Climate of the Past Discussions

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Comment on cp-2022-67

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Community comment on "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-CC1, 2022

I have reviewed the manuscript "Deglacial records of terrigenous organic matter accumulation off the Yukon and Amur rivers based on lignin phenols and long-chain n-alkanes", submitted to Climate of the Past by Mengli Cao and co-authors. It's a very interesting study based on two cores that record environmental changes during the shift from the last glacial and Holocene conditions. They present novel lignin biomarkers fluxes and lignin ratios, and interpret these in a very multi-disciplinary discussion, that encompasses pollen-based vegetation reconstruction, biomarker based sea ice, temperature and wetland reconstructions, and modern lignin ratio datasets. They link changes in the timing of terrigenous organic matter fluxes to the marine system to the timing of local sea ice loss. The reconstruction of vegetation based on the lignin ratios is more complicated, and a large effort is made to discuss the impacts of degradation on these proxies. Still, I have included a few suggestions to make both the introduction and the discussion more clear.

General comments:

In general, the introduction can be elaborated on, as it doesn't cover all used ratios at the moment. It should fi introduce the TEX86 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?

As a second suggestion, I would restructure the discussion, starting with section 5.2. on the sources of terrgenous 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. Tis 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.

I find that the conclusion supports the data very well.

In-line suggestions:

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

L 47. “have developed”

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.

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.

L 83.

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?

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.

L 110, refer to Fig. 1 here.

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

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.

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

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

L 225: 1% deactivated SiO2?

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

L 234. The TEX86 index can not be interpreted as a sea surface temperature proxy when soil input is large, as this will influence the TEX86 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 TEX86 ratio should be mentioned in the text.

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).

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 short-term 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)

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 (d13C)?

L 375. Degradation, or a higher sea level?

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?

L 483. Mention those values here in-line.

L 507, 508. Very interesting and important observation!

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?