

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 three end-member mixing model 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

“Characterization of particulate organic matter in the Lena River Delta and adjacent nearshore zone, NE Siberia – Part 1: 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”.

Overall, the manuscript was restructured according to reviewer comments and also due to the additional dual-carbon-isotope three end-member mixing model as suggested by reviewer 2. In order to make the calculations of the soil fractions in the binary mixing models and new three end-member mixing model consistent and comparable (using Monte Carlo simulation) we chose end-member values and associated uncertainties, which are slightly different from the previous manuscript version. Yet, as shown in Fig. R2 at the end of this reply letter, the old and new (revised) calculated soil fractions for both binary mixing scenarios (based on POC:PN ratios and $\delta^{13}\text{C}$, respectively) are within the uncertainties of each model. An additional reason for differences in the soil fraction calculation of the POC:PN-based scenario is a mistake in the calculation of the soil end-member. It should have been 23.7 ± 11 (Table 4 in the manuscript), but was 20.2 due to missing rows in the respective excel sheet. Consequently, the calculated soil fractions and related soil $\Delta^{14}\text{C}$ estimates changed in Tabel 5 (previously Table 4). As noted above, the changes were within the model uncertainties (Fig. R2) and therefore did not change the outcome/interpretation of the data.

Overview of major changes:

- we added section 2.4 “End-member modeling” to the methods chapter
- previous sections 4.1.1 and 4.1.2 were combined and summarized them under 4.1 “Origin of organic matter in the Lena Delta
- we added section 4.3.3 “Comparison pf scenario 1 and 2
- we added section 4.3.4 Holocene soil versus Pleistocene Ice Complex deposits

- we added a table (now Table 4) with the end-member values used for each mixing model (binary and three end-member)
- we extended the supplementary information including several additional figures and three additional tables with individual values and their references used to calculate the soil, Holocene soil and Ice Complex end-members (POC:PN, $\delta^{13}\text{C}$, $\Delta^{14}\text{C}$)
- we added Fig. 4 showing the results of the dual carbon isotope three end-member mixing model
- Fig. S2 (POC vs. PN) was slightly changed, because the plot for 2010 did not include all samples for 2010 in the previous version

Additionally, the paragraph discussing the sorption of inorganic nitrogen to clay minerals and thereby lowering the POC:PN ratios was shifted from **page 14424 lines 8-12 to page 14423 line 23**. As noted below in comment 39 (reviewer 2, page 13 of this pdf file) the sentence paragraph “Using...” to “... in 2011” (page 14423 line 23 to page 14424 line 2) was deleted. Because we combined the sections 4.1.1 and 4.1.2, there is a rephrased paragraph on the $\delta^{13}\text{C}$ results following after the shifted paragraph.

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 $\Delta^{14}\text{C}$ estimates between POC:PN-based- and $\delta^{13}\text{C}$ -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., $\Delta^{14}\text{C}$ of phytoplankton is 49‰). Therefore, y-intercept of the regression line obtained from a plot for $\Delta^{14}\text{C}$ 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 $\Delta^{14}\text{C}$. However, there is no linear relationship between 1/[POC] versus $\Delta^{14}\text{C}$ 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 ($\delta^{13}\text{C}$ and $\Delta^{14}\text{C}$) 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., $\delta^{13}\text{C}$ value of phytoplankton = -33‰) is questionable: in general, periphyton is more ^{13}C -enriched than phytoplankton. 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 in section 2.2. Further, we **included the samples from 2011**, which were previously in Table S1 in the supplement into Table 1 of the manuscript.

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, " $\Delta^{14}\text{C}$ concentration" is not appropriate. Use " ^{14}C concentration" or " $\Delta^{14}\text{C}$ value". Furthermore, " $\delta^{13}\text{C}$ composition", " $\delta^{13}\text{C}$ signature", " $\Delta^{14}\text{C}$ composition" and " $\Delta^{14}\text{C}$ signature" are often used in text, but some researchers do not accept these expressions. I recommend simply using " $\delta^{13}\text{C}$ " or " $\Delta^{14}\text{C}$ 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", " $\Delta^{14}\text{C}$ value", and " $\delta^{13}\text{C}$ value" in the manuscript.

Reviewer 1 specific comments:

1. P. 14414, L. 12, 20, 22: “ $\delta^{13}C$ ”, not “ $\Delta^{13}C$ ”

Changed.

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

Changed and remaining references checked.

3. P. 14418, L. 26: Pore size of Whatman GF/F should be $0.7\mu m$

That’s correct, we changed it to $0.7\mu m$.

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

Reply: We provided analytical precision and uncertainty, respectively.

5. P. 14422, L. 20: “the lowest $\Delta^{14}C$ values”, not “the most depleted $\Delta^{14}C$ values”

Changed.

6. 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.

7. 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 and restructured it. We deleted the whole paragraph from **page 14426 line 23 to page 14427 line 6**, because the considerations and the discussion about estimating the phytoplankton OM contribution and the phytoplankton $\Delta^{14}C$ end-member occurs again in section 4.4 “Implications of estimated soil derived POM $\Delta^{14}C$ ” on **page 14435 line 3**. Here, we added more information on possible DIC sources in the Lena watershed and factors possibly influencing the DIC $\Delta^{14}C$ of Lena water from **page 14435 line 11** onwards.

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 $\delta^{13}\text{C}$ values are rough estimates compared to the above methods.

8. P. 14426, L. 29: " $\Delta^{14}\text{C} \sim 49\text{‰}$ " not " $\Delta^{14}\text{C} \sim 49\text{‰}$ and"

Changed. The new $\Delta^{14}\text{C}$ end-member value for modern phytoplankton for the binary mixing models and the three end-member mixing model is $41.9 \pm 4.2\text{‰}$. This value is the average atmospheric CO_2 $\Delta^{14}\text{C}$ value for May-August 2009-2011 determined at the Schauinsland observatory, Germany (Levin et al., 2013).

9. 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. **This sentence** is part of the deleted and shifted paragraph from reviewer comment 7 above. The detailed explaining paragraphs on this issue can be found on from **page 14435 line 11** following.

Because there exists no DIC ^{14}C value for the Lena, we assume it equals the current atmospheric CO_2 ^{14}C value ($\sim 41.9 \pm 4.2\text{‰}$) 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 ($\Delta^{14}\text{C} = -1000\text{‰}$ 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 after all 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.

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

Changed. As noted in the replies to **comments 7 and 9** above, this sentence was deleted from this part of the manuscript and is now discussed in section 4.4 of the manuscript.

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

Changed. As noted in the replies to **comments 7 and 9** above, this sentence was deleted from this part of the manuscript and is now discussed in section 4.4 of the manuscript.

12. P. 14428, L. 25: *“The calculated” not “The so calculated”*

Changed.

13. P.14429, L. 14: *“<11600 yrs BP” not “ \approx 11600 yrs BP the oldest”*

Changed. Note, the new ^{14}C ages based on the modified soil and phytoplakton end-member are 17,250 years BP and fossil (>50,000 years BP).

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

Changed.

15. P. 14430, L. 5: *“data suggest” not “data suggests”*

Changed. We rephrased this sentence.

16. 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 CO₂ is an important source for DIC and we forgot to mention this here. We added “atmospheric CO₂” as possible source.

17. P. 14431, L. 14: *“samples were” not “samples are”*

Changed. We deleted this sentence, because with the Monte Carlo simulation these samples could be considered in the model.

18. P. 14431, L. 15: *“values were” not “values are”*

Changed. As noted in comment 17, we deleted this sentence, because with the Monte Carlo simulation these samples could be considered in the model.

19. P. 14432, L. 17-18: *“considerably 14C-depleted” not “considerably depleted”*

Changed.

20. P. 14451, Fig. 3: *Additional plot for $\Delta^{14}C$ 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.

We added **Fig. S1** to the supplement.

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 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.

Reply: We agree that hydrology is important when interpreting riverine POM data. We had difficulties to get discharge 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 numerous 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.

As noted in comment 20 above, we added Fig. S1 to the supplement depicting the Lena River discharge determined at Kyusyur gauging station ca. 100km south of the delta and the POC content as well as $\delta^{13}\text{C}$ and $\Delta^{14}\text{C}$ values of our samples from 2009-2011.

We further added the method **section 2.4** on end-member modeling, the **section 4.3.4** dealing with the results of the dual-carbon isotope three end-member modeling, **Fig. 4** depicting individual fractions of phytoplankton OM, Holocene soil OM, and Pleistocene Ice Complex OM to each POM sample, and we added **Table S5** in the supplement displaying the fraction of the three end-member for each sample.

Reviewer 2 specific comments:

21. P. 14415, line 24: *There are in fact also a few ^{14}C -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 were taken in the Lena River and it is difficult to compare them with our data.

We **added the reference** of Vonk et al. (2010) to the sentence.

22. 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 covers 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.

We **added some sentences** describing the Lena Delta geomorphology and the first delta terrace in more detail on **page 14418 line 4** following.

23. 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, **page 14418 line 19** following.

24. 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.

25. 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.

26. 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 ¹⁴C concentration of organic matter. We added this information to the text.

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

Changed.

28. 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.

- we combined the previous sections 4.1.1 and 4.1.2 under section 4.1 “Origin of organic matter in the Lena Delta”
- section 4.2 was changed to “¹⁴C and age heterogeneity of POM (¹⁴C_{POM}) in the Lena Delta
- section 4.3 was changed to “Quantitative POM source determination and soil ¹⁴C concentrations
- section 4.3.1 was changed to “Binary mixing model scenario 1” and section 4.3.2 was changed to “Binary mixing model scenario 2”
- section 4.3.3 “Comparison of scenario 1 and 2” was added
- section 4.3.4 “Holocene soil versus Pleistocene Ice Complex deposits” was added
- section 4.4 was not changed

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

Changed.

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

Reply: In agreement with the comment from reviewer 1 we deleted the whole sentence (comment 6 above).

31. 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 (comment 6 above).

32. 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 ¹⁴C value of 41.9 ± 4.2‰ (average atmospheric CO₂ Δ¹⁴C May-August 2009-2011 from Schauinsland observatory, Germany; Levin et al. 2013) for phytoplankton and why we think that this might not be true. As noted in our

reply to comment 9 we deleted the paragraph from this section of the manuscript and explain and discuss the issue in **section 4.4 (page 14435 line 11 following)**.

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

Changed.

34. 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 S4 in the supplement.

35. 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.

36. 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 manuscript. The reference of Mook & Tan (1992) will be added. Furthermore, we re-phrased parts of this paragraph to elaborate more on the $\delta^{13}\text{C}$ 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 $>10^\circ\text{C}$ (July to Sept.; A-GRO data set 2), it seems more appropriate to use a mean fractionation factor of $-22.5 \pm 2.5\%$ to account for some uncertainty. Further, Galimov et al. (2006) published values for bicarbonate and plankton $\delta^{13}\text{C}$ 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 $\delta^{13}\text{C}$ values of for example -9.1 to -

14.8 ‰ for the Yenisey with corresponding plankton $\delta^{13}\text{C}$ of -31.9 to -36.2‰. Similar values were observed for the Ob' River by Galimov et al. (2006). Our assumption of the phytoplankton $\delta^{13}\text{C}$ end-member seems to be within the possible range of large Arctic rivers.

For the Monte Carlo Simulation of the binary mixing model (scenario 2) we chose a phytoplankton $\delta^{13}\text{C}$ end-member of $-30.5 \pm 2.5\text{‰}$ and added a short explanation and the reference of **Mook and Tan (1991)** on **page 14430 line 16** following.

37. 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 ^{14}C -POC age (y-axis) against Julian day (x-axis) for all samples (for all years in the same graph), or, ^{14}C -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 36** above. We reconsidered to take a $\delta^{13}\text{C}$ fractionation factor for phytoplankton of $-22.5 \pm 2.5\text{‰}$, which results in an $\delta^{13}\text{C}$ end-member for phytoplankton of $-30.5 \pm 2.5\text{‰}$ (-8‰ for DIC $\delta^{13}\text{C}$). Accordingly, we will change the calculation.

Furthermore, as also suggested by reviewer 1 we added 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).

38. 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 a further confusion.

39. Page 14434 Line 8: *here the end-member has a ^{13}C 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 ^{13}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?)*

Reply: First of all, the $\delta^{13}\text{C}$ value should be $-26.6 \pm 1\text{‰}$. 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 $\delta^{13}\text{C}$ 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 (**page 14423 line 23 following**). Note, **page 14423 line 23 to page 14424 line 2** were deleted ("Using..." to "...in 2011"), because the discussion on end-member mixing was completely shifted to section 4.3.

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.

We added the **Tables S1, S2, and S3** to the supplement showing the individual values used to calculate the soil OC:TN end-member, the soil $\delta^{13}\text{C}$ end-member, and the Holocene soil $\Delta^{14}\text{C}$ end-member. Further, we added this information when discussing the mixing models in section 4.3.

40. 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: An notet in our reply to the third general comment to reviewer 1 (page 4 of this pdf file) this happened during the typesetting process and slipped our attention in the proof reading process. .

41. 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?)*

Reply: *Changed everything as suggested.*

42. 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.

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

Reply: "TSM" added.

44. 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.

45. 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: The quality of the underlying image does not allow to zoom more in. Furthermore, the GPS coordinates in these high latitudes are not accurate enough to provide a realistic location within the individual channels (despite the decimal places in the coordinates. However, we **added a description** of our general sampling locations in smaller delta channels (sampled in the middle) and the main delta channels (not in the middle, but far off the shore, in water depth >10m) to **section 2.2 "Sampling"**.

46. 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 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.*

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".

47. 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 ^{13}C -estimate on ^{13}C 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 $\delta^{13}\text{C}$ of the soil end-member relates also to a comment from above. Generally, we provided three tables in the supplement (**Tables S1, S2, S3**) giving the values that comprise our chosen end-members including the literature values. Further, the $\delta^{13}\text{C}$ 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** and only show the end-members used by Vonk et al. (2012).

48. 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: Table S1 was included into Table 1 from the manuscript to avoid confusion. The new Table S1 shows the soil OC:TN end-member values.

49. 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., 2015: Characterization of particulate organic matter ... Part 2: Lignin-derived phenol composition). We **added the TSM** values from the companion manuscript to **Table S4** (previously Table S2) to give a comprehensive overview of our data.

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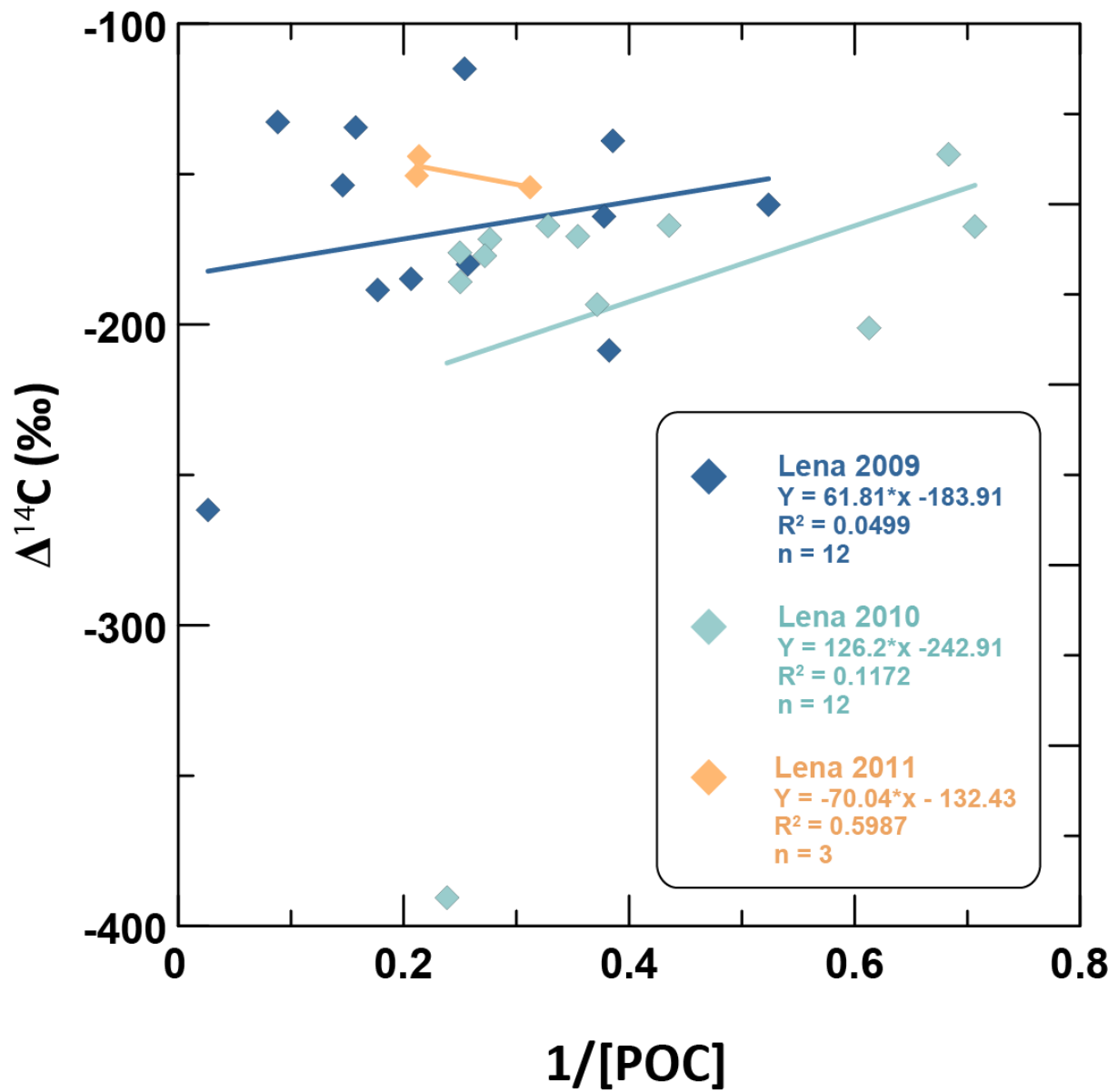


Fig. R1. Relationship between the POM bulk $\Delta^{14}\text{C}$ and $1/[\text{POC}]$ content for the samples from 2009, 2010, and 2011. There is no correlation for the years 2009 and 2010 and the R^2 of 0.6 for 2011 is based on only three samples.

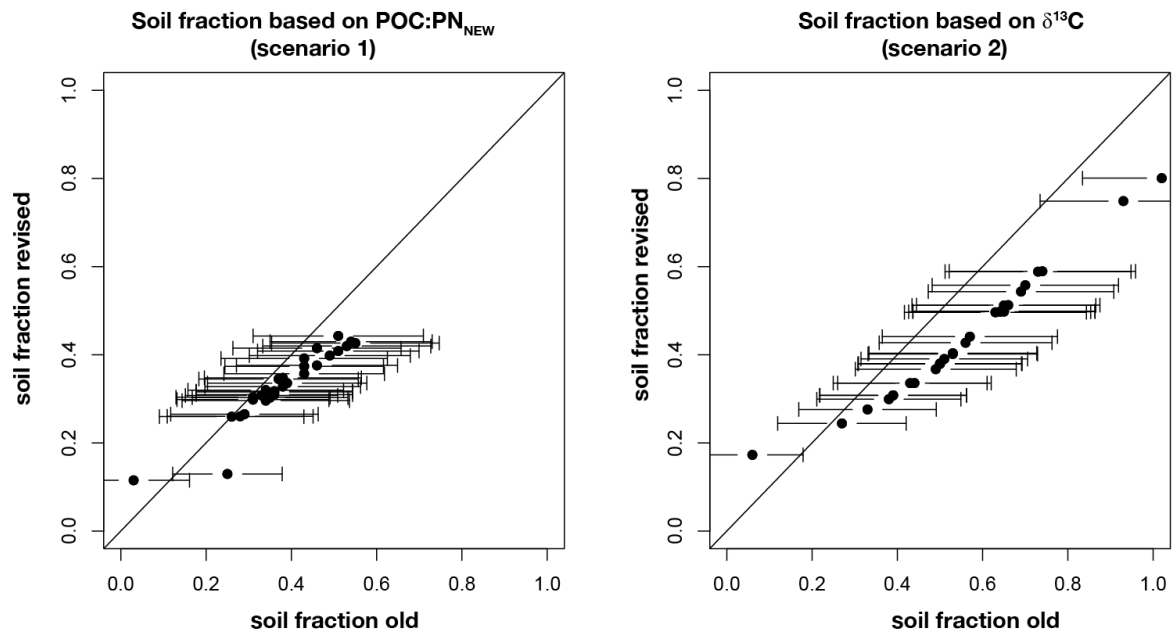


Fig. R2. The old calculated soil fractions versus the newly calculated soil fractions using modified end-member values and Monte Carlo Simulation for both binary mixing models.