**Revision overview on cp-2024-60**

We would like to thank the editor and the two reviewers for their precious suggestions, which significantly helped us to improve this manuscript. Following a suggestion from one of our co-authors, we replaced the Sd/Sl ratio with the Vd/Vl ratio to provide a better overview of vegetation degradation. This change was made because syringyl phenols (Sd and Sl) are predominantly found in angiosperm vascular plants, while vanillyl phenols (Vd and Vl) are present in both gymnosperm and angiosperm vascular plants (cf. Tesi et al., 2014). The correlation between the Sd/Sl and Vd/Vl ratios is high in both cores ($R = 0.97$, $p < 0.0001$ in core PS51/154, and $R = 0.94$, $p < 0.0001$ in core PS51/159, see the supplementary figure below this revision overview), so this substitution did not affect the interpretation of the data.

In the revised version, we include an additional degradation index using *n*-alkanes to supplement the Vd/Vl ratio, adopted the $C_{25}/(C_{25}+C_{29})$ ratio as a peat input index, and updated ΔR values to calibrate all the core records to the Marine20 curve. These changes in indices and ΔR values did not significantly alter the results, and the interpretation remain generally unchanged. We also excluded records from non-Arctic regions and those with lower temporal resolution to maintain focus on Arctic records. The format was checked, including reordering the in-text citations by year and double-checking the overall formatting.

Below, we outline manuscript revisions corresponding to each comment. Comments from the editor and the reviewers are in blue, and our responses are in black.

Dear Tsai-Wen Lin and co-authors,

Than you for submitting your responses to the reviewer comments. I invite you to resubmit your manuscript with all responses incorporated. Please submit both a track changes and a clean version of the updated manuscript, together with an overview of how the responses have been incorporated. I keep the option open to obtain a new evaluation after revision.

From your response to Reviewer 1, comment 2, it is not clear for me if you plan to include clarification/discussion on this point in the revised version, following the argumentation in your response. I will suggest that you do.

We thank the editor’s suggestion to include this part of the discussion. Please refer to our response to Reviewer 1, Comment 2.

I have one other minor comment that you may want to consider. In the introduction you say that "Studying these periods of rapid environmental change can improve the understanding of how current abrupt warming, sea ice loss, and sea-level rise might affect permafrost stability and the release of previously freeze-locked carbon". I cannot see that you follow up in this statement in your discussion/conclusion. Is it possible to add a sentence addressing implications of your results for our understanding of current changes?

We added a paragraph to the conclusion addressing projected permafrost mobilization under warming scenarios in lines 510−517.

Best regards,

Bjørg Risebnrobakken
Editor, Climate of the Past

**Revisions according to comments by reviewer 1 on cp-2024-60**

In this manuscript, Lin et al., evaluate changes in terrestrial organic matter input into the Arctic ocean over the last 18,000 years. The chosen study sites drain vast quantities of permafrost, such that enhanced terrestrial OC input might indicate enhanced permafrost thaw. The authors identify several pulses of terrestrial OC burial, some of which may correlate to known climatic events (e.g, meltwater pulse 1a). There is remarkably low terrOC input during the Holocene but lots of variability in terrOC delivery between 10 to 16 kyr. Each peak shows different compositional characteristics, suggesting distinct terrOM sources derived from different mechanisms. However, it would be interesting to try and unravel this further using your existing data (see comments below). Overall, the manuscript is well written, the figures are clear and the captions offer an appropriate level of detail. A few comments are included below that would be worth exploring further...

Comment 1: it is hard to tell whether the pulse in terrestrial OC is due to permafrost thaw or enhanced delivery of plant/soil OC. This could be assessed by measuring 14C values in different lipids, but I realize is beyond the scope of this paper. However, there is existing literature that could be helpful - for example, Feng et al. (2013; PNAS) explored how the 14C signature of different compound classes varied across the Pan-Arctic region and lignin phenols appear to mostly derive from recent carbon, whereas n-alkanes are derived from older carbon sources (e.g,. permafrost). Although you don't have 14C measurements, it could imply distinct sources for lignin vs n-alkanes in your samples.
...but another way to tackle this could be to look at other n-alkane indices such as the carbon preference index. This is frequently used to assess changes in OC maturity and may provide further insights into the type of terrOC that is being delivered into the marine realm, especially during the three pulses. If it was older OC, it may yield slightly lower CPI values. It may also tell you whether you are reworking old petrogenic OC into the marine realm too (which could be a CO2 source if it was oxidised; see work by Sparkes 2016 the Cryosphere, but also work by Bob Hilton/Valier Galy etc)

The calculation of the CPI index has been added in lines 212−217, and the CPI data is included in Fig 2c. The CPI results for cores PS51/154 and PS51/159, along with their source implication for permafrost rather than petrogenic sources, are discussed in lines 264−271. The discussion of how transport distance affects CPI data is included in lines 352−363. As noted in our response to the reviewer's comment, the ratio of lignin phenol to long-chain lipid contents in core PS51/154 was more influenced by a slumping event than by paleoenvironmental changes, so we didn't include the index.

Comment 2: Export vs preservation
It is important to confirm that the increase in leaf wax mass accumulation rates is not due to enhanced preservation but is reflecting enhanced terrOC export. This could be explored by calculating MARs of of short-chain alkanes (algal-derived), mid-chain alkanes (moss or macrophyte derived), and long-chain alkanes (vascular plant) n-alkanes during the three "pulses". If all three increase, it might suggest that OC preservation is the main driver. But if only the mid- and long-chain alkane MARs increase, it would imply enhanced terrOC input.

The discussion about terrOC export and preservation has been included in lines 320−330. Content changes for short-chain, mid-chain, and long-chain (HWM) *n*-alkanes are presented in Fig S6.

Comment 3: a note on pAq

The authors use the pAq index (the ratio between mid vs long chain n-alkanes) to infer changes in wetland input, but I would note that's its more complex than this. For example, both Sphagnum moss and aquatic macrophytes are characterized by similar lipid distributions (Baas et al., 2000; Ficken et al., 2000), so without knowledge of the local vegetation, its challenging to say whether the pAq ratio is due to changes in moss input or macrophyte input. Perhaps you could narrow this down by drawing upon predicted vegetation patterns during the Holocene/LGM etc. There is also great work by Jorien Vonk, Bart van Dongen, Orjan Gustafsson etc in similar pan-arctic regions that might be useful. For example, Vonk 2009 paper in Org. Geochem shows that "…the C25/(C25 + C29) n-alkane ratio is most suitable for terrestrial OM source apportionment in these coastal regions". This might be worth exploring alongside the pAq (although I suspect you will get similar results!).

References to vegetation changes and peatland development during the Preboreal and early Holocene in northern Siberia have been added in lines 342−344. We adopted the $C_{25}/(C_{25}+C_{29})$ *n*-alkane ratio, with the calculation method in lines 203−211 of the revised manuscript. Results are described in lines 262−265, and Fig 2b has been updated accordingly. The interpretation remains the same as that of the previously used $P_{aq}$ index due to the identical results between the two indices.

Minor comments:

L63: v/v – the v's should be italicised – change throughout

This has been corrected in lines 163, 166, and 170 in the revised manuscript.

L205: correct that its used for macrophytes – but also for sphagnum mosses.

The introduction of the $P_{aq}$ index has been replaced with an introduction to the $C_{25}/(C_{25}+C_{29})$ *n*-alkane ratio, as in lines 203−210 of the revised manuscript.

L252: is this statistically significant?

The modified sentence is in lines 257−258 of the revised manuscript. While the content remains unchanged, the sentence clarity has been improved.

L365: dialkyl

Please refer to the next response.

L366: and also found in peats – and lakes - and marine sediments!

The original sentence introducing brGDGTs has been removed from the revised manuscript, as the terrOM MAR record using brGDGT is not from the Arctic Ocean and has been excluded from the revised discussion.

L412: subscript CO2

This is corrected in line 441 of the revised manuscript.

**Revisions according to comments by reviewer 2 on cp-2024-60**

Summary

This paper presents organic geochemical proxies used to distinguish the source of organic matter (OM) in two 17-18 kyr records from the Laptev sea. The data is integrated with pan-Arctic records in an attempt to investigate local versus global drivers of enhanced OM flux during deglaciation. It is a very nice study and paper that fits well within the scope of Climate of the Past. However, I also think there is some room for improvement in he presentation and discussion of the data. Specifically, 1) a clearer presentation of sedimentation rate changes in the studied cores (and how they impact the mass accumulation rate estimates) is important, 2) a more in-depth discussion about the timing and link to OM flux/source changes associated with previously identified meltwater events in the Laptev Sea between MWP-1A and MWP1B. These are discussed further in the points below:

Specific comments

Line 25: "Additional terrOM MAR peaks coincided with periods of enhanced inland warming, prolonged ice-free conditions, and freshwater flooding, which varied between regions.". I wonder if this sentence could be more specific, for instance, be specific about what periods evidence for coastal erosion in response to sea-level rise are identified, and at what periods are inland warming and freshwater flooding seen? Also - maybe the last sentence could be re-written so that the expression 'regional terrOM fluxes' only occurs once.

The revised abstract, as proposed in the comment response, is presented in lines 22−26 of the revised manuscript.

Could the authors add a bit more information on how a dR value of -95 +/- 65 years for the Marine20 calibration curve was derived from Bauch et al, 2001? It is good to describe the conversion of old dR values when making them compatible with the Marine20 calibration curve, it is good bookkeeping.

Detailed information about the updated ΔR values for cores PS51/154 and PS51/159 has been incorporated in lines 140−145 of the revised manuscript.

Result, Section 4.1 chronology: "The mass accumulation rates (MARs) of all biomarkers were largely affected by the pronounced sedimentation rate changes and thus, showed similar temporal changes in all terrestrial biomarkers, including HMW n-alkanes, HMW fatty acids, and lignin phenols (Fig S3, contents of each biomarker in Fig S4). Fig 2a and Fig 5 i, j show the mass accumulation rate of HMW fatty acids as a representation.". Mass accumulation rates play a very important role in the environmental interpretations presented in this paper. In a number of areas the author's highlight how important the number of age-depth control points, and more generally changes in sedimentation rate, can impact these calculations. Furthermore, one of the most general questions about how offshore sedimentary processes responded to regional and global climate changes, concerns how the mass accumulation of sediments (sedimentation rate) changed. If there is an influx of material from land, one would expect that there would be enhanced deposition of sediments offshore. One question not answered in the current paper is - do we see this? I think it would be great if the authors added a 'sedimentation rate', or 'mass accumulation rate of sediments' panel in Figures 2 and potentially 5. The age depth models in the supplementary material do not really show the changes in sedimentation rate through time (not in enough detail), and it would be nice to clearly see how this impacting the calculated MAR's of other components (like TerrOM).

Sedimentation rates of cores PS51/154 and PS51/159 have been added to Fig S3. Additionally, changes in sedimentation rate across Arctic records are now included in Fig S7.

Line 253: "remained rather constant despite the periods of peak MAR (Fig S5)." I think you should specify what MARs you are discussing.

The modified sentence is in lines 257−258 of the revised manuscript.

Line 375: "Before the Bering Strait opened at around 11 kyr BP (Jakobsson et al., 2017), the coastlines of the Beaufort Sea and the Chukchi Sea were connected, allowing the potential westward transport of terrOM from North America. Therefore, we consider the record before 11 kyr BP from the Chukchi Sea (4-PC1) as a representation of terrOM signal from the North American Arctic.". I am not sure that I buy this argument. On one hand this paper is trying to disentangle regional from global climate drivers for TerrOM delivery to the Arctic, but then wants to use a record from the Herald Canyon in the Chukchi Sea as a proxy for deglacial processes operating across Arctic North America? Even when we look at the basic sedimentology, the deglacial records that have been published from the Canadian Beaufort Sea have a very high detrital carbonate content, which is not mirrored in the Chukchi Sea records. Maybe there is something I have misinterpreted, in which case it would be good to clarify what is meant in this sentence.

Record from the Chukchi Sea has been excluded from the revised discussion and Fig 5 to maintain better focus on comparing terrOM MAR peaks with periods of rapid sea-level rise.

Line 385: "Age-depth models for these records were recalibrated against the Marine20 calibration curve (Heaton et al., 2020) or a combination of Intcal20 (Reimer et al., 2020) and Marine20 curves, depending on the original studies to achieve congruent age control across all records. Reservoir ages were taken from the original publications." Is this accurate? or were reservoir corrections taken from the original publications and updated to fit with the Marine20 calibration curve by . . . and then specify how this was done.

The selection of updated ΔR values for calibrating against the Mairne20 curve in each core is described in lines 409−417 and Table S3 of the revised manuscript. Age models in Fig 5 have been updated accordingly. After updating to the new age models, the terrOM MAR peaks align more closely between cores, as well as between core records and the two periods of rapid sea-level rise. The discussion for core ARA04C/37 from the Canadian Arctic has been modified as described in lines 441−445, while interpretations of other records remain unchanged.

Line 407-409:"The rapid global sea-level rise during meltwater pulse 1A (mwp-1A) was an important process in terrOM mobilization across the pan-Arctic region. TerrOM MAR peaks during this period are observed widely in records from the Eurasian Arctic and the Bering Sea". The title of this section is 'Pan-

Arctic factor: sea-level rise' but no mention is made here of the Canadian Arctic/Beaufort Sea. I think it is hard to argue this without data from the Canadian Arctic, and it does not seem like that data exists (i.e in Fig 5, the Beaufort Sea records do not extend that far back in time). I can imagine that there may be a difference in glaciated versus non-glaciated margins etc. I at least think that this needs to be discussed in the text, as I am not at all convinced that the Chukchi Sea record is representative of North America. A core from the Herald canyon cannot tell us about all the processes operating across the northern coast of Canada.

Records from the Bering Sea have been excluded from the revised discussion to focus solely on the Arctic Ocean. The section title of Chapter 5.2.1 in line 435 has been modified to "Regionally recurrent factor: sea-level rise".

Lines 431-435: "In the North American Arctic, terrOM MAR peaks appeared during the interval between mwp-1A and mwp-1B (Fig 5d, e). Inland warming in North American began at approximately 13.5 kyr BP, while the Eurasian Arctic remained cold (Brosius et al., 2021). This regional temperature discrepancy possibly explains the exclusive terrOM MAR peaks observed in the North American Arctic during the interval between mwp-1A and mwp-1B (Fig 5c, g)". I think one of the most important observations that is not picked up in this paper is the link between TerrOM fluxes in the Beaufort and Chukchi seas and the d18O excursion reported by Spielhagen et al., 2005 in the outer Laptev Sea. All of these events appear to occur between MWP-1A and MWP-1B and there seems to be a coincidence in timing, even with some of the HMV Mar's in the Laptev Sea (PS51/154) and Fram Strait. However, this is hardly discussed in the paper and I wonder if it deserves more attention (see next comment)

As this comment is directly related to the next one, we have combined our responses below.

Lines 465-470: "However, freshwater events were less likely to be the cause for terrOM MAR in the western Laptev Sea. While freshwater flooding events were recorded in an icedammed lake upstream of the Lena River (14.9 ± 2.0 kyr BP) and in a sediment record from the Laptev Sea (PS2458, at 12.7 ky BP) (Spielhagen et al., 2005; Margold et al., 2018), the timing of these events did not 470 correspond with any of the terrOM MAR peaks in cores PS51/154 and PS51/159. This temporal mismatch suggests that Siberian freshwater pulses had little impact on the increase in terrestrial biomarker MAR in the western Laptev Sea.". The d18O peak in foraminfera from PS2458 (Fig 7 in Spielhagen et al 2005) does seem to overlap or is very close to some of the increased terrigenous biomarkers MAR's shown in figure 5 of this paper. I think it would add a lot to recalibrate the ages of PS2458, and plot this data on one of the summary figures. It seems to be an extremely complementary dataset to the goals of this study (looking at processes impacting terrigenous OC mobilization to the Arctic). The current arguments that it is not correlated to any of the documented periods of enhanced TerrOM flux is not really supported by the current presentation of data. It may be true – but can it be shown more clearly?

The comparison between freshwater events and terrOM MAR peaks in the Beaufort Sea and the Laptev Sea is presented in lines 482–492 and Fig S8 of the revised manuscript. All the records used for this comparison have been calibrated against the Marine20 curve, and the updated ΔR values are listed in Table S3.

**Supplementary figure**

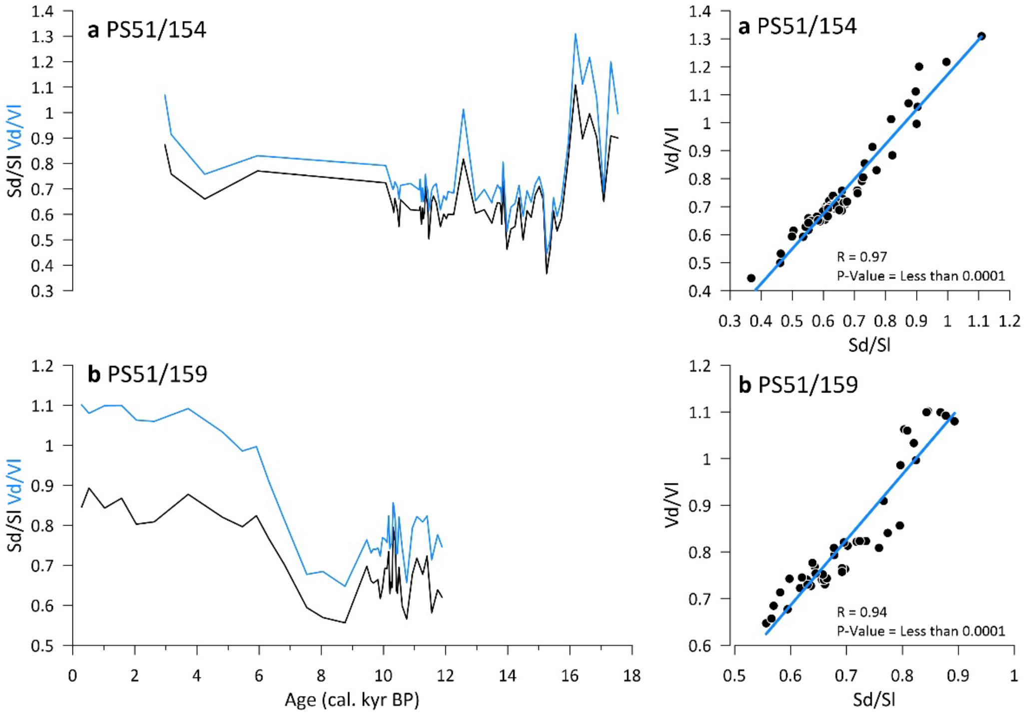


Figure 1. Sd/Sl (black lines) and Vd/Vl (blue lines) ratios for cores (**a**) PS51/154 and (**b**) PS51/159, and correlations between Sd/Sl and Vd/Vl ratios in cores (**c**) PS51/154 and (**d**) PS51/159.

**Reference**

Tesi, T., Semiletov, I., Hugelius, G., Dudarev, O., Kuhry, P., and Gustafsson, Ö.: Composition and fate of terrigenous organic matter along the Arctic land–ocean continuum in East Siberia: Insights from biomarkers and carbon isotopes, Geochimica et Cosmochimica Acta, 133, 235-256, 10.1016/j.gca.2014.02.045, 2014.