*".

Line 53: I suggest to rephrase: "the application of IP25 is limited by two scenarios that result in the lack of IP25: the absence of an ice cover (which serves as the habitat for the sea ice diatoms that synthesise IP25), and the presence of a permanent and thick ice cover that limits the light penetration through the ice and, consequently, the growth of sea ice diatoms (Müller et al., 2009)".

This has been implemented in the revised manuscript lines 56-58 as:

"The application of IP_{25} is limited by two scenarios that result in the absence of IP_{25}: the absence of an ice-cover (which serves as the habitat for the sea ice diatoms that synthesise IP_{25}, and the presence of a permanent and thick sea ice cover that limits the light penetration through the ice and thereby the IP_{25} production (Müller et al., 2011)."

Line 71: please specify that it is Late Pleistocene

"*Late Pleistocene*" was added in revised manuscript line 75.

Line 120: replace 'content' with 'compounds'

"Contents" was replaced with "*compounds*" in revised manuscript line 127.

Line 133: "Identification of IP25 was based on the retention times of the compound in reference sediments". Were at least some of the samples analyzed in full scan mode (to get mass spectra)? This would be very helpful for a more reliable identification of IP25. The C25 HBI monoene and diene have a very similar retention time and often overlap, and here mass spectra would be crucial for a proper identification of IP25.

This has been clarified in revised manuscript lines 141-142 as:

"*To ensure accurate identification of IP_{25}, the reference sediment and subsamples of 31-PC were first analyzed in full scan mode before SIM analysis.*"

Line 136-137: have any instrumental response factors been considered for the quantification of the HBIs and sterols? If not, this is ok, but please state it as it may be relevant for quantitative comparisons with other published records.

To clarify the quantification process in the manuscript lines 136-137 were changed in the revised manuscript lines 144-147 to:

"*Quantifications of IP_{25} and sterols were based on the differences in instrumental mass spectral responses of the target compounds and the internal standards, following the method of Belt et al., (2012). The response factors were determined using external calibration curves of HBI reference sediment, and external standards of sterols (Sigma-Aldrich*)."

Line 155: please add a sentence on how the fluxes were determined

A clarification of flux determination has been added to the method section in the revised manuscript lines 152-154 as:

"*Biomarker fluxes were determined by multiplying analyte concentrations with the dry bulk density and sedimentation rate in each slice of the sediment core following the approach of Martens et al. (2020).*"

Lines 196-197: "The DO-3 event (28-27.5 kyr B.P) is likely recorded in 31-PC (spanning 27.2 ± 2.3/-1.4 kyr B.P". Looking at figure 3 and lines 155 -157, there seem to be no data points covering the DO-3 event. The first signals date to 27.2 ka - so the discussion of DO-3 is a bit misleading here. It may be helpful if you further explain the age model uncertainty for this section of the core and indicate this in the figure.

In the revised manuscript lines 215-217, we have clarified this in the discussion and fig.3 as:

"*Given the age model uncertainty, the DO-3 event (28-27.5 kyr B.P), or the aftermath of it, is likely captured at the base of 31-PC (spanning 27.2 ± 2.3/-1.4 kyr B.P, 90 % confidence interval; Muschitiello et al., 2020; Fig.2).*"

Line 198: seasonal sea ice 'cover' (not 'conditions')

"Conditions" were changed to "*cover*".

Line 202: seasonal sea ice 'cover' (not 'conditions')

"Conditions" were changed to "*cover*".

Line 219- 221: this sounds like a concluding remark and I suggest moving this sentence to the conclusion accordingly - and please remove one of the two 'impacts'

This sentence is an important summary of the first part of the discussion and is therefore kept. However, the first "impact" has been removed in the revised manuscript.

Line 231: please explain HS-1

The abbreviation "HS-1" is now elaborated as "*Heinrich Stadial-1*" in the revised manuscript line 251.

Line 233: I suggest to rephrase "...that thick sea ice inhibited the growth of open-water phytoplankton but also hindered ice algae growth by limiting light penetration through the ice"

> The last part of this sentence has been rephrased according to the referee's suggestion in the revised manuscript lines 252-254 as:
>
> "*The absence of these biomarkers suggests that thick sea ice inhibited the growth of open-water phytoplankton, but also limited light penetration through the ice."*

Line 237-238: could polynyas have played a role here, too?

> This is a good point raised by referee #1 and polynyas could likely play a role here. This has been included in the revised manuscript lines 260-261 as:
>
> "*… but it should be noted that the differences in sea ice cover can also be explained by the presence of polynyas in the Laptev Sea".*

Lines 244 and following: this line of reasoning is a bit difficult to follow. Just a few lines earlier, you state that other records from the Laptev Sea indicate seasonal or ice-free conditions at the continental shelf during HS-1. Since these sites are located closer to the coast, the argument that a permanent ice cover (recorded at 31-PC on the southern Lomonosov Ridge) would have reduced coastal erosion does not hold up.

> A clarification of the HS-1 is now included in the revised manuscript lines 271-279 as:
>
> "*While limited coastal erosion is evident at the Southern Lomonosov Ridge, a seasonal sea ice cover in the Western Laptev Sea resulted in a stronger input of terrestrial organic matter (Hörner et al., 2016) compared to 31-PC. During the HS-1, 31-PC was rather close to the coastline (75 km), indicating that the core should provide a record of coastal erosion and terrestrial export of organic matter. However, low fluxes of OC and terrestrial biomarkers were present in 31-PC (Fig. 2&5). In the Western Laptev Sea, β-sitosterol and campesterol concentrations were around four times higher during the HS-1 (Hörner et al., 2016) compared to 31-PC. This highlights the role of perennial sea ice in limiting terrestrial carbon export during the HS-1 in 31-PC. Additionally, the proximity to the coastline of both cores captures how seasonal sea ice in the Eastern Laptev Sea permitted higher terrOC input through coastal erosion. Conversely, perennial sea ice cover over the Southern Lomonosov Ridge limited erosion of the nearby coastline."*

Lines 247-249: here, you note that permanent sea ice cover minimized the erosion/input of older carbon, whereas pre-depositional 14C ages reach their maximum between 26 and 24 ka (Fig. 5)

> This sentence has been revised and clarified in the revised manuscript lines 268-269 as:
>
> "*After 26 kyr B.P., the decreasing pre-depositional age and low ICD-OC flux suggest that permanent sea ice may have acted as a barrier for transport of terrestrial matter to the study site*."

Line 274: I suggest to delete "in the context of sea ice"

> *"*In the context of sea ice" has been deleted and the sentence changed in the revised manuscript lines 309-310 as:

"*While permanent sea ice persisted during the B/A, there was a modest increase in terrestrial input, coupled with a notable increase in the pre-depositional age of OC, reaching approximately 15 kyr (Fig.2&5).*

Line 278: in addition to sea level rise, could it be that atmospheric warming has also contributed to the increased thawing of older permafrost deposits?

The role of atmospheric warming has been implemented in the revised manuscript lines 315-317 as:

"*This indicates that permafrost carbon remobilization during the B/A was primarily influenced by sea-level rise and atmospheric warming, rather than by sea ice conditions in our study location."*

Line 284: same comment as above for HS-1: permanent sea ice at site 31-PC but seasonal/ice-free conditions at more coastal sites in the Laptev Sea during the B/A permitting coastal erosion

We have clarified this in revised manuscript lines 320 to 329:

"This suggests a significant role of the permanent sea ice limiting coastal erosion during B/A close to the Southern Lomonosov Ridge. Indeed, the OC-fluxes were much larger in 31-PC when seasonal sea ice was present after DO-3 and the PB (Fig. 2&5). Additionally, nearby cores in the Eastern (PS2458; Fahl and Stein, 2012) and Western Laptev Sea (PS51-154/11; Hörner et al., 2016) display open water/seasonal sea ice conditions concurrent to input of pre-aged OC/n-alkanoic acids, but in several orders of magnitude higher fluxes than in 31-PC (Fig. 5; Nicolas et al., 2025). This suggests that the presence of perennial sea ice at the Southern Lomonosov Ridge, similar to the HS-1, served as a barrier to limit the transport of terrestrial derived material to the core location, and that coastal erosion was limited close to the site. In contrast, regional variability was evident in the Laptev Sea, where seasonal sea-ice permitted substantially greater transport of terrestrial organic carbon, partly due to reduced sea-ice cover facilitating enhanced coastal erosion (Fahl and Stein, 2012; Hörner et al., 2016).

Line 296: During the PB/EH transition,

This beginning of this sentence has been changed to "*During the PB/EH transition"* in revised manuscript line 338.

Line 300: I guess you mean "warm early (not late) Holocene" - however, Hörner et al. (2016) identify a decreasing sea ice cover during the early Holocene

"late" has been changed to "*early"* in revised manuscript line 342.

Line 309-311: looking at the pre-depositional ages of core 31-PC in figure 5, there is just one data point reflecting an old (ca. 15000 year) 14C-OC age at ca. 9 ka (while MWP-1B occured distinctly earlier at ca. 11.3 ka BP) – I suggest to rephrase this sentence

The sentence has been rephrased in revised manuscript lines 351-353 as:

"*Source apportionment of OC suggest a significant contribution from ICD-OC to 31-PC (Fig. 5; Martens et al., 2020). Due to cross shelf-transport of 0.6 to 2.6 kyr of the OC (Martens et al., 2020), the mobilization is likely linked to coastal erosion driven by sea-level rise during the MWP-1B (Lambeck et al., 2014)*."

Line 321: "...phytoplankton and sea ice algae productivity"

"marine" was changed to "*phytoplankton"* in revised manuscript line 363.

Line 336: "...using the sea ice biomarker IP25 and open-water algae biomarkers..."

"*the"* has been added before "sea ice biomarker" in revised manuscript line 379.

Figures

- *I suggest to change the order of figures 2 and 3 as figure 2 already contains an interpretation*

  The order of the figures has been changed and the citations referring to the figures corrected accordingly.

- *Figure 5: please explain the light green shading in panel (a)*

  The shadings in the figure are now explained in figure text 5b (as another panel a was added).

Anonymous referee #2: https://doi.org/10.5194/egusphere-2026-2414-RC2

***General Comments**:*

Eriksson et al. have produced an interesting and relevant dataset to investigate the connections between changes in sea-ice coverage on the southern Lomonosov Ridge and the input of terrestrial organic matter. The data are worthy of publications. The paper could use some polishing, some lines of evidence are not clearly laid out, and error estimates need to be produced in order to be able to fully diagnose the validity of claims made in the paper. I recommend the authors make moderate revisions to their manuscript.

In the methods section, the authors need to describe how they calculated parameters used in the paper like the limit of detection for biomarkers. Additionally, none of the figures they show contain uncertainty estimates for the biomarker concentrations/fluxes, for isotope ratios, or for derived parameters such as pre-depositional ^{14}C ages. All of the figures must include error bars or envelopes.

> Methods for calculating limit of detection have been inserted in Eq.3 of the revised manuscript. Error bars have been inserted in figures 2 (biomarker concentrations) and 5 (pre-depositional age) and are explained in the corresponding figure captions.

Section and/or Line-specific Comments

Methods

The age model for the core in this study was made in past papers but there are no details in this manuscript for what it is – it would be good to provide some cursory background – especially when it comes to things like stitching the multicore and piston core together. Were samples in this study only analyzed on the piston core or also the multicore? Perhaps the authors could provide some details about some characteristics for the cores, like the accumulation rates – not everyone who reads this paper will refer back to the previously published papers, and it is good to provide the reader at least some mild detail here.

> In terms of the already published age model for the piston core, we have now expanded the explanation in the current manuscript of how the age model was established, and included accumulation rates in the revised manuscript lines 102-108 as:
>
> *"The chronology of core 31-PC was developed through a combination of bulk radiocarbon dating and stratigraphic correlation with Greenland ice-core records, resulting in an age-model covering the last 27 kyr (Muschitiello et al., 2020; Martens et al., 2020). The age model was developed using a Markov Chain Monte Carlo algorithm to probabilistically align variations in sediment porosity with $\delta^{18}O$ records from Greenland ice cores, with eight bulk radiocarbon ages as maximum-age constraints. The age-model development is described in detail in Muschitiello et al. (2020). Sedimentation rates in 31-PC were on average 41±24 cm kyr^{-1}, apart from the past 9 kyr where they were lower (15 cm kyr^{-1}; Muschitiello et al., 2020)."*

Is the d13C of OC really from AMS? The AMS is often significantly fractionating such that the d13C values are not accurate. Have the d13C values of OC been verified with external accuracy standards?

> The $\delta^{13}C$-OC was validated through external isotopic standards for the EA-irMS and is now clarified in the revised manuscript lines 160-161 as:

"*The stable carbon isotopic composition (δ^{13}C-OC) in the surface sediment of 31-MUC was determined using an elemental analyser coupled to an isotopic ratio mass spectrometer (EA-irMS, Delta V Plus, Thermo Scientific, Germany) following the procedure in Martens et al. (2020).*

More details on the age model are needed to verify that equation 4 is not circular – the authors use the age model to predict the initial 14C of the samples – but are the 14C of the samples being used to calculate the age model? Does the age model derive from radiocarbon dates on, say, carbonates or other organic molecules rather than the OC dated here? Is the material being dated in this study TOC, TC, TIC, or something else? This needs to be explained more clearly and specifically in the methods section.

We have now clarified how the age-model was derived in a previous comment. In age model development, bulk ages were used as maximum limiting ages, yet the age-model is not solely based on that as elaborated in the earlier comment. Although they are related, it is not a circular argument due to the additional constraints other than radiocarbon in age-model development. We have now also revised section 2.3 so that it is evident that it is the pre-depositional age of organic carbon that is investigated.

Fig 3 – LOD is not defined in the methods section, how is this calculated?

The calculation of limit of detection of biomarkers are now clarified in the revised manuscript lines 154-158 as:

"*The limit of detection (LOD) for each biomarker was experimentally determined for each biomarker through external calibration curves of reference sediments (IP_{25}) and sterols (external standards) according to Eq. 5.*

$$LOD = 3.3 * SD_{xy}/\mathrm{b} \qquad (3)$$

Where SD_{xy} represents the residual standard deviation of the calibration curve and b is the slope of the calibration curve."

Results

Lines 155-160 – Be careful of phrasing. "Relatively" elevated – relative to what? "Massive" fluxes is subjective – for a results section there is some editorializing that should perhaps remain in the discussion – a description of how the values of different biomarkers change through time would suffice here.

This sentence is now rephrased in revised manuscript line 174 as:

"Elevated compared to the rest of 31-PC."

"Massive" has been changed to "*high*" in revised manuscript line 175.

Discussion

Lines 197-199: I am confused about the description here. It looks as if DO-3 is actually not recorded in the core, according to figure 3, but rather the time period immediately following DO-3. There are no data points in the yellow shaded area corresponding to DO-3. These lines also refer to "relatively low" PIP-25, however the PIP-25 values are nearly as high as the LGM. Don't the values of PIP-25 in the time interval following DO-3 simply indicate seasonal sea-ice coverage?

In the revised manuscript, we have clarified this now also in the discussion and fig.3 in revised manuscript lines 215-217 as: *"Given the age model uncertainty, the DO-3 event (28-27.5 kyr B.P), or the aftermaths of it, are likely captured at the base of 31-PC (spanning 27.2 ± 2.3/-1.4 kyr B.P, 90 % confidence interval; Muschitiello et al., 2020; Fig.2)."*

Line 200 – the timing of LGM sea-ice onset is roughly coincident with what Pavia et al. (2025) observed using the D(Th-He) proxy at the BC28 site on the Lomonosov Ridge.

Pavia et al. (2025) has now been implemented in the revised manuscript lines 256-257 as follows:

"*The onset of perennial sea ice cover during the LGM is consistent with recently published proxy records from the central Arctic Ocean (Pavia et al., 2025).*"

Line 205 – Why would nutrient input result from sea ice loss during this brief time period? Is it clear that this wouldn't lead to stratification and reduced nutrient supply from below?

During/in the aftermaths of DO-3 there is a peak in the input of marine organic carbon (Fig.5). Nutrient input from sea ice loss is one possible explanation, but likely not the only processes of importance. However, it is clear that a seasonal sea ice cover allows for more marine organic carbon input compared to times of perennial sea ice. In order to clarify this, an additional sentence has been added in the revised manuscript lines 226-228:

"*However, nutrient supply from terrestrial sources could also be an important contributor to increased productivity. "*

Section 4.2. – Again, this is consistent with the results of Pavia et al. 2025 for both the Mendeleev Ridge and the Lomonosov Ridge, inferring permanent sea ice coverage during the LGM and through HS-1.

We have now emphasized this according to the earlier comment about "line 200".

Lines 305-306 and Lines 325-326: The authors should be very precise about what they mean here. "Orbital forcing", implies that the authors are inferring a direct effect of increased solar radiation on the heat budget of the surface Arctic leading to sea ice melt. To prove this point, the authors could calculate whether the Early Holocene increasing in NH summer insolation is sufficient to supply the heat necessary to melt sea ice over a given area – I suspect they will find that it isn't enough. The authors likely mean that atmospheric temperature increases, rather than ocean warming or Atlantification, are responsible for the sea ice decline – which are also consistent with the results of Pavia et al. 2025.

In order to clarify these two points made by referee #2, "orbital forcing" is rephrased as "*increased solar insolation*". Additionally, lines 325-326 in the submitted manuscript are clarified in the revised manuscript lines 367-370, and throughout the manuscript as:

"*Dong et al. (2022) found that the primary driver for mid-Holocene sea ice reduction in the East Siberian Sea was increased solar insolation driving heat discharge from Arctic rivers. In accordance, the sea ice cover in our record as well as Dong et al. (2022) expanded in the Late Holocene, attributed to reduced summer insolation and resulting heat discharge from nearby rivers."*