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## ***Interactive comment on “Characterization of particulate organic matter in the Lena River Delta and adjacent nearshore zone, NE Siberia – Part 2: Radiocarbon inventories” by M. Winterfeld and G. Mollenhauer***

**Anonymous Referee #2**

Received and published: 30 October 2014

Winterfeld and Mollenhauer present an interesting isotopic/geochemical dataset of particulate organic matter and soil organic matter from the Lena River delta region. They present some of the first  $^{14}\text{C}$  data on particulate OM in the Russian Arctic, a rapidly warming region where a better understanding of land-to-ocean OM transport is very important. The manuscript is well written but there are a few important points that are currently lacking in the study, and I think these should be addressed before I would support publication in Biogeosciences.

Main points: - Interpretation of river OM geochemistry can in my opinion not be done

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without information on hydrology (i.e. discharge). I do not see anything on this topic in the manuscript. The differences in radiocarbon age, POC concentrations, POC:PN values could very well be related to timing and intensity of the freshet (how "flushed out" is the system in August?), the discharge later in summer/early fall (are there precipitation-event related peaks? is there still enough "force" for active bank erosion), etc. It is also important to know whether 2009, 2010 and 2011 are anomalous or normal years with respect to river discharge. - The authors argue that the results are useful for dual-carbon-isotope simulation studies in the region focusing on unraveling source contributions. I agree with that. But, I think the authors miss out on a good opportunity: to do this themselves with the three end-members for the Lena River. As most of the POC data are from the delta and the river, I think the authors should ignore the marine end-member, and run simulations (preferably with Monte Carlo) with the three "fluvial" end-members: (1) Ice complex deposits, (2) surface soils, and (3) fluvial plankton. A sturdy assessment of the associated uncertainties would be needed though (e.g. this is now not done at all for the plankton end-member of 49 per mille). In the manuscript they now only make calculations with two end-members (page 14431). An attempt to assess the relative contributions of these three end-members for the Lena River would, despite the associated uncertainties, be very valuable. I feel that the authors can do more with the data than they currently present (all focused on source-apportionment), and this is only one suggestion.

Further comments: P. 14415, line 24: There are in fact also a few  $^{14}\text{C}$ -POC values published from the Lena River: between -220 and -350 per mille in Vonk et al. Biogeosciences 7, 2010.

P.14418, line 17: I find the information given on the soil profiles very minimal. Can you give a short description of the sites and the soils? Holocene/Ice Complex? Actively eroding sites or not? Also, you talk about "first delta". What does that mean?

P. 14418, line 21: "... was removed with a spade for the total height of each bluff". You mean that you did this before you sampled the frozen soil that was behind the thawed

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material? Please elaborate a bit in the text.

P. 14418, line 21: "peat"? Did you only sample peat?

P. 14418, line 26: Glass fibre filters (GF/F) have a nominal pore size of 0.7  $\mu\text{m}$ , and not 0.45 $\mu\text{m}$ .

P. 14419, line 22: You write that you submitted the sampled unprocessed. I presume the inorganic carbon was removed at NOSAMS? It would be good to include that information.

Page 14421, line 17-18: "and references therein" should be included in the brackets of the reference.

Section 4: Discussion Can you think of a bit more descriptive titles of the different parts? It would be nice if the titles describe some of the main points instead of just using the measured parameter as the title?

Page 14423, line 9: I suggest to write "(0-400m elevation)" or so to avoid confusion with depth of deposits.

Page 14425, line 16: Both "permafrost" and "affected" say something about "watersheds" so you should use a hyphen in between them: "permafrost-affected watersheds". Same for "Pleistocene-aged OM" in this sentence.

Page 14425, line 18: I suggest to write ".. might only have a minor effect".

Page 14426, line 29: You give an algal-derived OM 14C signature of 49 per mille. Where is this based upon? You also write "and" in between the brackets, that should not be there I suppose. And, next page, "although this might not be true?" Can you clarify this part?

Page 14427, line 3: ";" should be removed.

Page 14428: Equation 1: "POC:PNPOM" should be "POC:PNNEW" I think? Line 6: I

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presume you here mean "corrected value" instead of "measured value"? Also, I think it should be Table S2 instead of Table S1. Line 24: Doesn't it make more sense to write "or" instead of "and" here? Line 25: I suggest to remove "so" Where is Table 3 in the Supplementary Information?

Page 14429: Line 5-6: Remove "theses". Line 8: Do you mean "POC:PNNEW" or is "POC:PNcorr" something else? Line 19: Insert "of" in between "estimation" and "soil".

Page 14430, lines 16-19: How certain are you about these fractionation factors?

Page 14431 Line 9: Can you give uncertainties for the soil and plankton end-members? Lines 3-13: I am not convinced that the seasonal "aging" of POC is (only) due to active layer deepening (like it is for DOC). POC and DOC seem, age-wise, very decoupled and POC is also much strongly affected by erosion. Before making conclusions on why POC is sometimes older than other times, I think the data need to be correlated/associated with discharge. Related to this, it would be very interesting if you could plot for example a figure of 14C-POC age (y-axis) against julian day (x-axis) for all samples (for all years in the same graph), or, 14C-POC age (y-axis) against km from delta head (x-axis) to see patterns in potential aging (?) when travelling through the delta?

Page 14433, line 3: it is not entirely clear what you mean by "here".

Page 14434 Line 8: here the end-member has a 13C value of -26.6 per mille, but on page 14431, line 9 it is -26.9 per mille. This is a bit confusing, could you be a bit more clear/insightful on how you get to this number(s)? Line 6: so do you use published Alaskan soil values to calculate the Lena River soil OM end-member? Line 15: Selective degradation is a very important issue. This point, together with the lowering of POC:PN ratios by the contribution of inorganic nitrogen, should in my opinion already be discussed earlier in the manuscript: preferably when first introducing scenario 1. Line 18: "fairly well constrained"? I do not completely agree, because (i) the readers need more information on where the plankton end-member value comes from, and (ii)

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how you calculate the soil-derived  $^{13}\text{C}$  end-member value is also not so transparent (it is based on your data and data from literature, right? could you for example include all the values in a table?)

Page 14436, line 13: here and at more places in the manuscript: please write  $^{14}\text{C}$  (or  $^{13}\text{C}$ ) concentrations and not " $\text{D}^{14}\text{C}$  concentrations". Also, note that the delta symbol for  $^{14}\text{C}$  (not stable) is different than for  $^{13}\text{C}$  (stable); this is not correct in the abstract.

Table 1: - Could you write  $^{\circ}\text{N}$  and  $^{\circ}\text{E}$  with the coordinates? And also explain somewhere that "dec." means "decimal degrees". - Could you add "soils" and "SPM" or something like that to the first two lines and the rest, respectively? - is it an idea to just say that all SPM samples were collected at 0.5m depth instead of listing the same number many times? Same for bluff height (maybe just mention that somewhere else?)

Table 2: - In 2009 the differences between mean and median values are quite high. So are there relatively many extreme values this year? Maybe something to elaborate on?

Table 3: - "Lena Delta Aug 2009" is all TSM right? Could you add that?

Table 2-3-4: Could you make the headings of the different parts of the tables more consistent? (e.g. now Table 3 "Lena Delta TSM late May 2011" and Table 4 "late May 2011")

Figure 1: The scale of panel (b) is in my opinion much too small. Can you "zoom in" even more? (i.e. only north of  $71^{\circ}\text{N}$  and only west of  $134^{\circ}\text{E}$ ). And make the figure larger? It is very difficult to exactly see the delta channel patterns and also the sampling points.

Figure 2: - what does "a" mean as the superscript of TCM ? - I suggest to remove "surface water total suspended matter" above every row of graphs and instead include it in the caption. - here too, could you "zoom in" even more? The actual area of the figures where the colored data points are can be much larger. - it might be an idea to adjust the color scale when there is one data point that is much higher or lower than

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the rest. For example, for TSM in late May 2011: the extreme value of this sample makes interpreting the color scale on panel A, B and D rather difficult.

Figure 3: - what do the "a" and "b" mean as the superscripts of "ratios" (panel A, purple and green diamonds), and at the proposed end-member (superscript "c")? - I suggest to leave out the ice complex end-member proposed by Karlsson et al. 2011 as the Vonk et al. (2012) ice complex end-member is based on about 900 datapoints and much more solid than the one from Karlsson et al. - Why (this also relates to the manuscript text) do you choose your Lena River soil OM end-member to be close to the  $^{13}\text{C}$ -derived estimates (in the figure it even looks more enriched than these estimates?!) instead of the POC:PN-derived estimates? You claim that the  $^{13}\text{C}$ -derived estimates are more robust, I believe, but I am not sure I agree with this, as you essentially base your  $^{13}\text{C}$ -estimate on  $^{13}\text{C}$  values you choose yourself to be the end-member.

Supplementary information: Table S1: in caption you have ".." in the end. And, you here write "shown in Figure 2D", but there is no Figure 2D.

Table S2: you only report TSM for the June/July 2011 samples, but since you both have POC (mg/L) and POC in %weight, you can also back-calculate TSM for all the other samples. I do not understand why you do not do this.

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