Review of Cao et al. "Deglacial records of terrigenous organic matter accumulation off the Yukon and Amur rivers based on lignin phenols and long-chain n-alkanes" (cp-2022-67)

<u>Synopsis</u>

The primary focus of this study is to reconstruct vegetation changes in the Okhotsk and Bearing Seas (near the Amur and Yukon Rivers, respectively) since the end of the last glacial period. To do so, the authors report lignin concentrations and molecular compositions from two marine cores, and they compare these to previously published *n*-alkyl lipid records from nearby cores. The authors conclude that vegetation change initiated sooner in the Yukon Basin than in the Amur Basin, and that wetland extent stabilized after the Preboreal. Importantly, the authors also conclude that lignin and *n*-alkyl lipids experience similar accumulation rates at this time, implying a similar transport pathway and delivery mechanism, which contrasts with results on export pathways in modern Arctic river systems.

Overall, I find that this study is interesting and provides a high-quality dataset of lignin concentrations, molecular ratios, and accumulation rates. The results will be of interest to those studying Arctic systems, permafrost mobilization/loss, and terrestrial biomarkers. That said, I concur with the other two reviewers that there is a bit of confusion when reading this manuscript as to what is new vs. what is compiled from previous studies. I highlight this and other general comments below, noting that I do not belabor the points that are already well-articulated in the other two reviews. Once these issues have been sorted, then I fully support this publication of this manuscript in *Climate of the Past*. Please do not hesitate to contact me with any questions regarding this review.

Sincerely,

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General comments:

L38: It is worth stating in the abstract what is the consequence of "both types of terrestrial biomarkers [being] delivered by the same transport pathway." This contrasts with the modern river system; introducing this contrast (and a short sentence on proposed reasons why) would fit nicely at the end of the abstract.

L51: Add comma after "Holocene"

L76: Of course, the "predominance of the odd carbon number homologues" in *n*-alkyl lipids is only true for alkanes---alcohols and alkanoic acids exhibit the opposite preference!

L79-85: The authors should clarify that this discussion on the difference in transport pathways between alkanes and lignin refers specifically to the modern systems---which they contrast with their own results in the discussion.

L87: I second reviewer 1's opinion that the BIT index should be removed here. As far as I can tell, it is never mentioned again and, given the huge complexity and uncertainty in using this as a terrestrial OC source indicator, this brief mention only raises more questions than it answers.

Sec. 1: Overall, I agree with both previous reviewers (esp. articulated by reviewer 2) that the introduction should be reworded to clearly articulate what is new to this study and what is derived from the literature. This should also mention all of the ratios and metrics that will be used throughout this study, and briefly state their interpretation (e.g., Paq is currently not introduced as a proxy for wetland expansion until Sec. 3.2, and the interpretation of various lignin ratios is currently not clear until the discussion!)

L105: LGM not yet defined.

L187-190: The inclusion of "some other oxidation products that do not necessarily originate from lignin" appears rather out-of-the-blue here, but these compounds are discussed later on. I therefore suggest at least mentioning these compounds and their utility in the introduction so that a reader knows to expect them.

L202-205: I strongly suggest re-writing these "equations" to be proper equations, not just words written in pseudo-equation form. Something like:

"…calculated as follows:

$$\mathrm{MAR} = \mathrm{SR} \times \rho, \qquad \text{(Eq. 1)}$$

where MAR is the mass accumulation rate in g cm^{-2} a^{-1}, SR is the sedimentation rate in cm a^{-1}, and ρ is the dry bulk density in g cm^{-3}." Etc. This would greatly simplify the reader's ability to interpret these calculations. Further down, the Paq and TEX "equations" should be restructured similarly.

L212: Again, I would mention Paq in the introduction since this comes a bit out-of-the-blue here. This would all be clarified with a re-write of the introduction to more clearly articulate what is new and what is taken from the literature.

L215: I concur with the comments of reviewer 2 for this section.

L250: Should this be “11.3 ka”?

L275-283: It’s not clear to the reader at this point what the “3,5Bd/V” ratio represents or why it is important. As mentioned above, these types of ratios should be first introduced (including their utility and interpretation) in the introduction.

L284-294: My above comment also applies for Ad/Al ratios. These should be mentioned earlier so the reader knows how to interpret, e.g., a range of “0.19 to 0.80”.

L296: Are TEX_{86}-derived SSTs *really* reliable to one 100^{th} of a degree? I suggest adjusting reported precision and honestly reporting TEX_{86} uncertainty here.

Sec. 5.1-5.2: I concur with reviewer 1’s suggestion to restructure these sections to begin with a discussion on terrestrial OM sources, fluxes, accumulation rates, etc. and *then* move into an interpretation of these biomarker ratios, including potential impacts of degradation.

L555-556: This is an interesting finding, but it is only mentioned in passing here. Why do the authors think these delivery mechanisms have changed relative to the modern? I would appreciate a bit more discussion on this topic, as I think some reviewers will find it highly relevant.