

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

We are thanking both anonymous referees for their comments and are glad to read that both referees are positive about the manuscript. We hope that our comments and final revision will improve this paper significantly.

Below are our responses to Reviewer #1 and Reviewer#2.

Referee #1

General comments:

This effort addressing carbon sources and variability from high in the Lena watershed through to the Lena delta and beyond is commendable in its scope. Also, simultaneous consideration of inorganic and organic carbon is helpful for constraining the potential role of river inputs versus other sources contributing to the carbon budget of the East Siberian Arctic Shelf region. However, this paper could be significantly improved by addressing two major issues.

1st Comment: First, the limitations of using data that are primarily from mid to late summer and early winter need to be thoroughly acknowledged. How does a lack of information for the mid-May through mid-June timeframe, when a large portion of export from the Lena River watershed occurs and organic matter composition is markedly different, limit your ability to generalize about how the coupled river/ocean system operates? This should be addressed in the introductory material as well as in the Results and Discussion section.

Response: Thank you! This topic will be addressed in the revised paper.

Freshwater discharge from the Lena is highly variable throughout the year with maximum during the spring breakup (late May-June). It is supposed that a significant portion of the Lena sediment discharge happens at the same time (Stein and Macdonald, 2004). However, sediment transport dynamics during breakup are poorly known and the limited data that exist are controversial. For example, Pivovarov et al. (1999) showed an increase of suspended particulate matter (SPM) concentration from “clear” winter-May water (0.5- 3 mg/l) up to 20 mg/l during the flooding peak, with a maximum of up to 70 mg/l observed at one station located near the main channel (Bol’shaya Trofimovskaya) outlet. **A similar range of SPM concentrations was observed along the Lena River stream after breakup time (late June-August), as discussed in this paper.** Another “mooring-based” study (Wegner et al. 2005) shows that during and shortly after the river-ice breakup (June-early July) the main river SPM transport onto the mid-shelf occurs, with peak SPM concentrations of up to 6.5 mg/l. Surprisingly, events with the highest SPM concentrations (up to 9.1 mg/l) were recorded over the mid-shelf during the ice-free period and the freeze-up time. Thus, we can consider our current multi-year data to be a product of the soundest basic approach at present; this approach should be improved in future studies. MORE DETAILS WILL BE SHOWN IN THE REVISED PAPER.

2nd Comment: Second, given that export of DOC is much greater than POC from the major Eurasian arctic rivers, it should be given more explicit attention in the manuscript. Discussion of “TOC” and “terrOM” at various points in the manuscript must be referring to DOC since the particulate fraction is minor. However, it often seems that these terms are being used with the POC fraction in mind. Although a DOC dataset was collected, it seems almost to be treated as a footnote, with the majority of analysis and discussion of organic matter devoted to POC.

Response: Thanks! We will discuss both DOC and POC data sets and their dynamics in detail in the revised manuscript.

More **detailed comments** from Igor Semiletov and co-authors (**marked as IS**) are itemized below:

1) Revise the title to reflect the focus on carbon.

IS: Agreed. The new title is: **“Carbon transport by the Lena River from its headwaters to the Arctic Ocean, with emphasis on fluvial input of terrestrial particulate organic carbon, vs. carbon transport by coastal erosion”**

2) Page 2095, lines 6-10: The end of this sentence does not make sense.

IS: Agree, thank you: deleted

3) Page 2097, line 13: July and August is not typically a time of high water levels on the Lena River. While the water level during July/August is certainly higher than during the winter, it is typically much lower than during peak discharge in the spring. If 2003 was exceptional, please explain. Also, consider how atypical circumstances during the summer of 2003 may make it difficult to make generalizations with data from that summer.

IS: Unfortunately, at present we have very limited data sets representing reliable solid discharge and POC data throughout the year. For example, we cannot use the Soviet Union Hydromet Service multi-year data because they used non-standard filters (smaller pores) compared to the now-canonical Whatman GF/F borosilicate filters (see lines 17-20/page 2104). Then we calculated the approximate annual solid Lena River discharge based on our original data obtained along the entire Lena River in a “typical” year, which is 2003 (annual discharge in 2003 = 533 km^3 vs a long-term mean = 525 km^3). Note that “maximal” PM values obtained during the flooding time (Pivovarov et al., 1999) are comparable to PM values measured in summer 2003 (see response to the 1st General Comment above), while in winter-May the PM concentrations are low (<3mg/liter: Pivovarov et al., 1999). So, we use the mean PM concentration = 20mg/liter; we assume this value is close to the realistic annual value or slightly exceeds it. In addition, the annual Lena River discharge is highly variable, ranging between a maximum of 643 km^3 (1961 г.) and a minimum of 434 km^3 (1940 г.). Therefore, the actual annual solid discharge can be significantly different from year to year. To learn more about discharge specifics and variability additional long-term representative PM/POC measurements are required. Thus, our paper reflects the existing level of knowledge and presents newly-available data to Western scientists.

4) Page 2098, lines 16 and 17: Here it says late June to early August 2003, whereas it says late July to early August on page 2097, line 13. Please clarify.

IS: We apologize for this typographical error. Actually our studies in 2003 were performed from late June to early August.

5) Page 2105, lines 7-9. Given that PM concentrations are positively correlated with river discharge, using mean PM concentration and annual river discharge to calculate PM export underestimates the export value (potentially by a large amount!). A more rigorous calculation of flux is needed. The same is true for POC (line 10).

IS: We agree that this calculation is a very simple approximation, but it is the best approximation that we can make at this stage of our knowledge about the Lena River solid discharge and its dynamics. But we cannot agree that this is an underestimate of the export value because we used the mean PM concentration = 20 mg/liter which is a value similar to values obtained by Pivovarov et al.’s (1999) measurements made in the flooding time. Please note that maximal

riverbank erosion happens during the warmest months of August and September; this erosion could bring more PM/POC per Lena River's water unit than in flooding time.

6) Page 2106, line 17-21. Awkward sentence.

IS: Thank you, it will be rewritten in the context of comparing the fluvial terrestrial OM contribution vs. coastal erosion.

7) Page 2109, lines 5-9. This discussion of terrestrial organic matter age does not address the fact that bulk DOC in the Lena (and other major arctic rivers) is young, while bulk POM is ancient. Also, the modeling exercise mentioned later in the paragraph needs to be described more thoroughly.

IS: Thank you for pointing this out. This paragraph will be rewritten more thoroughly.

8) Page 2111, line 28. Something is missing (CO₂?) after the word "while".

IS: Thank you! It should be: TCO₂.

9) The methods section describes measurement of inorganic nutrients (nitrate, nitrite, phosphate), yet nothing is said about these data in the results and discussion section. Either remove the inorganic nutrients from the methods or develop them in the results and discussion.

IS: Removed. Thank you.

Referee #2

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 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.

IS: We agree and have added a map of the region

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.

IS: We agree.

P 2100. Discussion about pCO₂. With a precision of 0.02 mmol/kg in TA and 0.01 in pH, the uncertainty in pCO₂ should be at least 50 μatm. 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?

IS: This is a technical mistake. The real precision of our TA measurements is 0.002 mmol/kg, which gives an uncertainty in pCO₂ of about 10ppm . Different techniques will be described in the revised ms.

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 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.

IS: Thank you. We will redo these sentences so that they conform to the content suggested by the new title of this paper as recommended by Reviewer #1. The new title is: "**Carbon transport by the Lena River from its headwaters to the Arctic Ocean, with emphasis on fluvial input of terrestrial particulate organic carbon, vs. carbon transport by coastal erosion**".

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 the data is discussed.

IS: Thank you. We will redo this part according your recommendations.

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.

IS: In most studies along the Lena River we sampled the surface water, while in the survey of 2003 we used Niskin bottles from the surface to the bottom (page 2098/L. 16-18)

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?

IS: The shelf water residence time is about 1-2 months. Hence, the melt water and ice algae do not play a role in September, which is the end of hydrological summer, and is also when most of our data was obtained. The O-18 technique applied by Anderson et al. (2009) also demonstrated the absence of melt water in the East Siberian seas during September.

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!

IS: By computing CTOM we show that the Lena River solid/POC discharge does not play a significant role in input into and accumulation of sediments in the ESAS (*as many researchers still believe*), while we show that the coastal erosion is a main contributor to the accumulated terrOC.

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.

IS: I agree that this section should be deleted if we use the old title, but in light of our new title we use this sentence (to be rewritten) for further discussion concerning the Lena River's terrPOC contribution vs. the contribution of coastal erosion.

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

IS: In the western part of the ESAS PM ranged between 4.7mg/l and 79.7mg/l in the surface water, and 5.2mg/l and 106.4 mg/l in the near-bottom water (Semiletov et al., 2005), vs. up to 20 mg/l during the flooding peak, with a maximum of up to 70 mg/l observed at one station located near the main channel (Bol'shaya Trofimovskaya) outlet (Pivovarov et al., 1999).

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?

IS: Thank you for this suggestion. I will rewrite this sentence showing that some "biological hot spots" would be associated with coastal erosion (where it is possible to support this contention by data)

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.

IS: Thank you. I will rewrite this paragraph to connect it more clearly to the main discussion: the role of the Lena's POC input vs. POC input due to coastal erosion.

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

IS: This number refers to total input from coastal erosion. I will rewrite this sentence to make it more clear. Thank you!

P2107, L25. The title implies that processes are to be discussed. I do not see anything about processes but only on sources. Hence I recommend a change of title.

IS: Our new title is :“Carbon transport by the Lena River from its headwaters to the Arctic Ocean, with emphasis on fluvial input of terrestrial particulate organic carbon, vs. carbon transport by coastal erosion”.

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

IS: Thank you. I will do that.

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.

IS: Figure 4 shows that the highest concentrations of PM/POC in the surface water are not related to the Lena River plume as was earlier supposed by many scientists. This Figure shows that the main source of terrOC across the ESAS is coastal erosion; this hypothesis is supported by the CTOM distribution shown in Figure 3. I will rewrite this paragraph to make it clearer. Thank you.

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.

IS: We use these references (where we are co-authors) to make our arguments in this paper stronger.

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.

IS: We use these arguments and references (where we are co-authors) to show that the exported terrOC is bio-available and makes a strong contribution to the CO₂ that is released from the study area.

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

IS: Agreed.

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

IS: Agreed.

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."

IS: Agree, thank you.

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

IS: Agreed: Map 1a now shows Yakutia.

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.

IS: TOC from stations 40 and 41 are included; I will redo fig. 5 to make this clearer

P2111, L1. Primary production should decrease TCO₂ not increase it. Increase in DIC could be

IS: Yes, we agree with the Reviewer that PP should decrease TCO₂ if no additional sources of CO₂ are available, but we have an additional source which is oxidized terrOC. I will rewrite this sentence to make it clearer.

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

IS: Yes, these should be referenced to Figure 6. Thank you for catching our error!

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

IS: We have added TOC in Fig.5. Thank you for pointing this out.

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 (CO₂) mean in this sentence?

IS: This should be written as TCO₂ (not "total inorganic carbon (CO₂)"). Thank you! We agree that differences between 1991 and 1998 are not significant, but differences between 1995 and 1998 are quite significant; these are the differences that are discussed in the text.

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.

IS: Agreed, and made the changes.

P2111, L28. After "while" TCO₂ is missing.

IS: Agreed, and made the changes

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?

IS: The negative correlation was found for the 40 sites represented in Table 1. This oversight will be fixed in the revised text. Thank you!

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 CO₂, 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 pCO₂ is high but so is TA, see Pipko et al 2010. Finally I do not see how the arguments of CO₂ being transferred to OC fit to the data as it then has to move up the river.

IS: We understand the Reviewer's comment. I will rewrite this paragraph to make it clear.

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

IS: This may be true, but it is outside of this manuscript's area of consideration. Thank you.

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

IS: Fixed. Thank you.

P2112, L26. What is meant by fate? The given values of TCO₂ and TOC (L29) should be given with confidence interval. The TCO₂ concentration at S=0 is substantially lower 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.

IS: Thank you. Based on your comments I will add a discussion about the possible reasons for the differences.

P2113, L9. The variability in pCO₂ is significant in Fig 5. Thus give mean with std dev before drawing conclusions.

IS: We agree and I will add this to the text.

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.

IS: I will remove that discussion.

L2114, L18. Change carbonate to carbon in title.

IS: Done.

P2115, L9. Show names on map.

IS: Done.

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.

IS: We agree, and I will rewrite this paragraph.

P2116, L10. P&N are still available in significant concentrations, ~ 3 and 10-15 μM , respectively. Thus primary production is not nutrient limited.

IS: Agreed, and changed the text.

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

IS: Agreed, and changed the text.

P2116, paragraph starting on L20. The arguments of the Lena River influence on pCO₂ of the central Arctic Ocean is not convincing. -The high pCO₂ 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.

IS: I will rewrite this paragraph, updating it with new data.

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

IS: I will rewrite this section to make it clearer.

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.

IS: This section will be rewritten, but I cannot agree that “it does not add anything new” because here the data obtained along the Lena River in summer 2008 are presented for the first time.

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

IS: We show in Fig.6 the quasi-constant distribution of TC (sum of TCO₂ and TOC) along the Lena River; this information is not presented in Fig. 5.