Author response to referee comment and the resulting 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 for providing detailed comments and feedback on the manuscript. The constructive feedback has helped improved the scientific presentation and discussion of our interpretations.

All reviewer comments are included below in *black italic font* each followed by our detailed author responses, formatted as indented blue text. Citations of our implemented changes in the manuscript are formatted as indented *italic blue text*.

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.

> We thank referee #2 for their overall supportive assessment and for providing constructive feedback that has improved the scientific clarity of the manuscript. Particularly in regards to clarifying error estimates and calculations in the Methods section. The comments are addressed in detail below.

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.

> We thank referee #2 for suggesting to briefly outline how the age model was developed. Regarding the multicore, only the 0-1cm was used in this study, and represents 0 kyr B.P., I.e., the sampling year 2014.
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> 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 101-106 as:
>
> *"The chronology of core 31-PC was developed through a combination of bulk radiocarbon dating and stratigraphic correlation with ice-core records from Greenland, resulting in an age-model covering the last 27 kyr (Muschitiello et al., 2020; Martens et al., 2020). The age model was established through a Markov Chain Monte Carlo algorithm and Bayesian probabilistic alignment of the sediment core and Greenland ice-cores and 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?

We thank referee #2 for highlighting the δ^{13}C-OC methodology. The ^{14}C-OC was determined through accelerator mass spectrometry, and the δ^{13}C-OC was determined through an elemental analyzer coupled to an isotope ratio mass spectrometer (EA-irMS). The δ^{13}C-OC was validated through external isotopic standards for the EA-irMS and is now clarified in the revised manuscript lines 159-160 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 thank referee #2 for asking for clarifications regarding the age-model and its relation to the calculation of pre-depositional ^{14}C-ages. 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 152-156 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 173 as:

"Elevated in comparison to the rest of 31-PC."

"Massive" has been changed to "*high*" in revised manuscript line 174.

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?

> We thank referee #2 for asking for a clarification regarding DO-3. As we outlined to referee #1 the age model uncertainty for the core is 27.2 ± 2.3/-1.4 kyr B.P., which is also included in the submitted manuscript. Thus, the median age is 27.2 kyr, which is why the data point doesn't overlap with the DO-3 in the figures. However, as the uncertainty range encompasses DO-3, what is observed in the core is either the aftermaths of DO-3, or a direct result of it. In the revised manuscript, we have clarified this now also in the discussion as: "*The DO-3 event (28-27.5 kyr B.P), or the aftermaths of it, are likely recorded in 31-PC (spanning 27.2 ± 2.3/-1.4 kyr B.P; Muschitiello et al., 2020; Fig.3).*"

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.

> We thank referee #2 for pointing this out and providing this useful reference. Pavia et al. (2025) has now been implemented in the revised manuscript lines 259-260 as follows:
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> "*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 228-229:
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> "*However, nutrient supply from terrestrial sources could also be an important contributor to the observation of 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 thank referee #2 again for highlighting the observational consistency to central Arctic Ocean sea ice records. 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.*

We thank referee #2 for suggesting a clarification regarding "orbital forcing". Lines 305-306 in the submitted manuscript refers to findings of Stranne et al. (2014), where the perennial sea ice breakdown is attributed to the Early Holocene Insolation Maximum. As for lines 325-326, the sentence also refers to published work from Dong et al. (2022) as clarified in the submitted manuscript. In this work, they attribute East Siberian Sea ice retreat in the mid-Holocene to increased heat discharge due to the higher summer solar insolation.

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 360-364 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."*