Dear Dr. Zhongjun Jia,
Thank you for your decision and comments from reviewer 1.
I have now addressed all points raised and hope that critics on the MOX calculation are clarified to satisfy you and the reviewer.

Yours sincerely

Ingeborg Bussmann

Dear Dr. Bussmann,

Your manuscript has been reviewed by an external expert and myself. The criticism raised during the first round of reviews has been addressed, but still the justification of methane oxidation rate needs to be strengthened and discussed in greater details.

Meanwhile, the figure quality can be significantly improved, particularly for Figure 1. Please also remember that it is expected that your figure legends will be quite detailed and very precise. In fact, from the figure title and the axis labels of a graph/table the reader should be able to determine the question being asked, get a good idea of how the study was done, and be able to interpret the figure without reference to the text

Yours sincerely

Zhongjun Jia

The legend of figure 1 has been extended. Also, the legends of the other figures have been checked and amended.

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The manuscript "Methane dynamics in three different Siberian water bodies under winter and summer conditions" by Bussmann et al. has been substantially revised by the authors and most of my previous comments have been taken into account.

I still think that the argumentation on the MOX rates could be further strengthened, however. The discussion of the manuscript heavily relies on the calculated MOX rates, and in my previous review of the manuscript my main criticism was that I missed a chain of arguments that justifies the calculation of MOX rates for all water types from the fractional MOX rates measured in ice core and river samples.

In their answer to my review, the authors agreed that their approach has some drawbacks but stated that due to the difficult logistics, transferring the MOX rates measured in the ice core and river waters to the other water bodies is the best they can do. While I understand that conducting MOX rate incubation experiments under field conditions is very challenging, I

still think the authors need to discuss the limitations of their approach in the manuscript more clearly. In lines 449-454 the authors state that transferring the MOX rates from ice core and river data to the other bodies is justified due to the fact that MOX rates are generally low at low temperatures and due to the fact that the methanotrophic population (does this refer to the community composition?) of the ice cores is similar to the one measured in the water below. While I agree that the temperature dependence of the MOX rate indicates that MOX is low at low temperatures, I am not fully convinced that other factors have sufficiently been taken into account by the authors. Particularly the influence of CH4 concentrations on MOX rates may be of importance, as CH4 concentrations in the lake and the coastal embayment were significantly higher below the ice than in the river water.

The MOX rate is calculated by multiplying k' with the corresponding CH4 conc, thus only k' is per se independent from the CH4 conc. However, on a physiological or population level k' may well depend on the substrate concentration. We tested this for our data set: for methane concentrations ranging from 3 – 800 nM k' is independent from the CH4 concentration. Studies from Mau et al 2017 and Steinle et al 2017 support the fact that the k' to CH4 relation does not necessarily apply. However, it cannot be excluded that at the very high methane concentration as in Lake G. k' may also increase. Thus, our estimations of MOX would be an underestimation of the real rates and real k'.

The respective text in the discussion has been changed to:
In this study we determined the methane oxidation rate with tritiated methane as tracer. The advantage of the tracer injection method is that natural low concentrations are hardly altered and thus we assume that our values are close to the actual rates. The fractional turnover rate k' was determined in ice cores from the lake and Tiksi Bay, and in river water, but not for water samples from the lake and Tiksi Bay. Within these locations k' was evenly distributed. However, k' may vary between different environments (river, lake and brackish water) as well as between ice cores and underlying water. The fractional turnover rate is influenced by temperature, methane and oxygen concentrations (Steinle et al., 2017).Temperature was low at all locations and should not have a big impact on k'. For methane concentrations ranging from 6 – 800 nM, k' was independent from the methane concentration. Studies from Mau et al 2017 and Steinle et al 2017 support the fact that the k' to methane relation does not necessarily apply. However, it cannot be excluded that at the very high methane concentration in Lake Golzovoye the real k' may have been larger. Thus, at very high methane concentrations our estimations of MOX would be an underestimation of the real rates and real k'. For all other samples, we suppose that the application of one k' to all samples is the best possible assumption.

I furthermore have some minor comments that should be taken into account:

Lines 34 and 36: replace "10-times" and "40-times" with “10 times”, “40 times”
corrected

Line 55: I guess it should read “from 2.2 ppb/yr to..."
corrected

Line 67: replace “concentration” by “mole fraction”

corrected

Lines 67-69: CH4 emissions from water bodies (incl. lakes, rivers, coastal waters) have not been mentioned in the introduction before. How significant are these emissions in relation to other sources in the northern latitudes?
Methane emissions from lakes and rivers are discussed in the following paragraph.

Lines 189-193: if the samples are transferred to Nalgene bottles before filling the glass bottles, how is CH4 loss from the samples prevented?
The Nalgene bottles were completely filled to the top and transfer to the glass bottles occurred within 3 hours of sampling. This procedure does include a certain methane loss, however handling tubings, stoppers, cannulas and glass bottles at freezing temperatures proved to be more error bound.

Lines 313-315: "In Figure 3 the median concentrations in the … ": this formulation is somewhat strange. Please rephrase this sentence to something like "Figure 3 shows the median CH4 concentrations in the ice core and at in the water from the ice-water interface in…"
corrected

Lines 319-320: there seems to be something wrong with the reference that should be cited here.
corrected

Lines 328-329: can the authors give additional information on the CH4 concentrations in the samples that were used to determine MOX rates? Did the authors find any correlation between MOX rate and concentrations?
As outlined above, only k' is per se independent from the methane conc., while MOX is calculated by multiplying k' with the methane concentration. Thus, we focus more on the discussion of k'. In the samples in which k' was determined, methane concentrations ranged from 3 – 800 nM and k' was independent from the M-conc.

Lines 333-335: This sentence repeats the previous one.
The latter sentence has been deleted.

Lines 337-340: Does this mean that an overall median MOX (mean of ice cores and water samples) is transferred to the entire dataset? If MOX in 73% of the ice cores is below the detection limit would the overall median value from the ice cores not be below detection limit, too? Would it not make more sense to transfer the MOX rates from only the water samples to the other water bodies?
As explained in the text, in all positive (ice core) samples the median k' was 0.003. The samples below the detection limit were excluded here. The k' from positive ice cores samples and from river water samples was both 0.003.

Line 366: "lower than in summer"
corrected

Lines 618-619: this sentence is a repetition of lines 578-579.

The latter has been changed to "A shortened time of ice coverage on the water bodies is predicted with increasing temperatures in the Arctic"

Table 1: maybe the information on samplings for the MOX rate measurements can be added here? I think it is interesting to know where and when the authors took the samples for these incubations.
This information has been added to table 1

Figure 3: I would suggest to swap the data from Lake Golzovoye and Tiksi Bay, to show the Lake Golzovoye data next to the data from the separate ice core from the lake.
Changed accordingly