To the editors and reviewers

We greatly appreciate the constructive and helpful comments and criticisms by two anonymous reviewers. All comments were carefully considered and almost all suggestions will be incorporated in the revised version of the manuscript. We also thank the associate editor Naohiko Ohkouchi for handling the manuscript.
Moreover, we would like to add Thomas Laepple from the Alfred Wegener Institute, Helmholtz Center for Polar and Marine Research in Potsdam, Germany, to the list of co-authors. He provides the dual-carbon-isotope simulation suggested by reviewer 2 and contributes to the discussion of the results.
An overview of the changes made can be found in the attached pdf file (below). In those cases where we chose not to follow specific recommendations for alteration, we present further arguments supporting our approach.

Sincerely,
Maria Winterfeld

Biogeosciences Discussion, 11, 14413-14451, 2014

"Characterization of particulate organic matter in the Lena River Delta and adjacent nearshore zone, NE Siberia – Part 2: Radiocarbon inventories"

by Winterfeld et al.

Overview of revisions to the manuscript, and response to reviewer comments

As already mentioned in the reply to the review of the first manuscript (Winterfeld et al.) submitted as companion paper, we would like to ask the editors to consider a change of the order of the two submitted manuscripts and thus a change of the titles. As mentioned in the reply letter for the first manuscript, the paper on lignin phenols benefits from including carbon isotopic data from the second paper in the discussion and we refer to paper #2 several times throughout the manuscript. While we do not refer to lignin phenol paper that much in the discussion of the carbon isotopic data. Therefore it seems only consequent to treat paper on carbon isotopes as the background data providing "Part I" and the paper on lignin phenols as the second paper, i.e. "Part II".

The largest changes in this manuscript will be due to adding the dual-carbon-isotope simulation as suggested by reviewer 2. Consequently, we have to add the respective sections in the method, results and discussion chapters.

Reviewer 1 general comments:

The authors consider two independent scenarios to estimate radiocarbon endmember of their interest. I could see wide range and slight difference in Δ14C estimates between POC:PN-based- and δ13C-based-scenarios. For more clarification, I wonder whether or not Keeling plot approach could be applied to the dataset. [POC] and radiocarbon data are available, and the authors have already assumed the other endmember (i.e., Δ14C of phytoplankton is 49‰). Therefore, y-intercept of the regression line obtained from a plot for Δ14C values (y) vs 1/[POC] (x), would indicate soil POM end-member. Further details on this approach may be found in e.g., "Mortazavi B, Chanton JP (2004) Use of Keeling plots to determine sources of dissolved organic carbon in nearshore and open ocean systems. Limnology and oceanography 49:102-108".

Reply: We did consider plotting 1/[POC] versus $\Delta^{14}C$. However, there is no linear relationship between 1/[POC] versus $\Delta^{14}C$ for our samples from 2009 and 2010. For 2011 the R^2 is ~0.6, however, based on only three samples. Please see Fig. R1 at the end of this reply letter for illustration.

Furthermore we decided to follow the suggestion from reviewer 2 to use a dual-carbon-isotope ($\delta^{13}C$ and $\Delta^{14}C$) three end-member model combined with a Monte Carlo Simulation to estimate the contributions from riverine phytoplankton, surface/modern soil, and Pleistocene soil/ice complex to the POM in our samples. This gives us the chance to distinguish not only between phytoplankton and terrestrial OM, but also between the two terrestrial permafrost sources, namely the surface soil of Holocene age and the ice complex deposits of late Pleistocene age, which contribute to Lena POM.

The authors think that phytoplankton represents photosynthetic autotrophs in the Lena River. However, the study sites seem relatively shallow (water depth is 0.5m, Table 1) and I wonder there are any benthic primary producers (e.g., periphytic algae attached on reverbed substrate, or periphyton) contributing (suspended) POM to water column. If that is the case, the assumption used by authors (i.e., δ13C value of phytoplankton = –33‰ is questionable: in general, periphyton is more 13C-enriched than phytoplank- ton. For a study of similar setting (carbonate-weathering dominates the source of DIC) but different system (headwater stream), "Ishikawa NF, Uchida M, Shibata Y, Tayasu I (2012) Natural C-14 provides new data for stream food-web studies: a comparison with C-13 in multiple stream habitats. Marine and Freshwater Research 63:210-217" may provide some implications.

Reply: We have to apologize for the confusion resulting from the "water depth" column header in Table 1. It should have been "water depth sampled" and not just "water depth". In agreement with a comment from reviewer 2 we deleted this particular column from Table 1 and mention the sampling depth for TSM/POM in the table caption as well as section 2.2.
We took our water samples of the Russian riverboat Puteyski 405. In the smaller and narrower channels particularly between the islands in the central delta we sampled from the middle of the channel. In the bigger delta channels, e.g. in the Bykovskaya channel, we did not sample directly in the middle of the channel as they can be >1km wide, but within the main river current far away from the riverbanks.
We appreciate the paper suggestion made by the reviewer. However, we do not think that there is any contribution from periphyton in our samples, because we sampled in water depth >0.5m.

The authors should carefully check terminology and δ- and Δ-notations throughout the text. For example, "Δ14C concentration" is not appropriate. Use "14C concentration" or "Δ14C value". Furthermore, "δ13C composition", "δ13C signature", "Δ14C composition" and "Δ14C signature" are often used in text, but some researchers do not accept these expressions. I recommend simply using "δ13C" or "Δ14C value".

Reply: It seems that there was something mixed up regarding the δ- and Δ-notations in the editing process of the submitted manuscript and it slipped our attention in the proofreading process. We changed it accordingly.

Further, we follow the recommendations made about the appropriate use of words like "composition" and "signature" and only use "^{14}C concentration", "$\Delta^{14}C$ value", and "$\delta^{13}C$ value" in the manuscript.

Reviewer 1 specific comments:

P. 14414, L. 12, 20, 22: *"δ13C", not "Δ13C"*

Changed.

P. 14415, L. 4, 7: *"Guo and MacDonald 2006", not "Guo et al. 2006". Check other references once again.*

Changed and remaining references checked.

P. 14418, L. 26: *Pore size of Whatman GF/F should be 0.7µm*

That's correct, we changed it to 0.7µm.

P. 14419, *2.3 Laboratory analyses: Provide analytical precision/uncertainty*

Reply: We provided analytical precision and uncertainty, respectively.

P. 14422, L. 20: *"the lowest Δ14C values", not "the most depleted Δ14C values"*

Changed.

P. 14425, L. 16-18: *Do you have any evidence of this statement? At least provide one reference otherwise delete the sentence.*

Reply: We deleted the sentence.

P. 14426, L. 25-26: *"indirect evaluations have to be considered estimates" is unclear. Do the author want to say that non-phytoplankton materials are potentially included in POM?*

Reply: We agree that this sentence is confusing and therefore rephrased it. Surface water POM in rivers is heterogenous and can be distinguished in phytoplankton and terrestrial OM. Because we do not have data on chlorophyll-*a* contents and/or microscopic counts of phytoplankton species of our samples, which could be used to quantify the phytoplankton fraction, we wanted to emphasize the fact that our phytoplankton fractions based on the C/N ratio and $\delta^{13}C$ values are rough estimates compared to the above methods.

P. 14426, L. 29: *"Δ14C ~ 49‰'' not "Δ14C ~ 49‰ and"*

Changed.

P. 14427, L. 1: *"although this might not be true" Why do you think so?*

Reply: We explained this in the sentences following this statement (P. 14426, L . 1-6). However, it seems we have not been clear enough and therefore rephased the paragraph and added more information to clarify the statement that the phytoplankton might be older than modern depending on the DIC ^{14}C of the Lena. Because there exists no DIC ^{14}C value for the Lena, we assume it equals the current atmospheric CO_2 ^{14}C value (~49‰) and we use this value as an end-member for our mass balance calculations. However, the DIC ^{14}C could be depleted compared to the modern atmospheric CO_2 ^{14}C value due to contribution of fossil carbon ($\Delta^{14}C$ = –1000‰ by definition) from carbonate weathering to the total DIC pool. Further, Tank et al. (2012) found that the DIC yields are negatively correlated with continuous permafrost extent in watersheds of six large Arctic rivers including the Lena. This would imply that the contribution from carbonate weathering in the Lena catchment might not be so large afterall as 77% of the catchment are characterized by continuous permafrost (Tank et al., 2012).
Again, without any measured DIC ^{14}C values we cannot be sure. Under the assumption of a modern phytoplankton ^{14}C concentration the estimated ages of the soil-derived fraction have to be considered maximum.

P. 14427, L. 3: *"soils, both of which provide", not "soils, both, providing"*

Changed.

P. 14427, L. 6: *"in other words, maximum", not "i.e. maximum*

Changed.

P. 14428, L. 25: *"The calculated" not "The so calculated"*

Changed.

P.14429, L. 14: *"<11600 yrs BP" not "□11600 yrs BP the oldest"*

Changed.

P. 14430, L. 2: *"Hubberten, 1999). This is also reflected" not "Hubberten, 1999) also reflected"*

Changed.

P. 14430, L. 5: *"data suggest" not "data suggests"*

Changed.

P. 14430, L. 13-15: *Don't you think that atmospheric CO2 is also important source for DIC? Your assumption was that modern C of phytoplankton came from atmosphere.*

Reply: Yes, indeed we think that atmospheric CO_2 is an important source for DIC and we forgot to mention this here. We added "atmospheric CO_2" as possible source.

P. 14431, L. 14: *"samples were" not "samples are"*

Changed.

P. 14431, L. 15: *"values were" not "values are"*

Changed.

P. 14432, L. 17-18: *"considerably 14C-depleted" not "considerably depleted"*

Changed.

P. 14451, Fig. 3: *Additional plot for Δ14C vs sampling date may help understand seasonal variation.*

Reply: We agree, and will provide an additional plot. However, our samples were taken within one or two weeks during each sampling campaign as well as in same season for 2009 and 2010. There might not be a clear seasonal trend visible from our data.
We also included Lena discharge data for the three sampling years from the newly published Arctic Great Rivers Observatory (A-GRO data set 2, www.arcticgreatrivers.org, accessed: 15 January 2015), because the Lena discharge also has a very distinct seasonal discharge pattern.

Reviewer 2 general comments:

Interpretation of river OM geochemistry can in my opinion not be done 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 contri- butions. 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.

Reply: We agree that hydrology is important when interpreting riverine POM data. We had difficulties to get dischrage data for the Lena Delta and its channels for our sampling period (2009-2011). In the beginning of this year a new data set from the Arctic Great Rivers Observatory (A-GRO, www.arcticgreatrivers.org, accessed 15 January 2015) provides daily discharge values from the Kyusyur gauging station located about 200km upstream the delta entrance at Tit Ari Island. As already requested by reviewer 1 we will add a plot of POM ^{14}C concentration in relation to sampling date and discharge. However, when entering the delta the water gets distributed through the individual bigger and smaller delta channels and through the numeruos delta islands, which slows down the water flow and most likely results in different discharge values for different channels. The overall discharge pattern determined at Kyusyur, i.e. relatively higher and lower discharge values are reflected in the delta channels, but we cannot say anything about the actual discharge values during our sampling periods in the delta.
We appreciate the suggestion of running a dual-carbon-isotope simulation to estimate contribution from the three sources riverine phytoplankton, surface soil, and ice complex. Frankly, we did not consider this when writing the manuscript. We are currently working on this simulation with the help of Thomas Laepple from the Alfred Wegener Institute in Potsdam, Germany, and we will therefore add one section to the method chapter explaining the simulation and extent the results and discussion chapters explaining and interpreting the data.

Reviewer 2 specific comments:

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

Reply: We appreciate the information, because these two values slipped our attention as they are only mentioned in the text of the respective paper as unpublished data. Therefore we have no information on when and where these samples where taken in the Lena River and it is difficult to compare them with our data. However, we will consider these values as well as the newly available POC ^{14}C concentrations provided by the A-GRO (see also comments on Reviewer 1 above) from the Zhigansk gauging station about 900 km upstream the Lena Delta for our interpretation.

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?*

Reply: Indeed, the description on the soil profiles is lacking. We added information on the soil profiles in the text in section 2.2 (Sampling) as well as in the supplement with photos of the bluffs. All soil samples in the Lena Delta were of Holocene age.
Regarding the term “first delta”, we missed to explain the Lena Delta development and add this in the study site description (section 2.1). Briefly, according to Griegoriev (1993) and Schwamborn et al. (2002) the Lena Delta consists of three terraces. The first terrace including the active floodplains coves the eastern part of the delta and is referred to as the “active delta”. The second terrace is situated in the north western part of the delta consisting of sandy islands and the third terrace represents the late Pleistocene ice complex deposits predominantly found in the southern part of the delta.

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 material? Please elaborate a bit in the text.*

Reply: Yes, that is exactly what we did. We added some sentences to the sampling description in section 2.2.

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

Reply: The term “peat” was used here as a generalization to describe the appearance of the whole soil sequence and it is not used correctly. As stated in the comment above we will be more detailed on the bluff sampling and refrain from using peat where it is not appropriate. We also samples layers with higher sand contributions and plant detritus, which are not peat.

P. 14418, line 26: *Glass fibre filters (GF/F) have a nominal pore size of 0.7 um, and not 0.45um.*

Reply: We changed it to 0.7µm.

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.*

Reply: Yes, the inorganic carbon was removed as we specifically determined the ^{14}C concentration of organic matter. We added this information to the text.

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

Reply: Changed.

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?*

Reply: We will think more descriptive titles.

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

Reply: Changed.

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

Reply: Changed as suggested.

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

Reply: In agreement with the comment from reviewer 1 we deleted the whole sentence.

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?*

Reply: Also in agreement with a comment from reviewer 1 we rephrased this paragraph and added more information on the ^{14}C value of 49‰ for phytoplankton and why we

think that this might not be true. Please see our reply to reviewer 1 above (page 5 of this pdf file).

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

Reply: Changed.

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

Reply: Yes, it should be $POC:PN_{NEW}$. We changed it and also followed the other suggestions made here. And it should be Table 4 in the manuscript.

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".*

Reply: Changed as suggested. Also, our terminology is inconsistent regarding newly calculated POC:PN ratios. It should be $POC:PN_{NEW}$ throughout the manuscript and we changed this accordingly.

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

Reply: We took this fractionation factor from the literature and forgot citation in the submitted mannuscript. The reference of Mook & Tan (1992) will be added. Furthermore, we re-phrased parts of this paragraph to elaborate more on the $\delta^{13}C$ and ^{14}C values of the phytoplankton end-member, particularly in regard to the dual-carbon-isotope three end-member simulation that we are adding to the manuscript. In summary, Mook & Tan (1992) give a range from -20 to -25‰ as fraction factors between bicarbonate and primary production. The lower value under warm temperatures and higher fractionation factor under cold temperatures. We took the value of -25‰ for cold temperatures for our study area. However, because the Lena water is coming from the south, where it is much warmer in the summer season resulting in water temperatures of >10°C (July to Sept.; A-GRO data set 2), it seems more appropriate to use a mean fractionation factor of -22.5±2.5‰ to account for some uncertainty. Further, Galimov et al. (2006) published values for bicarbonate and plankton $\delta^{13}C$ in the Ob' and Yenisey estuaries. Both rivers have a comparable watershed size to the Lena and also drain several climate zones. The southernmost samples taken in both estuaries, which were 100% riverine according to the phytoplankton species distribution had bicarbonate $\delta^{13}C$ values of for example -9.1 to -14.8 ‰ for the Yenisey with corresponding plankton $\delta^{13}C$ of -31.9 to -36.2‰. Similar

values were observed for the Ob' River by Galimov et al. (2006). Our assumption of the pyhtoplankton $\delta^{13}C$ end-member seems to within the possible range of large Arctic rivers.

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?*

Reply: See also comment above. We reconsidered to take a $\delta^{13}C$ fractionation factor for phytoplankton of -22.5±2.5‰, which results in an $\delta^{13}C$ end-member for phytoplankton of – 30.5±2.5‰ (-8‰ for DIC $\delta^{13}C$). Accordingly, we will change the calculation.
Furthermore, as also suggested by reviewer 1 we will add a plot Julian day versus POC ^{14}C and discharge data for the sampling period from the new A-GRO dataset 2 (see also our comment to reviewer 1 on page 6 of this pdf file).

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

Reply: It should refer the southern boreal hinterland of the Lena. We clarified this to avoid an further confusion.

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) how you calculate the soil-derived 13C 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?)*

Reply: First of all, the $\delta^{13}C$ value should be -26.6. The other value is a typing mistake. We agree that we not explained the very clear where this value comes from. We followed the suggestion and added a table to the supplement with all the values used for end-member calculation including the used literature values. And yes, we used Alaskan soil values as well as they were from similar environment as the tundra in the Lena

catchment. But even if we leave these values out and only use Siberian $\delta^{13}C$ values we get a value of -26.6‰.

Line 15: We adopted the suggestion of discussing the selective degradation and associated lowering of POC:PN ratios earlier.

Line 18: The reviewer has a point. As mentioned above (also as reply to reviewer 1) we will provide a table with data used for end-member calculation and elaborate on that subject in more detail in the text.

Page 14436, line 13: *here and at more places in the manuscript: please write 14C (or 13C) concentrations and not "D14C concentrations". Also, note that the delta symbol for 14C (not stable) is different than for 13C (stable); this is not correct in the abstract.*

Reply: Please see our comment to reviewer 1 on page 3 of this pdf file concerning this subject.

Table 1: - *Could you write ◦N and ◦E with the coordinates? And also explain some- where 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?)*

Reply: We changed everything as suggested.

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?*

Reply: We will add some plots with Julian Day versus POC content, POC:PN, etc. including the discharge data from A-GRO to the supplement. However, the delta is very dynamic and POC samples represent only spatially very limited snapshots of the surface water. It is difficult to relate the difference between the mean and the median values to any particular process/event. We do not recall any distinct event, such as heavy rain event during the sampling periods.

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

Reply: "TSM" added.

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").*

Reply: We made the headers consistent.

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 degrN and only west of 134 degrE). And make the figure*

larger? It is very difficult to exactly see the delta channel patterns and also the sampling points.

Reply: Yes we can zoom a little bit more in panel b. Although this might still not be enough to see “excatly” where we took the samples within the channels.

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 he 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.*

Reply: The “a” was meant as a citation for Winterfeld et al. (2014), submitted as companion paper, but it is redundant with the figure caption where this is explained as well. We deleted “a”.

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 13C- derived estimates (in the figure it even looks more enriched than these estimates?!) instead of the POC:PN-derived estimates? You claim that the 13C-derived estimates are more robust, I believe, but I am not sure I agree with this, as you essentially base your 13C-estimate on 13C values you choose yourself to be the end-member.*

Reply: 1) The “a”, “b”, and “c” are redundant and were deleted from the figure.
2) We show the Karlsson et al. (2011) ice complex end-member in the figure, because we are also showing the surface soil/riverine and marine end-member to illustrate the different end-members used in the past. We agree that the Vonk et al. (2012) ice complex end-member is based on a very solid data set (n=300) and we will use this end-member for our dual-carbon-isotope simulation.
3) The comment regarding the $\delta^{13}C$ of the soil end-member relates also to a comment from above. Generally, we will provide a table in the supplement giving the values that comprise our chosen end-members including the literature values. Further, the $\delta^{13}C$ end-member used to estimate the contribution of phytoplankton- and soil-derived OM to our POM samples should be the same as the proposed Lena River soil end-member, but there was a mistake in the Excel-sheet Fig. 3 is based on. We corrected this in Fig. 3.

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.*

Reply: We corrected the "..". Figure 2D refers to Figure 2, panel D of the manuscript.

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.*

Reply: We apologize for creating such confusion here. We did not make it clear enough how our two companion manuscripts submitted to Biogeosciences Discussion are interrelated. Of course, we have the TSM data for our samples. They are shown for example in Fig. 2, panels A-D, but they are taken from the first manuscript (Winterfeld et al.: Characterization of particulate organic matter … Part 1: Lignin-derived phenol composition). We changed the introductory text to the supplement and the table caption of Table S2 to clarify this. We also added the TSM values from the first manuscript to Tabel S2 to give a comprehensive overview of our data.

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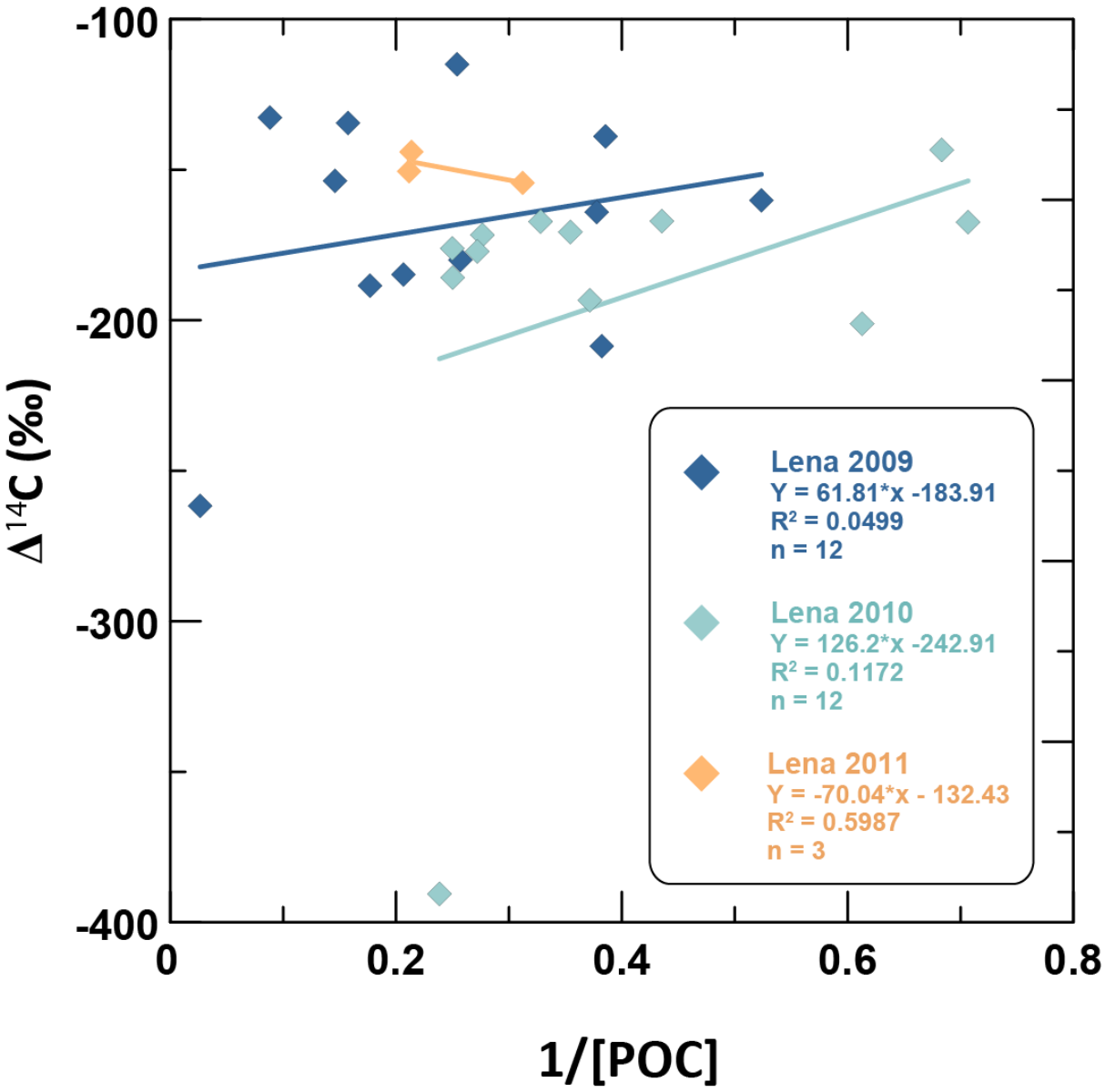

Fig. R1. Relationship between the POM bulk $\Delta^{14}C$ and 1/POC content for the samples from 2009, 2010, and 2011. There is no correlation for the years 2009 and 2010 and the R^2 of 0.6 for 2011 is based on only three samples.