

## ***Interactive comment on “On the biogeochemical signature of the Lena River from its headwaters to the Arctic Ocean” by I. P. Semiletov et al.***

### **Anonymous Referee #2**

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Overall quality of the discussion paper

The manuscript by Semiletov et al. aims at describing the biogeochemical signature of the Lena river and its impact on the adjacent coastal sea. At least this is my interpretation from the title and the abstract (where it is obvious that the focus is on the carbon system/fluxes). The introduction support this interpretation and it is even specified that the study is based on an extensive set of data collected in the Lena with its plume in the Laptev Sea. However, when getting to the result and discussion sections I get confused. Large parts of those addresses sources of carbon from outside the Lena river and are of review style. This is a pity as it takes the focus from the Lena river data and a more thorough discussion of these and their impact on the coastal sea. Hence I recommend that the authors make a major revision of this part of the manuscript and cut

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large parts of the text not focusing on the objectives of this contribution. Suggestions follow in the details below.

#### Detailed comments

One general aspect is that a set of geographical names are mentioned in the text. There is a need to include these in a map if a reader that is not very familiar with the region should be able to follow the arguments.

P 2094, L 5. Equilibration is when a process is balanced, but here it concern independent processes (one being primary production one being microbial mineralization) and thus it should be quasi-steady state instead of quasi-equilibrated.

P 2100. Discussion about pCO<sub>2</sub>. With a precision of 0.02 mmol/kg in TA and 0.01 in pH, the uncertainty in pCO<sub>2</sub> should be at least 50  $\mu$ atm. This is much greater than the 1% agreement within the techniques applied. Which different techniques? The text deals with fresh water but data from the Laptev Sea in also presented and discussed. Was another technique used for these data?

P 2102, starting on L 17. This contribution deals with the modern world and thus the Lena River basin is the present, i.e. not the paleo river basin in the Laptev Sea. Thus the text; "It is assumed that under the Laptev.....; Rachold et al., 2007)." addresses the subsea permafrost, a very exciting and interesting topic, that is not the topic of this contribution. It does not impact the fate of the Lena river input! The same goes for the text starting on L29: "Taking into ..... gas (Shakhova 5 et al., 2010a)." as well as the one starting on P 2103, L7; " Additionally ..... to the water column.". My suggestion is to delete these sentences.

Section 4.1. should deal with the river-derived PM and POC. The first paragraph discusses the Holocene situation as a motivation for this study. It is not needed for that reason, and if the authors think so it should be in the introduction. Alternatively if the data presented needs to be put into a wider perspective than it should be done so after

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the data is discussed.

P 2104, paragraph starting L 3. It should be stated that these data are from the surface water, even if that information is given in the method section.

P 2106, L 8. The del-13C value given for phytoplankton is for pelagic. However, ice algae has been shown to have a significant different value. How would that impact this discussion?

P 2106, starting on L 9. What is the relevance for computing the CTOM in the ESS? How is this coupled to the Lena river outflow? What is the importance of local coastal erosion? The given mean percentages does not say anything if it is not related to areas and residence times etc. Looking at Fig 3 it looks more like the 70% isoline divide the ESS in south - north more than east - west. The above comments are supported in the text of the next paragraph!

P 2106, L22. This is confusing. The arguments are difficult to follow and are not supported by any data. In the end it is stated that more discussion will follow in next sections. I suggest to delete this section and have deal with this issue in the next section, including new data as bases for the discussion.

P 2107, L6-7. What is the span of the given mean PM concentrations?

P 2107, sentence starting at L12. Can one distinguish between the importance of eroded and river transported OC for sustaining this high concentration of benthic biomass?

P2107, L20. Here the authors cite other work and state that they agree with their previous estimates. However, no references are included to where these previous estimates are published. I do not see what this contribution add to these earlier work.

P2107, L23. What is the  $\sim 4$  Tg/yr referring to? Total POC input or POC by Lena river?

P2107, L25. The title implies that processes are to be discussed. I do not see anything

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about processes but only on sources. Hence I recommend a change of title.

P2107, last line. Specify that it is the ESS hydrography that is impacted by the Lena River plume.

P2108, starting on line 4. These arguments are not convincing. Fig 4 illustrate the concentration of PM and percentage of terrestrial OM in the surface water. One cannot directly apply this distribution to the sedimentation or transport. More care has to be taken when assessing fluxes from concentrations.

P2108, paragraph starting on line 23. This conclusion is not convincing based on the data shown in this manuscript. However the data and arguments made by the cited work of Semiletov et al., 2005; Vetrov et al., 2008; Karlsson et al., 2011 makes a better base for this. Once again this contribution does not add anything substantial compared to earlier work.

P2109, paragraph starting on L 5. This paragraph also is of review character. No reference is made to any figure or new data, only to already published articles.

P2110, L 3. Information of the sampling should be moved to the Method section.

P2110, L7. Add "water column" after "whole".

P2110, L8. Change " throughout the major water column (except of deeps) were the same." to "were the same at all depths except close to the bottom."

P2110, L. 22. Note Yakutia on one of the maps.

P2110, L25. It is referred to TOC at stations 40 and 41, but it does not look like they are included in the graphs of Fig 5 or 6.

P2111, L1. Primary production should decrease TCO<sub>2</sub> not increase it. Increase in DIC could be caused by increase in salinity. Is there an oceanic contribution to these coastal stations?

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P2111, L8. The concentrations given for TC (and TCO<sup>2</sup> and TOC) does not make sense when looking at Fig 5. Something must be wrong.

P2111, L14. TOC at stn 40 is not in Fig 5, only in Table 1. Why?

P2111, L20. If the standard deviation of the measured concentrations are included the differences are not significant, except maybe for DIC in 1995.

Same line. What does (CO<sub>2</sub>) mean in this sentence?

P2111, L20. The sentence that starts on this line reads that "We suggest .." and then it refers to another publication. Even if some of the authors are the same it is essential to distinguish between the new findings of this contribution from that of earlier publications.

P2111, L28. After "while" TCO<sub>2</sub> is missing.

P2112, L4. For how many data points is the negative correlation found? If it is the three years that is presented in Table 2, than what about the variability in the mean and how this impact the correlation?

P2112, L8. The arguments of the sentence that start on this line does not make sense as the only explanation. Yes some TOC will be mineralized to CO<sub>2</sub>, but this could easily be out-gassed if not associated with increasing total alkalinity. This can only come along if metal carbonates are dissolved at the same time. Yes pCO<sub>2</sub> is high but so is TA, see Pipko et al 2010. Finally I do not see how the arguments of CO<sub>2</sub> being transferred to OC fit to the data as it then has to move up the river.

P2112, L15. Also this fairly constants pH support dissolution of metal carbonate in combination with decay of organic matter.

P2112, L23. TOC increases if anything, see Table 2.

P2112, L26. What is meant by fate? The given values of TCO<sub>2</sub> and TOC (L29) should be given with confidence interval. The TCO<sub>2</sub> concentration at S=0 is substantially lower

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than the mean value measured in 95 (Fig 5). One then ask if this is due to temporal variability, impact by primary production in the Laptev sea or any other cause. This shows that care has to be taken in making general conclusions from data collected in one year at one season.

P2113, L9. The variability in pCO<sub>2</sub> is significant in Fig 5. Thus give mean with std dev before drawing conclusions.

P2113, L19. Should it be 45 000 years (not per year)? Where is total accumulation of inorganic carbon in the Arctic Ocean (450 Gt C) coming from? If it is computed as the DIC inventory then this comparison is not relevant as the majority of this inventory is coming with the in-flowing seawater from the Atlantic and Pacific Oceans.

L2114, L18. Change carbonate to carbon in title.

P2115, L9. Show names on map.

P2115, paragraph starting at line 23. This is once again a review of the fist authors earlier findings. It should only be included if the information is relevant to the (new) data or interpretation presented here.

P2116, L10. P&N are still available in significant concentrations,  $\sim 3$  and 10-15  $\mu\text{M}$ , respectively. Thus primary production is not nutrient limited.

P2116, L10 and on. Compare with the data as is presented in Fig 10. Do not build the story, once again, on earlier publications!

P2116, paragraph starting on L20. The arguments of the Lena River influence on pCO<sub>2</sub> of the central Arctic Ocean is not convincing. -The high pCO<sub>2</sub> data reported on line 22 are very different from other observations during summer. One ask how representative they are. They are mentioned in Semiletov et al 2007, but without stating anything regarding the data quality. -Mixing of upper halocline water into the surface layer is highly unlikely. -That Arctic rivers contribute high DOM over the deep interior has been shown by several earlier investigations.

P2117. The text discuss several aspects of the carbon system, but it does not lead to any clear message or any new findings.

P2118. This section has to be rewritten as it now is a review of earlier work. Start with presenting the new data from the Lena River in 2008 and then set these data in perspective of the earlier measurements, and note what new it adds. If it does not add anything new I do not see much value in this section.

Figure 6. This figure is a repetition of Fig 5 and is not needed.

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**BGD**

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