

Interactive comment on “Distribution of methane in the Lena Delta and Buor Khaya Bay, Russia” by I. Bussmann

Anonymous Referee #2

Received and published: 15 January 2013

The manuscript submitted by I Bussmann presents data in an interesting field of research currently gaining increasing interest. It presents data from the Lena Delta and the Buor Khaya Bay (Laptev Sea), thus a river and estuarine system affected by thawing permafrost in the drainage basin, a process considered to have impact on the greenhouse gas and carbon cycles. The data were gathered in the summers of the years 2008 to 2010, and include measurements of dissolved methane in the water column and its stable carbon isotopic signature, salinity, temperature, and a series of incubation experiments designed to address the methanogenic and methanotrophic potential of bacteria in meltwater, freshwater and the most saline endmember of the study. The measurement of dissolved oxygen and pH is briefly mentioned, but data of these parameters are not shown nor mentioned and discussed in the text.

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Data in this area are difficult to get, sparse, and valuable. However, my major problem with this work is that neither the sampling approach nor the interpretation is addressing defined scientific hypothesis which could be significantly supported or even resolved. Reviewing the paper was severely hindered by the fact that none of the geographic features referred to in the text are labeled in the overview map (Fig 1) or anywhere else.

There is a complete lack of information about the strategy of sampling. The study obviously did not try a repeated sampling, with very different areal coverage and spatial distribution during the different years. Thus, it is unclear what the scientific point in the comparison of the median concentrations between the different years might be. Some differences in the patterns of salinity or temperature are mentioned, but there is a complete lack of information of interannual changes in the situation during the investigations (exact time of sampling, variations in regional climate at the time of and prior to sampling, year to year differences in run off). Data of flow velocity (and thus runoff) are apparently available, as river velocity data are used for a (very crude) water-air transfer coefficient. So, interannual variability cannot be addressed with the data set.

The derivation of a methane flux estimate is also difficult with the data presented. There is a complete lack of the discussion on the transfer coefficient k , a large uncertainty, and of course a complete lack of framework in the annual cycle. It is reasoned why the fluxes in summer potentially constitute the maximum flux, however solely based on the consideration of temperature. Variations in the flow regime or the annual cycle of meltwater production are lacking. With this in mind, the comparison between the aquatic and terrestrial setting (based on the data of the Samoylov Research station), which would be of large importance, is not supported sufficiently.

The author seeks to derive information about oxidation by considering a simple Rayleigh fractionation model. The problem is that the nature of the approach (which requires a closed system) is disproved by the data shown, including the increase of CH₄-

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concentrations along the river path, the mentioning of gas ebullition in some places, the outliers in the data set (which are “unexplainable data” rather than analytical outliers). As a side note, the removal of “outliers” from the interpretation, in particular when calculating averages etc., without any hint of analytical problems (i.e. rather with a lack of hypothesis for the data) is scientifically questionable. The author shows that the river and Bay data cannot be explained by dilution of a riverine end member with the water. In fact, also a proof that the riverine signal is significantly affected by the input of methane-rich meltwater is missing, as this would require a consideration of the water volumes and balances.

Lastly, the incubation experiments to test the potential for methanogenesis and methanotrophy provide an interesting approach. However, care has to be taken with respect to the interpretation of differences of values do not differ in the range of their error margins. I agree with the first reviewer that high rates of methanogenesis in oxic waters, in particular in a turbulent regime, are hard to explain.

The lack of hint for oxidation processes is indeed a surprise. Even more in connection to a data set which really displays a unusual and unforeseen heavy isotopic signature in all off the reservoirs investigated (meltwater, lake, estuary). Given the assumed biogenic source of the methane pool and the cited data from other Arctic regions, this is – if analytically bullet-proof, the potentially most interesting finding. So where is this rather heavy isotopic signature coming from? Is it a hint for efficient oxidation of a large part of the methane somewhere? This is completely unexplored in the current manuscript.

In summary, though data of this region are sparse and valuable, I have severe problems with the approaches, and interpretation of the manuscript. Some of the major questions are not answered and statements made not really supported by the data. At the other hand, the potentially most interesting findings (heavy isotopic signature, no hint for oxidation) are not explored. Given the high level of Biogeosciences, I cannot suggest the publication of the manuscript.

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Minors: Figure 1: pls indicate all locations named in the text. Use greyscale to distinguish land and water areas, highlight the positions used for isotopic and oxidation rate measurements (at the moment, they are less visible than the standard station) . . .

Figure 2: pls indicate the positions of the actual sampling positions and color code with actual values. In the way it is presented, it is not possible to evaluate the accuracy of the gridding interpolation scheme. Also, the figure caption is only referring to the right panels.

Figure 8: please use symbols which could be easily distinguished

16216: Study site: give exact duration of fieldwork 16217, line 12: the reproducibility for the $\delta^{13}\text{C}(\text{CH}_4)$ measurements cannot be given in “percent”.

Interactive comment on Biogeosciences Discuss., 9, 16213, 2012.

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