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Interactive comment on “Multi-molecular tracers of terrestrial carbon transfer across the pan-Arctic – Part 1: Comparison of hydrolysable components with plant wax lipids and lignin phenols” by X. Feng et al.

Anonymous Referee #2

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General comments: Understanding the fate of carbon in high latitude watersheds, which contain a significant fraction of the global soil organic carbon and above ground biomass, is critical in an environment that is experiencing a rapidly warming climate. This study adds new insights by characterizing the biomarker composition of sediment organic matter in major and minor river deltas in the Arctic region. As such, this manuscript is a valuable contribution to our understanding of fundamental carbon exchange processes. I have a few suggestions to make this manuscript more appealing to the general readership of “Biogeosciences”.

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Specific comments: Introduction – I would suggest to be more specific, quantitatively, in the introduction. How much carbon is stored in the general watershed areas, how much carbon is exported from the different rivers, including the dissolved and particulate pool, and how does that compare to net primary production in these watersheds. Also, how much sediment (TSS) is exported from the different rivers, this might be interesting in terms of the relative contribution of root material? Some of this information can be summarized in table 1, but should be part of the introduction as well.

Methods – The samples compared in this study have been collected from estuarine sediments, mid-shelf sediments and from river ice. There needs to be a section in methods discussing the potential consequences for the biomarker composition. The authors need to present an explanation, why they think these samples are directly comparable to each other and potential biases resulting from such a heterogeneous sample set. The authors also used 2 different extraction methods for different samples, what is the potential error introduced by the different sample preparation methods. Has there been a direct comparison between the 2 methods?

Results - The results and discussion section would benefit from more discussion of the data. One of the main points in this manuscript is the difference between the GRAR and North American rivers with the NA rivers having more root derived and less litter derived OC. What is the mechanism for such difference? Is it the fact that NA rivers carry more sediment (erosion), if so the relative abundance of root derived OC should be related to the TSS loads in the rivers. Did the authors look at such relationship? This would be a potential mechanism explaining the difference and an interesting addition.

Could the information in figure 2 be summarized in table form, including standard deviations? This would reduce the number of Figures in the manuscript.

The discussion of acid to aldehyde ratios being either an indicator for source or degree of oxidation could use some refinement. If used as an indicator for degradation than the western Eurasian rivers should be less degraded because of the higher abundance of

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wetlands, however, the data show that the eastern river sediments have a higher OC content and the authors argue for the colder temperature and dryer climate as the main reasons. Acid/aldehyde ratios are also affected by preferential leaching as indicated by the authors, why would the acid/aldehyde ratio of vanillyl phenols be affected differently than the syringyl phenols? This does not seem to be reflected in the leached dissolved fractions presented by Hernes et al. (2007).

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