

Review BGD *Methane distribution and oxidation around the Lena Delta in summer 2013* by Bussmann et al.

Bussmann et al. present data from a measurement campaign in September 2013 in the coastal area close to the Lena river delta where river water and polar water mix. The activity (qPCR) and the abundance of methanotrophic bacteria was investigated and statistically compared to methane concentrations and physico-chemical parameters in order to determine environmental controls of MOX. Three water masses (river, mixed and polar) were defined previous to statistical analyses. This manuscript employs primers developed by Tavormina et al., which were even improved since the last publications by these authors. The use of these primers to investigate the methanotrophic marine community is quite new and I think that this is the strongest point of this manuscript. Conventional primers often don't cover the marine diversity. I enjoyed reading the manuscript since it is clearly written and everything is well-explained and a wide-range of literature is being put in context with the results of the presented study. There are, however, quite a few formatting/language mistakes. More importantly, I'm missing a more conclusive discussion (see below). If the remarks below can be addressed, most importantly the discussion, this manuscripts presents a solid addition to the current scientific pool of MOX studies and is suitable for publication in BG.

General remarks:

1) Did you try to analyze the data statistically without grouping it into different water masses? What are the results then? Or maybe set the salinity borders differently?

Yes, we worked also with the whole data set, but no clear patterns were discernible then. We also applied the salinity border of Goncalves et al (at the same study site), but clearest results were obtained with the classification of Caspers. Also with North Sea data this was the "best" classification.

2) It would be interesting to do qPCR with sediments samples from the river and coastal area. Especially for the 'outlier station' where authors hypothesize that part of the community got resuspended due to stormy weather. Was this done?

Unfortunately we did not extract DNA from the sediment, even though it would have been important and very interesting.....

3) The discussion is quite descriptive. I'm missing a more in-depth analysis of the results. For example, the third paragraph of 4.2 is very descriptive. What are the possible reasons that these communities are limited by different factors? Why is the riverine community more diverse? Due to stability? My opinion is that for the MS to be published in BG a less descriptive Discussion part is crucial.

We added the following paragraph to the section 4.2:

Methane concentration and nitrogen availability are strong driving forces shaping MOB community composition and activity (Ho et al., 2013). Furthermore the interactions with other heterotrophic bacteria influence the methanotrophic community (Ho et al., 2014). As DOM removal and degradation occurs mainly at the surface / riverine water (Gonçalves-Araujo et al., 2015); this may also lead to an enriched methanotrophic population in the riverine water. We also assume that the riverine environment is exposed to more environmental changes (salinity, light), temperature) than the polar one. Changes in salinity have different impact on sensitive and non-sensitive MOBs, thus also shaping the methanotrophic community (Osudar et al., in revision). In contrast to our more divers riverine population, the methanotrophic population in the proper Lena river was

characterized by a rather homogenous community (Osudar et al., 2016). However, the classical concept of r- and k-strategist nowadays has been replaced by the C-S-R functional classification framework and type Ia MOB, responding rapidly to substrate availability and being the predominantly active community in many environments can thus be classified as competitors (C) and competitors-ruderals (C-R) (Ho et al., 2013).

4) A wide range of statistical data is presented. It would be better to discuss the most important findings to avoid confusion of the reader.

We moved 2 tables with statistical details to the appendix, and hope to make the text clearer.

Several small remarks, also with regard to formatting/language mistakes:

-please check upper/lower case of chemical formulas/mathematical formulas

We checked the text again and hopefully have now found all errors.

-abstract line 11: biological “way” sounds a bit strange. Maybe biological sink?

Changed accordingly

-abstract line 21: riverine, not rivine

Changed accordingly

-abstract, line 22: “..riverine water TO (not AND)..”

Changed accordingly

-abstract line 17: “..a median OF 28 nM..”

Changed accordingly

-line 44: hydrate not hydrated

Changed accordingly

-several times you write ‘according to/XX to (XX et al, 1998)’. Please put the parentheses at the right place.

We checked the text again and hopefully have now found all errors.

-2.2 why are you using different chemicals (H₂SO₄ and NaOH) to kill samples for methane analyses for sediment and water samples.

When measuring MOX the control values were lowest when applying H₂SO₄ to the water samples, thus we used the acid for all water samples. For sediment samples we used NaOH to avoid dissolution of any carbonate and subsequent CO₂-production.

-if you’re sampling sediments with a grab sampler for methane analyses, is there not a lot of methane lost on the way up to the ship?

The study area is very shallow, max. depth 20 m, thus the grab sampler took only few minutes to return on board.

-line 199: remove the ‘than’

Changed accordingly

-line 238: herEby

Changed accordingly

-if you're correlating MOX to CH4: how can you be sure that's possible since $MOX=CH4*k$. Isn't what you're calculating then just assessing if k is much smaller than the CH4 concentration (which it generally is).

Yes, we are aware that this correlation is "difficult", because of this autocorrelation. Nevertheless, it is often used in the literature and the differences between the 3 groups are very strong. We added the following sentence "However as MOX is calculated with the methane concentration, this correlation has to be regarded with caution."

-line 311: "...seemed to be.."? or there was none?

Changed to "there was no significant difference"

-line 324: degradation processes? You mean methanogenesis in the sediments?

Yes, changed to "This correlation can be related to degradation processes finally leading to methanogenesis,..." "

-line 334 and after: I can't really follow your explanations. Could you rephrase/shorten/write it clearer. I might have missed something but I did not get your point.

We try to explain the missing correlation between freshwater input from the river and the methane concentration. If there is another freshwater source (from ice melting) with low methane concentrations (in contrast to the riverine freshwater with high methane content) this could explain the missing correlation. We rephrased the paragraph to make it clearer.

-4.2: there was recently a paper published in BG about MOX in coastal environments (Baltic Sea, Eckernförde Bay). Would be good to include it.

This work is now included.

-line 356: "...fractional turnover rateS.."

Changed accordingly

-line 375: "...but more..": what do you mean? More than no correlation? Please rewrite.

Changed to "but correlations to"

-line 380: what's the difference from dormant MOB to not active MOB? Do you mean dormant, for instance as endospores? Please write more clearly. Like this, it reads like a repetition from line 376.

Yes, it is a sort of repetition, but the first (in line 376) is a general statement concerning the restriction of the method, and the line 380 refers to more specifically to methanotrophic bacteria.

-line 403 and 407: limited or influenced? I would prefer a clearer way of writing this.

Ok, they were limited (negative correlation)

-line 433: where was Graves et al., 2015 measuring fluxes?

They calculated the methane flux, as the other studies in this sentence.

-line 437: did Sapart et al. not measure atmospheric fluxes? Graves et al., 2015 also measured atmospheric methane.

Yes, they also measured the atmospheric concentrations, but the flux was calculated

based on the water borne methane concentrations (bottom up). In contrast to Myrhte and Thornton, whose flux calculations were based on the atmospheric concentrations (top-down).

We changed the sentence to ... few studies focus on the atmospheric concentrations...."

-line 439: remove the ":"

Changed accordingly

-line 443: what is ESAS?

East Siberian Arctic shelf (ESAS)

-line 447: change than to then (also at other places in the MS, please double-check)

Changed accordingly and throughout the text

-line 451: there was recently a paper published in BG about MOX in coastal environments (Baltic Sea, Eckernförde Bay). Might be interesting to compare the two.

A comparison is now included in the text, L463 ff

-Figures made with Ocean Data View: Make sampling spots more visible! It would be better not to use the mode where two data-points merge together (interpolation) since there are so few data points.

The stations are now indicated with a black dot within the colored circles (Fig. 3, 6 and 7), in figure 2 the stations are indicated with a vertical line.

-Figures: check lower/uppercase

Changed accordingly

-Table 5: there is not a very good coverage for shelf seas (eg North-Am. Coast, Baltic Sea)! I enjoy this table and it would be good to extend it a bit.

The Baltic Sea and the North Am Coast are now included!