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/

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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 #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.

> We are grateful to referee #1 for recognizing the novel contributions of the current study, in particular the link between permafrost carbon mobilization and the sea ice cover. The suggestions of specific references help to improve our interpretations of the data and understanding of the Laptev Sea deglaciation period and are thus greatly appreciated. We highlight that sea ice is of importance during this period, but cannot distinguish the relative importance of the two with this record and are thus not adding the sea level rise as an additional graph. The comments are addressed in detail below.

Line 10: something is wrong with the wording here

> We agree. This has been rewritten in the revised manuscript lines 9-10 as:
>
> "*Sea ice plays a central role in the polar climate system. In recent decades, the rapid decline of Arctic sea ice has triggered cascading results on the albedo effect and gas/heat transfer in polar regions."*

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*".

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)

We thank referee #1 for pointing out the important role of sea ice on atmospheric circulation and the transfer of heat/moisture to permafrost landscapes, and for providing this useful reference. This is now implemented in revised manuscript lines 41-43 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

We thank referee #1 for this distinction. "Abrupt climate change" has now been removed from the sentence. We had a hard time finding another appropriate study to cite and kept the reference. To clarify that it is not during periods of abrupt climate change the sentence is now clarified in revised manuscript line 40 as:

"*Paleoclimate evidence implies that this could have been important in past climates with reduced sea ice conditions enhancing permafrost thawing, leading to greenhouse gas releases (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 54 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)".

We agree with the clarification suggested by referee #1 and have implemented the suggestion in revised manuscript lines 54-57 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 74.

Line 120: replace 'content' with 'compounds'

"Contents" was replaced with "*compounds"* in revised manuscript line 125.

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 is an important point brought up by referee #1 and it is indeed crucial for accurate identification of IP_{25}. The reference sediments and subsamples from 31-PC were analyzed in full-scan to ensure accurate identification of IP_{25}. Thereafter, all samples from 31-PC were analyzed in the same batch to ensure correct identification.

This has been clarified in revised manuscript lines 139-140 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.

We thank referee #1 for acknowledging the importance of accurately providing quantitative comparisons to other sea ice record. Yes, we have determined instrumental response factors. As stated in the submitted manuscript in lines 136-137 we followed the method by Belt et al. (2012). To clarify, we quantified both HBIs and sterols according to Eq.3 in Belt et al. (2012):

$$IP_{25} = \frac{\left(\frac{P_a}{P_{is}}\right) \times RF}{Ms} \times M_{is}$$

Where Pa is the peak area of the analyte, P_{is} is the peak area of the internal standard, RF the instrumental response factor, M_s the mass of sediment extracted and M_{is} the mass of internal standard added to the sediments prior to extraction. The instrumental responses were verified using an external calibration curve of HBI reference sediment and the internal standards 7-HND and 9-OHD. In the case of sterols instrumental responses were determined using an external calibration curve with external standards for brassicasterol, campesterol, B-sitosterol versus the internal standard androstanol.

To clarify the quantification process in the manuscript lines 136-137 were changed in the revised manuscript lines 142-145 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 150-152 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.

We thank referee #1 for suggesting a clarification of the DO-3 event and how it is recorded in 31-PC. As the age model uncertainty encompasses the DO-3 event at 27.2 ± 2.3/-1.4 kyr B.P., it is clear that the sea ice conditions in the bottom section of 31-PC are either the aftermath or occurring concurrent to DO-3. However, as 27.2 kyr B.P. is the median age we decided to plot this age as the oldest in the core even though the age uncertainty includes the DO-3. In lines 159 of the submitted manuscript this is clarified as "the aftermath of DO-3" to make this distinction of the age model uncertainty.

In the revised manuscript lines 217-219, we have also clarified this in the discussion 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; Muschitiello et al., 2020; Fig.3).*"

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

"Conditions" were changed to "*cover*" in the revised manuscript line 220.

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

"Conditions" were changed to "*cover*" in the revised manuscript line 224.

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 243.

Line 231: please explain HS-1

The abbreviation "HS-1" is now elaborated as "*Heinrich Stadial-1*" in the revised manuscript line 254.

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 256-257 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 262-264 as:

"… but it should be noted that the differences in sea ice cover also can 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.

We thank referee #1 for raising this point. The misunderstanding comes from differences in processes occurring at the different sites. Our core (31-PC) is located much further away from other Laptev Sea records that are described and therefore coastal erosion around our core location is likely much different. In 31-PC there is very low OC input during the time of perennial sea ice in comparison to when there was a seasonal sea ice cover/open water. Therefore, in the case of the Southern Lomonosov Ridge it is possible that the presence of permanent sea ice limited coastal erosion. However, as you mention, the coastal erosion at Laptev Sea sites could be different, this is evident from terrestrial biomarkers ~4x higher concentrations during HS-1 compared to those in 31-PC (Hörner et al., 2016; This study). It should also be noted that these other Laptev Sea records only include the HS-1, and not the LGM, so it might be that coastal erosion was limited during LGM at these sites as well but since there aren't sea ice reconstructions in the Laptev Sea covering the LGM it is unresolved.

A clarification of the HS-1 is now included in the revised manuscript lines 275-279 as:

"*While limited coastal erosion is evident at the Southern Lomonosov Ridge, seasonal sea ice cover in the Western Laptev Sea resulted in a stronger terrestrial input (Hörner et al., 2016). 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 the Southern Lomonosov Ridge. This highlights the role of perennial sea ice in limiting terrestrial carbon export during the HS-1 in 31-PC*."

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)

We thank referee #1 for pointing this out. At 26 kyr B.P. is when the pre-depositional age is the highest, and the related OC-flux is relatively high. However, after this (LGM to HS-1), the ICD-OC flux is rather low (Fig. 5a) and the pre-depositional age is decreasing, pointing towards a low contribution of OC mobilization from coastal erosion. This sentence has been revised and clarified in the revised manuscript lines 272-273 as:

"*After 26 kyr B.P., the decreasing pre-depositional age and low ICD OC-flux suggest that permanent sea ice could serve as a barrier for transport of terrestrial matter into the marine environment*."

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 307-308 as:
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> "*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.3&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?

> This is an interesting question. The role of atmospheric warming has been implemented in this sentence in the revised manuscript line 314 as:
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> "*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 279: looking at figure 5, MWP-1A (14.5 – 14 ka BP) actually coincides with a drop in pre-depositional 14C ages...

> This is true for one of the two datapoints of pre-depositional age during this period. The first one displaying ~15 kyr old ages and the second one ~5 kyr. This points towards a mix of old ICD OC and active layer PF OC being mobilized and supports the hypothesis that shelf erosion from sea level rise eroded ICD OC and inland meltwater drove the active layer OC mobilization.

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 thank referee #1 for pointing this out. The point we want to make here is related to the referee's comment. In 31-PC the magnitude of terrOC mobilization is much lower during B/A compared to other events in 31-PC and in the Laptev Sea records. This observation supports the argument that the presence of sea ice limits coastal erosion in the vicinity of the 31-PC site, as the magnitude of terrOC is much higher in nearby Laptev Sea records where there is seasonal sea ice. In order to clarify this point, the sentence has been changed in the revised manuscript line 318 as:
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> "*… at the Southern Lomonosov Ridge."*

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 330.

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

> We thank referee #1 for pointing this out. "late" has been changed to "*early"* in revised manuscript line 334.

Line 303 and 304: again, Hörner et al. (2016) (and Lin et al. 2024) actually report a decrease in sea ice cover during the early Holocene. The PIP25 record of core PS51/154-11 shown in figure 4 is a bit *misleading here as Hörner et al. (2016) point out that additional (freshwater-derived) brassicasterol affects the interpretation of this lipid as phytoplankton marker in this time interval (which also limits its usefulness for calculating the PIP25 index). Maybe core PS51/159 discussed by Hörner et al. (2016) is better suited?*

We thank referee #1 for bringing this to our attention. In figure 4 of our submitted manuscript, we display both P_DIP_{25} and P_BIP_{25} (i.e., dinosterol and brassicasterol derived PIP_{25} indices). While it is apparent from Hörner et al., 2016 that there are brassicasterol sources from freshwater input (between 12.9 to 10.4 kyr B.P., not the early Holocene), the resulting PIP_{25} doesn't seem to be affected. Dinosterol is mainly related to marine sources and should be less affected by terrestrial input. The dinosterol PIP_{25} index aligns well with the brassicasterol PIP_{25} displayed in figure 4 and is therefore an accurate representation of what is presented in their record.

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 later arrival of OC in 31-PC related to MWP-1B was in Martens et al., 2020 suggested to be from a time-gap related to cross shelf transport of the material by 0.6-2.6 kyrs. The sentence has been rephrased in revised manuscript lines 343-346 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 356.

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 374.

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 5a.

References

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