Anonymous Referee #2

I would like to thank the reviewers for their valuable comments, and the Ms has been restrutured and additional calculation were added. I believe that the changes made have improved the Ms substantially.

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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.

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 severly 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.
A new figure 1 is now presented with labelling of the river channels and features in the Bay.

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.
A more detailed description of the study site is now added (2.1). Also the problem of different locations is now adressed. Also are the data now splitted into creek, river and estuarine water from the different years (new figure 3). Stations in the river were mostly at the same positions (within 0.05°). In the estuary we tried to sample at the same or at least comparable stations, but navigating with russian ships at different cruises is not so easy.

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) waterair transfer coefficient. So, interannual variability cannot be addressed with the data set.
For the different years the amount of data available is very different. Thus I restricted the more detailed analyses to 2010 when data on river characteristics are also available. With the incomplete data set for the different years, I did not intend to address interannual variability.,Only in some cases simple comparisons were made.

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.
A discussion on the transfer coefficient is now included in the method and discussion section. Also k600 is now calculated for better comparison with other studies.
It was not possible to estimate or calculate fluxes in other seasons, as all data needed are from the summer season. It could be that the winter flux may be even higher than the summer flux. This aspect is now discussed.

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 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).
The Rayleigh fractionation model was only applied to the data of Buor Kaya Bay, where the assumption of a closed system is more realistic.

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.
At least for the river section a balance of methane fluxes is now presented and some estimations of the riverine input for the Bay area.

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.
I agree that the outcome of the experiments was rather surprising, and is certainly difficult to explain. I hope to repeat similiar experiments this year, taking more care on the oxygen concentrations. Therefore I decided to omit the experiments, but only state, that methane oxidation was not observed, and that there are indications for a methane production rate.

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 have 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.
The unexpected heavy istoptoic signal of the methane is now discussed in more detail. In the estuary a thermogenic origin is discussed [Cramer, 2006 #1918] and in the river interactions of the methane with humic substances makes interpretations of the methane origin difficult [Whiticar, 1996 #2242].
The undetectable methane oxidation is difficult to explain. In the estuary, the changing salinity could inhibit methanotrophic bacteria (as indicated by recent unpublished data). In the river we assume other environmental factors which lowered methane oxidation rate below our detection limit, further experiments are planned therefore.

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.

Minors: Figure 1: pls indicate all locations named in the text. Use greyshade 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) .
In the new figure 1a +b (river and estuary), the name of the Channels and their mouths are now indicated. Also the asterics are now made better to distinguish.
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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.
In the modified figure 2 only the salinity data are now presented. The positions of this transect are indicated in the lower map. Also the actual data points are now indicated, but without the actual data as this would make the figure too overloaden.

Figure 8: please use symbols which could be easily distinguished
Open and closed dqaures are now used as symbols.

16216: Study site: give excact duration of fieldwork
These data are now included:
„ Water samples were collected during three summer expeditions (9.8 -14.8 2008 [Boike, 2009 #2165];, 14.8 -23.8.2009 [Wetterich, 2011 #2164] and 29.7 – 9.8.2010) within the main channels (Trofimovskaya, Bykovskaya and Olenekskaya Channel) of the Lena River and Buor Khaya Bay (**Fehler! Verweisquelle konnte nicht gefunden werden.**a). Stations in the Bykovskaya and Olenekskaya Channel were identical (within 0.05°) in 2009 and 2010. Stations in Buor Khaya Bay were only partly comparable between the years (**Fehler! Verweisquelle konnte nicht gefunden werden.**b). “

16217, line 12: the reproducibility for the d13C(CH4) measurements cannot be given in ╥percent╨.
Originally the standard error as the standard deviation given in percent of the average was given. It is no change to range of standard deviations obtained and given in ‰.