Dear reviewer,

Re: Manuscript ID: cp-2022-67 and Title: Deglacial records of terrigenous organic matter accumulation off the Yukon and Amur rivers based on lignin phenols and long-chain *n*-alkanes. by Mengli Cao et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-67-RC1, 2022

We thank you for your precious comments and suggestions concerning our manuscript. Those comments are all valuable and very helpful for revising and improving our study, as well as the important guiding significance to our researches. We have studied comments carefully and have made correction which we hope meet with approval.

General comments:

1. *In general, the introduction can be elaborated on, as it doesn't cover all used ratios at the moment. It should fine introduce the* TEX_{86} *ratio, and why a temperature reconstruction is needed to evaluate the sources of OM into marine sediments. Also, the S/V, C/V, 3,5Bd/V and Ad/Al ratios need to be introduced. How are they generally interpreted, and what records exist from the Arctic already?*

Response: We agree with the reviewer's comment regarding the introduction of our manuscript and realize the need to re-organize the text and include additional aspects. In the revised manuscript, we re-organized the introduction and believe that it now better explains why (biomarker-based) reconstructions of sea-surface temperatures and sea-ice extent are needed to obtain a better understanding of the deglacial changes in permafrost stability and vegetation development in the region. We will also include more detailed descriptions of the proxies used and what they can indicate (TEX_{86}^L, BIT, S/V, C/V, and so on), but we believe that the right place for these detailed descriptions is the methods section.

2. *As a second suggestion, I would restructure the discussion, starting with section 5.2. on the sources of terrigenous organic matter, that discusses the provenance of OM based on the fluxes. Following up on this, the impact on degradation on section 5.1. vegetation proxies can be discussed, potentially explaining a lot of the short-term variation that is especially apparent in the Yukon sediments. This way, the reader will leave with a well supported interpretation of the vegetation changes, which is the main aim of this manuscript. Then, both the changes in provenance and vegetation can be compared with the temperature and sea ice proxies.*

Response: We thank the reviewer for this constructive suggestion. We started with vegetation change in the discussion section because that is the main thrust of this study. However, we noticed that following the comments given by the reviewer may make it easier for readers to understand our main points step by step. Therefore, we will change the structure of the discussion section, discussing organic matter sources based on biomarker concentrations first, followed by vegetation development in the two basins.

In-line suggestions:

1. L 44. Unless one is familiar with the system, "beneath offshore arctic continental shelves" is difficult to understand.

Response: As a response to the reviewer's comment, we changed this sentence as follows: It occurs both on land and subsea in the Arctic and subarctic regions, and underlies about 22 % of the Earth's land surface (Brown et al., 2002; Wild et al., 2022).

2. *L 47. "have developed"*

Response: Changed.

3. L 49. 0–3 m soils = Surface 3m of soil? This sentence is a very specific comparison, but it's not clear why delta's and yedoma are included, or the reference to pre-industrial is made here. Also, is Yedoma deposit used here in reference to the Yedoma region mentioned above? The term is not introduced.

Response: This comment refers to the sentence "Across the northern circum-polar permafrost regions, Yedoma deposits, 0–3 m soils and deltas contain about twice as much carbon as the pre-industrial atmospheric carbon pool (Hugelius et al., 2014)." The "0–3 m soils" means 0–3 m depth range in soils, or the surface 3 m of soils. We want to briefly introduce the content of organic carbon stored in circum-polar soils. We agree that this sentence is not clear. Therefore, we revised it to the following:
Permafrost regions around the world store twice as much carbon as is contained in the atmosphere at present (Hugelius et al., 2014; Friedlingstein et al., 2020). Across the northern circum-polar permafrost regions, the surface permafrost carbon pool (0–3 m depth) amounts to 1035 ± 150 Pg (Hugelius et al., 2014).

4. L 70. Sakhalin peninsula and Hokkaido: are these areas in the same region? Include a short description of the geographical relation of these areas for the non-expert.

Response: In response to the reviewer's comment, we have made a brief introduction to the relationship between these two regions (Sakhalin peninsula and Hokkaido) and the region we want to study as follows: Several studies suggested major deglacial changes in the vegetation of permafrost-affected areas during the last deglaciation, including the Lena River basin (Tesi et al., 2016), the Yukon Territory (Fritz et al., 2012), the Amur River basin (Seki et al., 2012), and Sakhalin peninsula and Hokkaido (Igarashi and Zharov, 2011), the latter two bounding the Okhotsk sea to the Northwest and North.

5. L 88. Perhaps include that this ratio is based on microbial lipids? While the BIT index can be seen as an alternative proxy for soil-derived terrigenous OM (comparable to alkanes?) it is not quantified in this manuscript, is there a reason for this?

Response: The BIT index in this manuscript is mentioned in this sentence "According to lignin phenols and the so-called branched and isoprenoid tetraether index (BIT), Seki et al. (2014) found that terrestrial OM from the Amur River is a major source of OM in the North Pacific Ocean at present and that terrestrial OM in surface sediments is dominated by gymnosperms in the Okhotsk Sea." This sentence is an example to explain that lignin phenols can be used to assess vegetation development. We cited this paper by Seki et al. (2014), in which two biomarker parameters, lignin and BIT, were used. In the revised manuscript, this sentence will be deleted, and the BIT index will be included in the introduction and method sections.

6. L 114, Alnus was mentioned in the introduction, is this the 'birch' that is referred to here? For this manuscript, it would be interesting to mention whether these vegetation changes linked to permafrost degradation or warming? A link between vegetation and drainage features linked to permafrost degradation is mentioned in the introduction, but not revisited during the description of vegetation changes here.

Response: Birch belongs to *Betula*, and is one of the first trees to develop after the glacier retreated. *Aluns* grow in a warmer and wetter environment than birch. Therefore, pollen records indicate there were no significant changes in vegetation pattern from the LGM to about 16 ka BP, but after about 16 ka BP, birch pollen became significantly more abundant from western Alaska to the Mackenzie River (Bigelow, 2013). However, *Alnus* is a common genus in Yukon Holocene pollen records but far less common in interglacials (Schweger et al., 2011), suggesting both increasing summer temperature and moisture.

We fully agree with the reviewer that a link between vegetation changes and permafrost remobilization or climate warming should be mentioned where vegetation development is discussed in this manuscript.

7. L 110, refer to Fig. 1 here.

Response: It has been revised as follows: The Bering Sea is located in the north of the Pacific Ocean (Figure 1).

8. L 121, perhaps mention that this river drains parts of Russian Siberia and northern China?

Response: Thanks for your kind suggestion, more information about Amur River's basin has been included in the revised manuscript. See the following: The continental slope off Sakhalin Island in the Okhotsk Sea receives runoff from the Amur river, the largest river catchment in East Asia. The Amur is also one of the largest rivers in the world in terms of the annual total output of dissolved OM and substantially influences the formation of seasonal sea ice (Nakatsuka et al., 2004). The river originates in the western part of Northeast China and flows east forming the border between China and Russia.

9. L 127. Do the authors mean "The climate of the Amur Basin is largely determined by continental patterns from Asia, as the monsoon influences the amount of precipitation from the Pacific transport to this region during the summer."? Needs to be rewritten slightly.

Response: Thanks for your constructive suggestion, which is highly appreciated. We have revised this text and hope that it is now clearer. See the following: The climate of the Amur Basin is largely determined by continental patterns from Asia, as the

Asia monsoon influences the amount of precipitation from the Pacific transported to this region during the summer.

10. L 131. As suggested at L. 114; Do these previous studies make a link with the permafrost collapse?

Response: Different from the line 114 comment, we introduced the vegetation coverage in the Amur Basin at present here. The catchment of the Amur transitioned from complete permafrost coverage during the Last Glacial Maximum (LGM) to almost entirely permafrost-free conditions at present (Vandenberghe et al., 2014). Therefore, we think it might not necessary to include a link between vegetation development and permafrost collapse here. However, we will add a description of climate change from the last glacial to the deglaciation and Holocene in East Russia, and the change of vegetation in the Amur River Basin during the last interglacial period.

11. L 214. Was this ratio calculated for this manuscript, or already published before?

Response: The Paq ratios shown in our manuscript have been published by others. The Paq ratio of SO202-18-3/6 core was published by Meyer et al. (2019). The Paq ratio of SO178-13-6 core was published by Winterfeld et al. (2018). We also cited the Paq ratio of the XP07-C9 core (Seki et al., 2012), which is located in the Okhotsk Sea (Fig. 1) (lines 282–286, revised manuscript). The calculation of this ratio will be removed in the revised manuscript, but refer to these references when using them.

12. L 225: 1% deactivated SiO_2?

Response: Yes, it's 1% deactivated SiO_2. We will revise this text to address your concern.

13. L 232. Add the reference of the manuscript where this index was introduced (and not only at L 234).

Response: As a response to the reviewer's comment, references for all indices will be included where they first appear in this manuscript.

14. L 234. The TEX_{86} index can not be interpreted as a sea surface temperature proxy when soil input is large, as this will influence the TEX_{86} ratio values directly. Often, changes in the BIT index are interpreted, as a dominant marine Thaumarchaeotal source of the isoprenoid GDGTs will be reflected in low BIT index values. The BIT index should be reported at this setting where a significant soil input is expected (it can also be used as a proxy for soil-derived organic matter, especially when interpreted coupled to concentration changes in brGDGTs and crenarchaeol). At the least, this caveat of the TEX_{86} ratio should be mentioned in the text.

Response: We fully agree with the reviewer that sea surface temperature cannot be reliably reconstructed based on the TEX_{86}^{L} value in regions where the BIT index is high. This information will be introduced in the revised manuscript. Most BIT values in the Bering Sea are below the commonly assumed threshold value of 0.3, above which sea surface temperature reconstructions are potentially biased by terrigenous isoGDGTs (Weijers et al., 2006). We are confident that in our study area, marine-derived GDGTs dominate over terrigenous GDGTs, suggesting that TEX_{86}^{L} is not biased by terrigenous input. However, there are 9 samples BIT values higher than 0.3 from 13 ka BP to 10.5 ka BP, indicate that in these intervals, TEX_{86}^{L} may not reflect the sea surface temperature change.

In order to illustrate this, BIT index values will be included in the revised manuscript. This has an added benefit, as the BIT index also reflects the increase of terrestrial organic matter input from 13 to 10.5 ka BP in the Bering Sea, which agrees with our lignin results.

15. L 238. For the results section, please include very briefly why each parameter was reconstructed. This also allows to group the different ratios (used to reconstruct change in vegetation, vs change in degradation).

Response: As a response to the reviewer's comment, brief descriptions of these parameters will be included as follows: The S/V and C/V ratios can be used as vegetation development, angiosperm *vs*. gymnosperm, woody tissues *vs*. non-woody tissues (Fig. 4). The 3,5Bd/V and Paq ratios can be used to indicate the change of wetland in the study area. Similar to $(Ad/Al)_s$ and $(Ad/Al)_v$ ratios, S/V, C/V, and 3,5Bd/V ratios are also affected by degradation processes (Hedges et al., 1988; Otto and Simpson, 2006).

16. L 303. The discussion focuses on the interpretation of the ratios as vegetation markers, but only discusses the impact of provenance change afterwards. In my opinion, with this order, the reader is left to wonder whether the reconstructed vegetation changes are reliable after all. A more convincing order could be to determine the impact of sea level change on the ratios first, as sea level will dramatically impact the source and fate of the organic matter delivered to the sea floor. Here, I hypothesize that sea i) level drop = expected increase in erosion (coastal erosion, but also deeper incision of the rivers, delivering pre-aged organic matter that can/will reflect an older vegetation type, or as mentioned in the manuscript, melting of Yedoma with different S/V and C/V ratio values). Also, lower sea level (more oxic conditions), core location closer to river mouth. Then ii) sea level rise = development of anoxic conditions, better conservation. All these elements are of course mentioned in the manuscript, but not explicitly introduced as the framework in which these OM changes can be interpreted. I think this will allow to explain the shortterm changes in the vegetation ratios within the ED, BA and YD, as these seem to happen during (or rapidly following) changes in sea level. Then, the longer-term changes in the vegetation ratios can be interpreted as a change in vegetation. Following this suggestion through, the authors can discuss section 5.2. before section 5.1.

Instead of comparing downcore changes in vegetation and degradation proxies, perhaps a scatterplot would be more informative (plotting S/V or C/V vs 3,5Bd/V or Ad/Al ratio).

Response: Thanks a lot for the reviewer's comment. We think sea level change influences these proxies for vegetation reconstruction and organic matter transport in our study areas in two main ways: 1) the transport time of organic matter on the shelf or the distance between the river mouth and the study site and 2) the rate of coastal erosion.

For the first part, the change of transportation time for organic matter on the shelf can be reflected by degradation indices and radiocarbon dating. Although we don't use the radiocarbon method in this manuscript, we found no significant correlation between degradation parameters (Ad/Al) and sea level change in the two sediment cores. As mentioned in the manuscript, oxidative degradation of organic matter occurred mainly on land (Winterfeld et al., 2015). Therefore, the sea level change during the last deglaciation may have a certain impact on organic matter degradation in the Bering and Okhotsk Seas, but not much.

As the reviewer mentioned in the comment, we have discussed the impact of coastal erosion induced by sea level change on these lignin parameters. For example, in line 686–689, we mentioned coastal erosion during MWP-1B contributed to the increasing Ad/Al signals from early deglaciation to 10.5 ka BP in the Okhotsk Sea. However, the influence of coastal erosion on organic matter supplied from the different sources as reflected by increased vegetation and wetland indices is less discussed. Coastal erosion may also affect the transport of Alk and lignin from land to ocean in the Yukon and Amur Basins during the last deglaciation. We will strengthen the discussion of the effects of coastal erosion on the relative contribution of organic matter from the different sources and the transport of biomarkers. We think that sea level change can be used as an indicator for coastal erosion in this manuscript. We do, however, not think that there is a need to include a separate section in the discussion about the effect of sea level change on the biomarker parameters. However, same as general comment 2, we agree with this referee that section 5.2 should be discussed first to make this manuscript easier to understand. The structure of the discussion section has been revised based on the valuable comments of the reviewer.

We tried scatter plots before, plotting S/V or C/V vs. 3,5Bd/V or Ad/Al ratio, but we found no significant correlation between these parameters. We thus did not show these scatterplots in this manuscript.

17. L 313. Here, the authors assume that all n-alkanes are derived from the continent, without contribution from the marine primary productivity. Is this dominant source from soil OM supported by fi bulk organic matter properties ($\delta^{13}C$)?

Response: Thanks for the reviewer's comment. The *n*-alkanes used in this manuscript are only mid- to long-chain *n*-alkanes, but we failed to stress this in the manuscript. Combine with the second reviewer's comment, the HMW Alk of the Okhotsk Sea will be recalculated to bring it in line with that of the Bering Sea (C_{23}, C_{25}, C_{27}, C_{29}, C_{31}, and C_{33}). According to previous studies, the odd-numbered *n*-alkanes in the range of C_{23} to C_{33} are almost exclusively terrigenous (Eglinton and Hamilton, 1967; Otto and Simpson, 2005). Therefore, we can use the HMW Alk to reflect the contribution of terrigenous organic matter. To further quantify the contribution of marine organic matter in sediments, the $\delta^{13}C$ of bulk organic carbon and compound-specific radiocarbon analysis of lignin phenols for the two sediment cores will be analyzed in our next manuscript.

18. L 375. Degradation, or a higher sea level?

Response: These indices (S/V, C/V, and Paq) are influenced to some extent by degradation progresses. The distance between the river mouth and study site will increase when sea level rises, which will result in increased transport time for terrigenous organic matter on the shelf. As mentioned in the manuscript, oxidative degradation of organic matter occurs mainly on land in permafrost regions (Winterfeld et al., 2015). Therefore, the time of organic matter transport on the shelf may have little effect on its degradation, which means the impact of sea-level rise on these parameters is limited. In addition, if these parameters are mainly affected by sea level change, then they should be maximized in the B/A, not in the PB. We will include what the effects of sea level rise would be on these parameters before this conclusion in the revised manuscript.

19. L 474. In general, I miss the impact of the distance to the river mouth in this part of the discussion. Can the authors constrain this, i.e. how much closer was the river mouth during low sea level stands, based for instance on what is known from sea level rise and current ocean floor bathymetry?

Response: Yes, the distance of our study sites to the river mouth changed between the early deglaciation and the present, more so in the Bering than in the Okhotsk Sea. We will discuss the sea-level change for the Bering Sea from 20 ka BP to the present (Manley, 2002) in the revised manuscript, but we are not aware of a published local reconstruction of sea level change for the Okhotsk Sea during the same time. However, we do not think that this change in this distance exerts a strong control on the lignin proxies. Since the previous study suggests that the oxidative degradation of organic matter occurred mainly on land (Winterfeld et al., 2015). Thanks to the reviewer's comments, we will include the discussion of the effects of sea level change on these parameters in the revised manuscript (see comments 16 and 18).

20. L 483. Mention those values here in-line.

Response: Done; sentence changed to "….relatively high S/V and C/V ratios ranging from 0.47 to 1.01, and from 0.03 to 0.82, respectively (Tesi et al., 2014), indicating….". These values were also shown in Fig. 5 and 6.

21. L 507, 508. Very interesting and important observation!

Response: Thank you for your recognition.

22. L 912. Not sure if this exists, but are more local reconstructions of sea-level change available? Does rebound play a role here, possibly causing a mismatch between the global sea level rise and the local conditions?

Response: The data on global sea level change comes from Lambeck et al. (2014), which reflects global mean sea level change during the last deglaciation. We will include the local reconstruction of sea-level change for the Bering Sea from 20 ka BP (Manley, 2002) to present in the revised manuscript. We found the rate of sea level change in the Bering Sea is slower than the global average rate. However, we are not aware of a published local reconstruction of sea level change for the Okhotsk Sea during the same time. The shelf of the Okhotsk Sea is narrower than that of the Bering

Sea, so the effect of sea level change on the Okhotsk shelf may not be as strong as the Bering shelf.

We sincerely hope that these responses have addressed all your comments and suggestions. We really appreciate your efforts in reviewing our manuscript during this unprecedented and challenging time. Your careful review has helped to make our study clearer and more comprehensive.

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