

Interactive comment on “Space-time dynamics of carbon stocks and environmental parameters related to carbon dioxide emissions in the Buor-Khaya Bay of the Laptev Sea” by I. P. Semiletov et al.

Anonymous Referee #2

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The manuscript “Space-time dynamics of carbon stocks and environmental parameters related to carbon dioxide emissions in the Buor-Khaya Bay of the Laptev Sea” by Semiletov and coworkers presents a large dataset of hydrological parameters such as pH, total alkalinity, DIC, DOC, oxygen, nutrients, etc. from several cruises between 2000 and 2011 to the Buor-Khaya Bay in the Laptev Sea. The presented work is within the scope of Biogeosciences. The data are new and interesting and will substantially increase our knowledge on water and sediment transport to as well as carbon fluxes from the Laptev Sea. However, the presentation of the data should substantially be

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improved.

The abstract is very short and does not give a clear and in particular complete summary of what is presented in the manuscript. The authors state in the abstract that they present “. . . the variability of carbon stocks”. I see data on CO₂, DOC, POC, CDOM concentrations but I could not find data on the respective stocks. Furthermore, the authors did not describe how they calculated CO₂ emissions from the sea (and the land) surface to the atmosphere although this seems, according to the title and the abstract, the focus of their work. It is difficult to review the emission data presented in the current manuscript when it is unclear how they were obtained. It seems that they were not measured but calculated from the CO₂ concentrations in the water column. It should clearly be described how this was done. Hence I also recommend rephrasing the title because carbon stocks and are not the focus of the manuscript.

The authors present results and discussion in one section which often makes it difficult for the reader to understand what are new data and what has been published previously. I recommend splitting this section into two separate sections clearly presenting the new data of the current manuscript and discussing these data subsequently considering current knowledge.

At the end of the manuscript, fluxes from different land sites (beach, near-shore and Primorsky coastal plain) are reported without describing neither these sites (e.g. vegetation, temperature, thawing depth, water content, sediment characteristics) nor the methods used for flux measurements. This information is essential to assess the reason for the very different fluxes.

Furthermore some of the very general conclusions given at the end of the manuscript are not supported by the presented data and should be rephrased (see below).

Further comments:

P2161 | 5: ESAS has been explained before

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P2161 | 24 must read “ In the ESAS”

P2162 | 1ff: The second part of this paragraph is unclear. First it should better read OM not OC because carbon cannot be degraded. Furthermore, is the OC in the water column and the sediment DOC or POC? What is erosion-induced OC? Do you mean OC released by erosion?

P2162 | 9: What do you mean by biologically immobile? Please specify. I understand this as inert to microbial degradation but the next sentence seems to imply the opposite.

P2166 | 7 ff: Please describe the analytical methods, esp. if referring only to Russian literature which is not available to most of the international readers.

P2166 | 15 ff: Please describe in more detail how you treated the DOC samples after sampling and for storage. Did you filter the water? Did you poison the samples?

P2167 | 4 ff: What was the threshold between “coarse” and “fine”.

P2167 | 14 ff: How deep is “top-most”.

P2167 | 17 ff: Is bottom sediment the same sample as top-most surface sediment?

P2169 last three paragraphs: The description of the two sedimentation regimes is not very clear. Please clearly describe the characteristics of type 2. In line 3 Type 1 is erosion-accumulation, in line 4 type 1 is accumulation. Please clarify.

P2172 | 17 ff: It is unclear why the same nitrate concentrations should result in the same OM degradation rates. The most important parameter for OM decomposition rates is the availability of OM which might be lower when the Lena river discharge is higher (see your discussion in the first paragraph of this page).

P2174 | 5: should read “concentrations” not rates. Are nutrient concentrations in the Lena river discharge high? I understood from the preceding paragraph that they are not.

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P2175 | 14ff: Please explain how you calculated the fluxes from the concentrations. I assume the fluxes were calculated from the concentrations measured during the cruises and hence do not represent a whole year but only the situation during the sampling. Hence the month of sampling should be given as well (as in graph 15). Furthermore please give also the mean value (or median) of fluxes not only the range.

P2176 | 5ff: How did you measure the CO₂ fluxes on land? Please describe in M&M. For better comparison please give not only the range but the mean (or median) value. Where is the Primorsky coastal plain and how is it characterized. Is it on Moustakh and similar as the sites you measured on Muostakh (Fig. 16). And what is the difference between “near-shore” sites and “beach” (esp. concerning the C-content of the sediments). The last paragraph is somewhat confusing because it mixes CO₂ fluxes on land with those at sea sites (lines 12ff) which indeed are difficult to compare.

P 2176 | 21f: Is this indeed an increase over the last two decades or only interannual variability? The full dataset of discharge over the study period would help.

P 2176 | 23f: The conclusion that “fresher and warmer water [is] observed in summer and winter” is too general. “Fresher and warmer” in comparison to which time period? This issue has not been discussed in the manuscript and only T data from two summers and spring cruises are presented without discussing them. I recommend omitting that conclusion.

P 2177 | 10f: The conclusion that the data imply “that the sea surface is a more significant source of CO₂ to the atmosphere than the tundra” is too general and not supported by the presented data. The manuscript reports land fluxes of up to 319 mM m⁻² d⁻¹ which are much higher than the sea surface fluxes. Furthermore, there are several studies on tundra CO₂ fluxes in the literature even over whole vegetation periods but none are discussed in this manuscript. The presented sea surface CO₂ fluxes from two sampling campaigns in the Laptev Sea are not sufficient for assessing the quantitative importance of sea surface versus tundra as source of atmospheric CO₂, esp. when

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considering that the tundra fluxes are not discussed.

P2177 | 12ff Please clarify that the 2006 data are yet published and not original data from this manuscript. Please give mean (or median) values for the fluxes for better comparison. Furthermore it should better read “coastal area of Muostakh” because this seems the only area that was studied.

Fig. 3: Can you present the river discharge over the whole study period until 2011. The data are mentioned in the text and hence should be given also in the graph. Furthermore it is unclear what the green, red and blue color in panel b represents. Please specify.

Fig. 4: please note the range of salinity below the color codes (not only numbers explained in the caption). Please use the same color codes for the same values for bottom and surface and add the unit for the measurement.

Fig. 5: please note the range of temperature below the color codes (not only numbers explained in the caption).

Fig. 6: please label the color codes as recommended for Fig. 4 and 5.

Fig. 7: please label the color codes as recommended for Fig. 4 – 6 and use the same color code for all panels.

Fig. 8: The color coded parameters should be at the color code (not above the graph) as in Fig. 12 and the units of x and y axes and of the color code should be given in the graph (not only in the caption).

Fig. 9: see comment Fig. 8, panel c should read SPM (?)

Fig. 10: see comment Fig. 8

Fig 11: see comment Fig. 8, for better comparison, the color code of panel a and d should be the same.

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Fig 12: please give units for x, y axes and color codes in the graph.

Fig 13: see comment Fig. 8, please label color code and give all units.

Fig 14: please label the color code with the parameter presented and the respective units, for better comparison, the color code of all panels should be the same.

Fig 15: see comment Fig. 14.

Fig 16: If data from panel b was published in Vonk et al. it should be indicated in the subtitle.

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