Final author response to all interactive comments:

We thank R. Macdonald and the second anonymous reviewer for large effort in providing valuable and very constructive comments, which have been useful in our major revisions of the ms. Naturally, we are encouraged that both reviewers consider our multi-year investigations in the Bhuor-Khaya Bay, SE Laptev Sea, to provide an important piece to the Laptev Sea puzzle. We agree with the key comments that the original ms was overly descriptive, lacked reference to key previous works and could be better placed in context of those. We have now revised the ms substantially to improve the presentation of our extensive field studies in a little investigated yet recognized important area for the Arctic land-ocean system. In the below, each review comment is listed first in italics, followed by our response and a description of resulting edit, in normal font.

General comments by R. Macdonald (Reviewer 1)

Other work relevant to this paper seems woefully neglected. This is true both in the sense of previous work on organic carbon and d13C measurements (see, for example, some of the references listed below), but also in the sense of prior publications from this general region investigating processes important to organic carbon including particle production and transport.

We agree that several valuable references were missing and those work are now discussed and added to the reference list. However, it deserves also to be mentioned that most of the studies suggested by the reviewer were conducted outside of the special system of Buor-Khaya Gulf, which is the main focus of the current paper.

The graphic presentation relies heavily on a series of areal displays which are hard to inter-compare, at least quantitatively Furthermore, where comparisons are made by reference to the figures, no estimates of confidence are possible (i.e., statistical comparisons based on t-tests or regressions). This manner of presentation then leads to frequent statements of difference not supported by any rigorous treatment. Given the amount of data available, this should not need to be the case. Much of the discussion could be better supported by a few well-chosen vertical sections to show distributions of properties and, where possible, property-property plots. No matter how the data are displayed, points at which data have been collected need to be included to give an idea of data distribution and density underlying the displays.

We agree with this comment. In the revised ms, statistical comparisons based on t-tests are instead included and the vertical distribution of suspended particulate matter and salinity are now included and discussed in the text. The study area map (Fig. 1) is revised to mark with different symbols, different types of oceanographic stations and for different years.

Finally, the application of d13C as a tool to distinguish different sources is done in a haphazard fashion that leads to seemingly contradictory statements throughout the text. The result is that although there appear to be many data available, no clear image of what they are saying emerges. The authors would do better to produce a coherent discussion of 13C (sources and processes), which would include the limitations of this measurement in the present discussion, and then ensure that later statements do not over-interpret this parameter, or interpret it differently in different sections. As the paper stands, it is predominantly descriptive and, although there may be several years of data collection presented here, the database remains thin

in the context of proposing interannual variability. That said, it is a valuable effort to try to make sense of widely different years, as the authors have done.

We used the value of d13C as an indicator of organic carbon (OC) sources because we had no other data such as molecular and isotopic measurement of OC (such as acyl lipid biomarkers and radiocarbon isotopes). But guided by the fact that the sedimentary material of the Laptev Sea coastal zone is stemming from two major sources: fluvial sediment discharge from the Lena River and thermal collapse and erosional input from the coastline ice-complex (Stein, 1998, 2000; Stein et al., 2004; Rachold et al., 1999; Semiletov et al., 2000, 2011; Mueller-Lupp et al., 2000; Dudarev et al., 2003, 2006abc) we used consideration of the specific sedimentological regimes, as an additional indicator for the separation of the two major sources of OC. In an earlier study we also used the distribution of d13C-OC in the surface bottom sediments to separate two different geochemical provinces existing in the East-Siberian Sea (Semiletov et al., 2005,*GRL*), an approach which was later confirmed with more detailed considerations using biomarkers, isotopes and C/N ratios (Vetrov et al., 2008; Vonk et al., 2010). Moreover, our conclusions regarding the sources of OC operating in 2008, were confirmed by the companion paper by Karlsson et al. (now published) in this special issue, which represents the molecular (acyl lipid biomarkers) and isotopic tools (stable carbon and radiocarbon isotopes) of the OC in the Buor-Khaya Bay for that single year. Thus, we conclude that this approach is useful and relevant also for other years. Furthermore, to illustrate our conclusions better we also included limited nitrogen (N-OC) data obtained in winter of 2007, and summer of 2005.

<u>Bottom line</u>: we certainly agree that the molecular tool is very useful, and that it should be used in all consequent complex studies.

Specific comments by R. Macdonald (Reviewer 1)

P1918, Line 27. The sentence implies that marine biogenic carbon can be found in winter, but not summer. Here and in much of the text that follows, the potential for marine organic carbon to contribute to 13C appears to be practically ignored from the discussion. What role does it play in the POC in this region? This sort of carbon is eventually discussed, but there needs to be a treatment of its properties and potential contribution much earlier in the text. See, for example, Vinogradov et al., 2000; Lalande et al., 2009; Sakshaug, 2004).

Based on publications by Are (1999), Bauch et al (2001), Dudarev et al., 2003, 2006abc, Grigoriev, 1993; Grigoriev and Kunitsky, 2000; Gustafsson et al., 2011; Naidu et al., 2000; Rachold et al., 2004; Semiletov, 1999ab; Vonk et al., 2011, and others listed in the References), it is known that in summertime the dominant source of organic carbon to this system is terrestrial carbon, while the marine biogenic carbon plays a small role in the shallow oligotrophic waters of the inner Laptev Sea (Anna-Stiina Heiskanen et al., 1996, Yu. I. Sorokin et al., 1996). This is also borne out in detail by our B-K Gulf companion paper by Karlsson et al., 2011. However, in wintertime the role of sea ice algae is noticeable, as mentioned in ms. Note that the publications referred to by Reviewer#1 consider the Laptev Sea areas located north off the Lena Delta, and on the outer Laptev Sea – a completely different system not nearly as heavily influenced by the terrestrial carbon export as the Buor Khaya Gulf

P1919, Lines 13-15. I would agree that this important area of the Arctic Ocean has a shortage of investigations. But that makes it all the more important to provide an idea of what has been done, and to set the current findings within the context of other data and interpretations. Commencing here, and following through the text, the authors practically ignore other organic

work on this shelf. I am not a Laptev Sea specialist, but I am well aware of related studies dealing with organic composition (%C, 13C) and other relevant correlates by, for example, Peulve et al., 1993; Boucsein and Stein, 2000; Boucsein et al. 2002; Fahl et al. 2001; Stein and Fahl, 2004; Mueller-Lupp et al., 2000; Viscosi-Shirley et al., 2003; Lobbes et al., 2000; Petrova et al. 2008 and so on. One result of this neglect is that the authors have isolated their findings from the context of what is already known or proposed about the functioning of the inner Laptev Shelf In my view, this then leads to a surficial, descriptive interpretation of the new data, relying almost entirely on areal mapping, with the result that the paper falls short of its potential.

We again agree with this criticism. The most important of the suggested references are now discussed and added in the text.

P1919 line 18-20. Where do these statistics come from? It would be helpful to provide some numbers here (e.g., typical input of the Lena for solids, freshwater and organic carbon set against input for the years data were collected).

The information comes from the references cited in the end of the sentence (Antonov, 1987; Sidorov, 1992).   To make it more clear, we added a new Table (under construction) with available historical data sets for each Lena delta channel.

P1919 line 26. With regard to the uncertainty of how much sediment reaches the Laptev Sea, the reader is left hanging. Does this uncertainty remain? Do the present data help resolve this issue? Is the budget put together in Stein and Macdonald (2004) right or does it need revision?

We included the basic reference on Stein and Macdonald, (2004) in the text. However, many uncertainties still remain especially concerning the fluvial vs coastal erosion contributions (see recent discussions also in Semiletov et al., 2011, this issue; Sanchez-Garcia et al., 2011; Vonk et al., 2010).   One of the results presented in this ms is showing that the main part of the terrestrial material transported by Sardhsko-Trofimovskaya channels is settled down in prodelta slope (also included in conclusions). For sure, additional data are required to re-evaluate the existing carbon budget for the Laptev Sea

P1920 Line 10. Again, how much sediment (t/yr) is involved in this retreat? How does it compare with the river input?

This data now has been added in the new Table (under construction).

*P1920 lines18-21. The statement of objectives should be clarified. The term
"shed light" does not shed any light for this reader.*

We have been replaced the "shed light" on "clarified".

P1921 lines 1-8. It is stated that 250 stations were occupied, presumably as marked in Figure 1. But were every one of these identical stations occupied during every mission including the one in late winter? This brief description neglects important details of sampling strategy (why, where, how many times, what was measured). The authors might consider presenting this partly in the form of a table. Also, some of the geographic locations used (e.g., Muostakh Island) are not marked anywhere, leaving most readers in the dark.

We agree that this was poorly communicated. The study area map (Fig. 1) is now revised to clearly identify, with appropriately different symbols, different types of oceanographic stations occupied in different years and seasons. The Muostakh Island hasnow also been marked.

Page 1921 lines 15-23. The precision/accuracy of the methods is very poorly presented. We are told only that accuracy and reproducibility are _ 0.1‰ for _13C, without any basis for how they were determined, and nothing for any other measurement.

The requested information concerning the analytical methods (precision/accuracy) has now been added.*Page 1922 lines 4-22. The discussion of water stratification and the subsequent distribution of suspended solids would benefit from the presentation of a representative section out from the coast. As it is, the paper relies almost entirely on comparisons of surface contour plots, which are difficult to read/compare and do not provide a sense of quantitative agreement between properties.* We agree. The vertical distribution sections of suspended particulate matter and salinity has now been included (new Figure, under construction).

Page 1922 line 14. Storm surge. How much, how measured?

Storm surges we identified using the Tiksi Hydromet data (daily hydrometeorological forecast) and own visual observations.

Since these were rather qualitative estimations, we have added text to explain that.

Page 1922 line 26. Where does the 'normal hydrometeorological condition of _10cm s-1' come from? Also if the authors mean to say that the normal current velocity is _10 cm s-1, then say that rather than presenting it in cumbersome jargon.

Correct, we meant 'non-stormy" conditions. Consequent edition has been made.

Page 1923 line 7. Again, a sectional plot illustrating the nepheloid layer would be helpful to the reader.

Agree: the vertical distribution of suspended particulate matter has been included (new Figure, under construction).

Page 1923, line 11-12. Figure 5 does not show the relationship between OC and sediment size distribution. It would be better to provide a plot directly showing this relationship (e.g., PSD vs 13C or POC) if that is what is desired, rather than forcing the reader to compare two areal contour plots.

We agree and have now included a plot of PSD vs POC (new Figure, under construction).

Page 1923, Lines 18-22. Discussion here and later would benefit from presentation of sectional views comparing the two regimes rather than text, which is difficult to visualize. Reference to pertinent work like that of Eicken et al., 2005, would likely also help.

Agree: the sections of vertical distribution of suspended particulate matter and salinity has been included (new Figure, under construction). The system description of Eicken et al., 2005 is now discussed to provide context and comparison with the current study.

P1924, Lines 3-6. This paragraph is confusing, and it is very unclear where the assertions come from. Why do fluvial sources hover at -29 ‰ while there is a plume of -23.6 ‰ opposite the Bykovskaya channel? This seems contradictory. What is this plume?

Yes, we are sorry about this misleading (now edited). Actually this is not a plume but rather a hot spot of heavy d13C, which can be explained by contribution by the sea ice algae (Stein and Fahl, 2004; Mueller-Lupp et al., 2000) This is now explained in the text.

Page 1924, Lines 17-25. Again, this would be much better presented with a representative D section(s). It is very difficult to visualize the differences between the sampling periods.

Agree: the sections of vertical distribution of suspended particulate matter and salinity have been included (new Figure, under construction).

P1925, Lines 5-6. What is the basis for this generalization? Citations?

We have clarified in the text that the basis for that generalization is our own multi-years data (2000-20010).

P1925, Lines10-14. The ragged presentation of _13C results in this section seems very incoherent to me. Why is -29 ‰ given as indicative of fluvial sources above, then but then -26.4 ‰ is found here near the river mouth, with -29 to 32 ‰ found in the offshore. There needs to be a more coherent approach to the _13C data, perhaps best done in one place in the discussion. A problem that must be faced with _13C here, and everywhere, is that there are multiple sources of organic carbon, and each source has variability in its composition. This limits the application and requires a very careful treatment. It might help the discussion if C/N ratio were also brought into the discussion, but perhaps N was not measured. It might also help if the _13C data were plotted against, for example, water depth, distance from shore, POC, psd, etc. This would do two things. It would give the reader a better notion of the range in data and their density, and also how they arrange themselves in space, or between years. If the highly depleted POC _13C is from primary production based on pre-depleted terrestrial DIC, then that would suggest that autochthonous production might be an important component of the POC distributions. It also would mean that marine (or algal) carbon could be anywhere from -16 ‰ (ice algae) to -32 ‰ making _13C of dubious value for source discrimination. And yet, this is hardly discussed at all. To this point in the manuscript it is as if the terrestrial POC dominates everywhere.

Yes, it is clear that d13C in isolation is not sufficient for quantitative source apportionment. The ms is now clear on this. Yet, the d13C is certainly providing useful information that at least qualitatively is pointing to certain source classes. We already edited this part of the paper notingthe high inhomogenity (or "mosaic") distribution of the OC-C13 signature near the river mouth and further offshore. We think that it might be a result of interplay between numerous river-borne sources of heavy (for example coal which is abundant for this area) and light OC (for example: freshwater phytoplankton) which is driven by highly changing hydrometeorological factors. So, lenses of water with different POC isotopic signature might be formed. The marine biogenic contribution in this particular area appears small as this area is heavily influenced by terrestrial organic export, as it has been shown by many researchers that fluvial sediment discharge from the rivers and thermal collapse and erosional input from the coastline ice-complex dominate in the shallow souther-eastern part of the Laptev Sea (Stein, 1998, 2000; Stein et al., 2004; Rachold et al., 1999; Semiletov, 1999ab; Semiletov et al., 2000, 2011; Mueller-Lupp et al., 2000; Dudarev et al., 2006abc). One additional reason for the low production in this particular area is light limitation because of high riverine CDOM and turbidity (Anna-Stiina Heiskanen et al., 1996, Yu. I. Sorokin et al., 1996; Semiletov et al., 2007; Pipko et

al., 2011). And again, our conclusions on this topic agree well with data presented in the sister paper by Karlsson et al., 2011 (this issue)

P1925, Lines 24-25. The reasoning here is unclear. It seems to me that there are potentially three main sources of POC in the system; river particulates, coastal erosion, authochthonous production. Both river and coastal erosion can have ancient components in their POC, but marine primary production would be modern. The discussion so far does not touch on the issue of carbon sources with any clarity or in a convincing manner that would suggest 14C could be applied to distinguish between organic carbon coming from rivers and from erosion. See for example Goni et al., 2005.

As mentioned above, two dominant sources of POC are existing in this part of the Laptev Sea: fluvial sediment discharge from the rivers and thermal collapse and erosional input from the coastline ice-complex, which is supported by many publications (Stein, 1998, 2000; Stein et al., 2004; Rachold et al., 1999; Semiletov et al., 2000, 2007; Mueller-Lupp et al., 2000; Dudarev et al., 2006abc), and marine biogenic source is small because of high CDOM and turbidity limit the light transparency (Anna-Stiina Heiskanen et al., 1996, Yu. I. Sorokin et al., 1996; semiletov et al., 2007; Pipko et al., 2011). We agree that 14C could be applied to distinguish between fluvial (mostly young OC from the surface soil) and eroded (old, mostly mineral) inputs. This approach was realized in the sister papers written by Gustafsson et al., 2011, Karlsson et al., 2011; and Vonk et al., 2010: the SAME ISSUE.

P1925 Lines 28 onward. There might well be two regimes in this system depending on whether or not wind/wave energy is getting into the water column as proposed. This finding would not be surprising and there are many other papers that discuss the effect of storms and surges on particle transport processes. The presentation of the two schemes in Figure 7 could be improved by giving a better caption that explains the difference between the regimes. I'm not at all sure what is being shown in the small picture panels to the right. There needs to be text highlighting to the reader what the significance is of these panels to the findings being presented.

We agree that we should make our new contribution to this subject more clear. Then we note that almost all previous data concerning coastal erosion into the shallow waters are based on ONSHORE morphological studies made by well known ACD researchers like Are, Grigoriev, Rachold, and others. How they did it: in good case they visited some areas at annual base and once per year, usually in the late July-early August, but they didn't studied the dynamics beyond their visits.   For example, the ACD people believe that quite high rates of coastal erosion was happened at the North Cape of Muostakh I. in 2008.   But it was happen there in September of 2007 (we saw drastic erosion up to 20-30m per week visiting this area twice in September of 2007) after they left from this site.   But we are not going to discuss all the ACD datasets in this ms because this is not a goal for this paper.   However, we quit sure that for this particular area we show for the first time that for the marine biogeochemistry the rates of "flushing" of eroded material from the beach are playing the role, not just rates of coastal erosion measured by quite a primitive way. To make the ms more understandable and readable we included additional comments to the Figure 7.

P1926, Lines 7-15. It would help if some quantitative and specific context were provided. For example, how stormy were these periods (and how was this measured), how long did the storms last in a given year before sampling, what was the ice cover for various years, and how strong were consequent storm surges (m)? The generalizations are of little help.

Guided by this comment, we included the Table of the storm frequency, waves height, and the ice thickness using the Navigation Book, 1998 (available in refence list)

P1926, line 14. What does 'considerable' mean and which 'pressures' are being referred to?

Thanks, we edited this sentence as following: The prevailing strong winds in the Gulf results in considerable storm surges on the Gulf coastline (Navigation book, 1998).

P1926, Lines 17-27. I have no argument with the conditions needed for resuspension, but did these conditions occur for the period in question, and if so, how were they measured? E.g., did currents exceed 25 cm s-1. By how much? For how long?

It is quite cumbersome to obtain large data set of bottom currents. We did measure the currents in 2008 using ADCP (limited number of stations). Therefore, we relied on literature data which are showing at what conditions the resuspension effect occurs (which we found in 2008 and called in this ms the "nepheloid layer").

P1927, Line 2. The points being made here toward the schematic in Figure 7 would be far better illustrated by sectional views than by comparisons between surface maps presented in Figures 2, 3.

We again agree with this comment: the sections of vertical distribution of suspended particulate matter has been included (new Figure, under construction).

Page 1927, Line 8. Where did these observations of wave height come from? When were they made? Likewise what is the next statement regarding what is usual for the Buor Khaya Gulf based upon?

We has been included the new Table of the storm frequency, waves height, and the ice thickness using the Navigation book, 1998).

Page 1927, Lines 15-17. Large volumes of terrigenous material? How much, how was this determined?

In this case we refered to the papers cited in the text.

Page 1927, Lines 20-25. I've no doubt that these islands have disappeared, but Fig 1 does not inform me much about where they were.

We didn't mark these retreated islands in the Figure 1, because it would be overloaded by information. So, for the interested reader we refer to Dudarev et al (2006a: the paper was translated in English).

Page 1928, Lines 10-15. Again, this would be helped by sectional views. It's very difficult to visualize the comparisons from the present set of Figures.

Agree: the sections of vertical distribution of suspended particulate matter and salinity has been included (new Figure, under construction).

Page 1929. Although I think I understand what the authors mean about 'accumulation only', one has to ask how accumulation can occur without sediment supply. The authors need to clarify exactly what they mean here.

Thanks: we rewrote this page showing that we meant under "accumulation" term. We meant that contribution of the coastal erosion is going down, fluvial input is decreasing from "small" to "negligible" values of PM/POC, and all the material from water column is accumulated because of no (or very low) resuspension and lateral (nepheloid) during such times.

Page 1929, Line 15. Discussion here would be helped by sectional views and/or references to appropriate literature on the functioning of the system in winter (e.g., Eicken et al., 2005) and in summer under low winds.

We again agree with this point: the sections of vertical distribution of suspended particulate matter, salinity, and ref. to Eicken et al., 2005 has been included (new Figure, under construction).

Page1930, *Lines 14-16. I'm unclear why Figures 1, 5 show that the Lena River delta has been growing. To do that, you'd need a temporal sequence I would think.*

This initial conclusion was based on our multi-year data, sat data, and the sea bottom morphological data. However, we have decided to remove this conclusion and instead consider writing a separate paper dedicated to this issue in the nearest future.

Page 1930 Lines 17-19. What are normal summers? What is the basis of comparison? How do you know there was less input if no numbers are available to compare?

Thanks: we rewrote this part of the paper supporting our discussion by our own multi-year and literature data

Page 1930, Lines 27-28. -29 ‰ is proposed as lighter than normal, with no explanation of how normal is defined and, again inferred to indicate a river source. The problem with the scattered commentary on 13C values in the manuscript is that no clear discussion is given anywhere on the end-member compositions for 13C, their variability in time and space and how they have been selected. Nor is any discussion given of results provided by other studies on this parameter and other markers of terrigenous carbon that might help with the interpretation.

Agree: we rewrote this part of the ms showing that "normally" (summers of 2000-2009, instead of 2008) the d13C-POC across the B-K Gulf is heavier than -27.5 ‰. However, in the anomalous summer of 2008 (see also Alling et al., 2010; Semiletov et al., 2011; Pipko et al., 2011) the d13C-POC was much river-born and lighter, which is agreeing also with molecular OC data (Karlsson et al., 2011)

Page 1931, Lines 16-18. We are now told that -26.4- -28.5 ‰ is typical for the Lena and for eroding ice. What happened to the -29 ‰ given earlier? The discussion around 13C is confused and unconvincing. And, in all of the discussion so far the production of organic carbon by ice algae (which can be very heavy in 13C) and marine algae has been completely ignored.

Thanks: we rewrote this part of ms showing that this " mean range " (-26.4-(-28.5) ‰) is typical for the BK Gulf (Stein, 1998, 2000; Stein et al., 2004; Rachold et al., 1999; Semiletov et al., 2000; Mueller-Lupp et al., 2000; Dudarev et al., 2006), except of summer 2008 when a strong and light riverborne signal was detected so far as north of the Novosibirsky Arc (discussed in detail in companion papers written by Alling et al., 2010; Sanchez-Garcia et al., 2011). This is agreed with results described in the sister paper by Karlsson et al (2011: this issue).

As we already mentioned above, two dominant sources of POC are existing in this part of the Laptev Sea: fluvial sediment discharge from the rivers and thermal collapse and erosional input from the coastline ice-complex which is supported by many publications (Stein, 1998, 2000; Stein et al., 2004; Rachold et al., 1999; Semiletov et al., 2000, 2007; Mueller-Lupp et al., 2000; Dudarev et al., 2006abc), and marine biogenic source is small because of high CDOM and turbidity limit the light transparency (Anna-Stiina Heiskanen et al., 1996, Yu. I. Sorokin et al., 1996; semiletov et al., 2007; Pipko et al., 2011).

Page 1932, Lines 1-5. Finally, marine carbon is brought up. The problem with the discussion is that there is river POC, coastal erosion, marine organic carbon (pelagic and ice algal) and algal carbon produced in the rivers, all of which have different and variable 13C. The discussion of 13C in this paper does not make contextual sense out of these varied sources. Of course with just 13C measurements, it is not possible to distinguish 4 potential sources. These problems must also be considered in the discussion of 13C in the subsequent text on pages 1932-1933.

We agree that the C13 data alone are not enough to be sure about the origin of the OC in the study area. Then, we added additional C/N data, and referred to the companion-paper written by Karlsson et al (2011), which employed more sophisticated techniques such as biomarkers and C14-OC partitioning approach for the same study area but only for the anomalous year 2008. Bottom line: our results based on the multi-year data described in this ms were proofed by the independent data sets obtained in 2008 (same area/same cruise) by Karlsson et al (2011). We furthermore provide more detailed sedimentology data and with wider spatial and temporal coverage. We also considered our new results in context of early published works (Stein, 1998, 2000; Stein et al., 2004; Rachold et al., 1999; Semiletov, 1999ab; Semiletov et al., 2007, 2000; Mueller-Lupp et al., 2000; Dudarev et al., 2006abc, Anna-Stiina Heiskanen et al., 1996, Yu. I. Sorokin et al., 1996)

Typos and smaller items.

P1918, Line 7. 'first recipient of the 16 overwhelming: : :' I don't get what the authors are saying.

This is a typo. It was replaced by: This study is investigating the coastal fate of the sediment and organic carbon delivered to the Buor-Khaya Gulf, which is the first recipient of the overwhelming fluvial discharge from the Lena River and is additionally receiving large input from extensive erosion of the coastal ice-complex (permafrost a.k.a. Yedoma; loess soil with high organic carbon content).

P1918, Line 11. The 250 stations. Were these sampled every year? Here and in the body of the paper, the number of samples used for various tasks (Table 1, Figures) is unclear.

As espoused above, the study area map (Fig. 1) is revised to clarify this. Thenumber of samples taken each year is now also shown in the dedicated table.

P1918, Line 16-20. Awkward sentence that might better be broken into two sentences.

We have extracted those sentences.

Page 1922 line18 criteria should be criterion.

Thanks: replaced.

P1924, lines 4-5. Fluvial sources of POC are…

Thanks: fluvial sources of POC have been replaced on riverine sources of POC.

P1924, lines 8-9. Is this statistically significant? This statement and a number of others are presented without any basis being given for the assertion.

Thanks: additional info was added.

P1924 Lines 13-14. What is the normal discharge (and variation)?

thanks: we included those data in the revised text

P1925, L12 has should be have.

Thanks: done.

General and specific comments by anonymous referee (Reviewer 2).

My major concern with this manuscript is the way the data is being presented. Most of the data presentation and discussion rely on colored graphs showing the areal extent, i.e. maps of the different properties being studied, i.e. SPM, OC, POC, d13C. From this type of descriptive graphic it is somewhat hard to actually follow changes quantitatively as they tend to be hidden by the graphic presentation. For example in Fig 2 the water column stratification is shown in three colors. Another way would be to pick out a few representative stations and show depth profiles with the stratification.

Thanks: the vertical distribution sections of suspended particulate matter and salinity has been included (new Figure, under construction).

The discussion regarding the d13C in section 3.3, which rely on the colored map in Fig 6 is somewhat hard to follow. Three of the panels marked a show the d13C in the particles from the water column and in one panel labeled b all the sediment data are reported. In the text it is suggested that the d13C in the sediments is a mixture of different OC sources including coastal erosion, river discharge and possibly primary production. From the figures it is hard to evaluate if this is actually the case. To show possible mixing I would suggest that the data are plotted somewhat differently maybe in a section with distance from the shore, or vs salinity or OC content. The end-members should be defined and their variability discussed. By doing a more formal treatment of the mixing, discussing different sources and showing this in plot it would be easier and

more convincing for the reader to understand what is happing to the OC.

Thanks: revised ms now includes a plot of OC vs d13C (new Figure, under construction).

More than 250 stations have been occupied and sampled for this report. From the text it is not clear to me how the sampling was actually done. Are the same stations being occupied every time? What sampling method was used? What time interval do these surface samples represent? What is the approximate sedimentation rate? I think these type of issues need to be discussed to give the large data set an improved frame. Analytical methods. How was the accuracy and reproducibility of the OC and d13C of □ 0.1‰ obtained? Is there a calibration need for the laser to obtain size distribution? If so how was this done? I think the analytical section need some more details, which also can include some references.

Thanks: we have included all the requested data in the Material and Methods.

On page 5 line 11-14 (Fig 4 and 5): "Spatial variability of the OC content in the bottom sediments follows the sediment size distribution (Fig. 5)". This might be the case but I cannot see this relationship, at least not from Fig. 5. In order to show this it would be easier to plot the OC content vs the particle size distribution.

Agree: revised ms includes a plot of PSD vs. OC (new Figure, under construction).

Two sedimentary regimes are defined "-on the basis of obtained results" (page 7, line 11). This is shown in Fig 7 but it is somewhat hard for me to understand that figure. Are the lengths of the red and blue arrows of importance? If so please clarify. What are the color pictures illustrating? I suggest a text explaining the differences between the two regimes and some reference in that text to the pictures shown in Fig 7.

Agree: we revised Figure 7 and captions to make it clear for readers.

RC: *In summary I think this manuscript report a large and important dataset, but my view is that the presentation and interpretation is very descriptive when mainly shown as areal maps of the distribution. I think there is potential for a more elaborated discussion which can be done by adding or replacing some of the "maps", with figures illustrating specific processes discussed, e.g. mixing. Also the interpretation of possible interannual variability should stand out more clearly by adding selected graphs for illustration.*

Thanks: we made the figures and text much more clear following your recommendations.

In the revised ms, statistical comparisons based on t-tests are instead included and the vertical distribution of suspended particulate matter and salinity are now included and discussed in the text. The study area map (Fig. 1) is revised to mark with different symbols, different types of oceanographic stations and for different years. Revised ms includes a plot of PSD vs. OC and OC vs d13C. We revised Figure 7 and captions to make it clear for readers.