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# *Interactive comment on* "Characterization of particulate organic matter in the Lena River Delta and adjacent nearshore zone, NE Siberia – Part 1: Lignin-derived phenol compositions" *by* M. Winterfeld et al.

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In this paper Winterfield and coauthors presented new biogeochemical data from the Lena river delta and Buor Khaya bay. Samples include suspended material from the river, soils from the delta's terraces and surface sediments collected in the Buor Khaya bay. This is the first of the two papers dealing with the carbon cycling in the Lena region and it focuses on the TerrOC which is supplied by the Lena and accumulates in the adjacent coastal region. The present study uses the fingerprint of lignin phenols to constrain source, concentration and degradation of terrigenous material. The second



paper presents complementary information on the same set of samples (except for the Buor Khaya bay sediments) and it focuses on the bulk organic matter composition based on carbon stable isotopes and radiocarbon measurements.

Despite the growing awareness of climate-induced change in the Arctic, high latitude regions still suffer from a thin database due to the evident logistical problems with collecting river and sediment samples in these remote regions. In this respect, this work contributes towards filling this gap. I enjoyed reading this paper and I would like to see it published. On a critical note, I found the choice of the authors to keep the two datasets (paper #1 and #2) completely separated too extreme. I understand that the two papers have different focuses and objectives which were well presented in both of papers. However, I believe that the results obtained from the CuO reaction products would have significantly benefited from the 13C and 14C data, especially during the discussion on the source and degradation of terrOC. At this stage I am not asking the authors to include the bulk data in the final version of the paper but at I'd like, at the very least, to see more information from paper #2 in the new version. The two papers almost read as two independent stories yet there are points where the bulk data can sensibly contribute to the discussion. I have noted these points in the text and I have included them with the other comments below.

Specific comments:

- Page 14377 line 25: ". . .it is not possible to draw meaningful conclusion based on this one spring flood measurement". This statement sounds somewhat defensive. At the beginning of the discussion, the authors made a very good point highlighting the different transport conditions which characterize the spring freshet and the summer period. The difference in TSM between the summer time-series (this study and Fedorova et al., 2013) and the datum presented here still reveals that timing is crucial, especially because a significant fraction of TerrOC is supplied during the freshet. I think that our current understanding of TerrOC flux to the Arctic Ocean is biased by the sampling because the concentration and composition of the particulate material supplied during

the freshet is poorly characterized. Indeed, I am not surprised that this spring sample (sample 37) has a distinct bulk composition (d13C and D14C) compared to the river end-member chosen in previous mixing model exercises (e.g. Karlsson et al., 2011). That said, one datum is clearly not enough to constrain the TerrOC flux to the Laptev Sea but the differences presented here should be discussed more in terms of lack of resolution in a system which is essentially event-driven. That's why I would replace the statement above and end the paragraph in line with the initial discussion about the seasonality of the river supply.

- Page 14370 line 24: be consistent with the terminology of P products in the text. The authors use para-hydroxybenzenes in the method and p-hydroxy phenols in the discussion.

- Page 14378 line 8-12: here it would be interesting to compare the results from paper #2 with the lignin concentrations. If the lower lignin content is indeed the result of the dilution of soil OC with river phytoplankton, I would show some numbers in the text to illustrate the relative proportion of phytoplankton in these samples (i.e., F_plankton, equation#1 paper#2). Also, differences in C/N ratio and lignin content can be simply driven by the relative proportion between vascular plant debris and mineral soil (e.g. Goni et al 2003 ECSS). This part of the discussion should either include this possibility or argue against it.

- Page 14379 line 14: be careful when comparing data by Amon et al 2012 and this dataset. Amon et al characterized the composition of dissolved TerrOC which has modern 14C age reflecting therefore a different source compared to the particulate material in suspension which is up to a few thousand years old. Despite the fact that there is potential exchange between the dissolved and particulate phases, the relationship between the two carbon pools is not so obvious to me. When comparing the two datasets, make sure that the reader understands that you are comparing modern TerrOC with pre-aged material old material (refer to paper #2).



- Page 14379 line 24-34: if the suspended material in summer is affected by phytoplankton as previously stated by the authors, the relatively increase of the P/V ratio would simply reflect the increase of the proteinaceous fraction rather than a change in vegetation. See for example the P yields in marine phytoplankton (Goni and Hedges 1995, GCA).

- Chapter 4.2.1: I might be missing something but I cannot find the sample ID 21 in tables or figures. This surface sediment was apparently was collected off the Muostakh island. Based on the map, this sample should be L09-34 instead. In addition, this sample doesn't display low lignin content compared to the rest of the samples as stated by the authors. Please revise.

Page 14383 line 1-5: this part reads as if the material depositing in surface sediment entirely derives from the watershed while it's well known that the Buor-Khaya bay is affected by intense erosion of ice complex deposits (Vonk et al 2010, Karlsson et al 2011, and many more) as also mentioned in the first part of the manuscript. It confuses me that this aspect is completely ignored from here on. For example, in discussing the C/V and S/V ratios the authors bypassed the importance of coastal erosion. For a comparison with soil profiles from erosional spots in the Buor-Khaya bay please see Tesi et al 2014 (GCA). Here we analyzed the composition of different soil samples from Muostakh island and Buor-Khaya Cape using alkaline CuO oxidations. I am sure that the interpretation of the lignin results in surface sediments will benefit from a discussion that encompasses both river and coastal erosion input. See also my next comments about this.

- Page 14383 line 6-15: see my previous comments on the proteinaceous source of P products and the limitations of the P/V ratio as vegetation proxy in marine and freshwater environments. These P/V trends observed might be driven by the TerrOC source as stated by the authors but the potential input by phytoplankton cannot be entirely excluded. Please modify the text accordingly.

- Page 14384 line 28: “. . .assuming that the ice complex deposit of Muostakh island. . .”. Lignin data from Muostakh island are available in Tesi et al 2014.

- Page 14385 line 9:” “More data on lignin composition of the ice complex deposit at various location is necessary. . .”. See comment above.

- Chapter 4.3.1 and conclusions. Here again the input of TerrOC via coastal erosion was ignored. The authors argue that the small tundra domain (about 10%) exerts first order control in the supply of angiosperm tissues. However, the ice complex deposit which is being eroded in Muostakh island and Buor-Khaya Cape (Tesi et al 2014) display S/V and C/V ratios (about 0.6 and 0.2, respectively) consistent with the lignin fingerprint observed in Buor-Khaya bay sediments. Therefore, in addition to trapping gymnosperm material in the floodplain (which can potentially occur), it’s clear that the composition of surface sediments is also affected by coastal erosion processes which result in diluting the original gymnosperm signal from the watershed.

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