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Biogeosciences Discussions

Interactive comment on "Effects of low oxygen concentrations on aerobic methane oxidation in seasonally hypoxic coastal waters" *by* Lea Steinle et al.

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Steinle et al. found that oxygen concentrations and temperatures control the rate of aerobe methane oxidation in coastal waters. The authors sampled the water column at a station with regular hypoxic conditions every 2-5 month over a time frame of two years. They measured physico-chemical water properties, methane concentrations, and methane oxidation rates (MOx). They observed that although methane concentrations are not linked to the different seasons in the bay, MOx related to the seasons. Higher MOx were measured in the summer/fall when the water column was stratified in contrast to lower MOx in winter/spring when the water column was mixed. The higher rates correlate with higher temperatures and lower oxygen concentrations during the

stratification period and the lower rates relate to lower temperatures and higher oxygen concentrations in a mixed water column. Experiments were implemented to validate the correlations. Highest MOx-rates were measured at a temperature of $\sim$30°C, except when North Sea inflow water was sampled, then highest MOx-rates were measured at $\sim$10°C. The experiments further showed that methane oxidation rates were elevated at lower oxygen concentrations compared to higher oxygen concentrations, with increased biomass incorporation at high oxygen concentrations. Both field data and experiments thus indicate a negative correlation between oxygen concentrations and methane turnover.

Although culture studies of known methanotrophic bacteria have already shown the influence of oxygen and temperature on microbial methane oxidation as summarized by Hanson and Hanson (1996, Microbio Rev, 60, 2, 439-471), investigations of these relationships in the marine realm lag behind. Steinle et al. thus target a severe knowledge cap in the marine methane cycle. The authors present a comprehensive, well written study that comprises a rather long time series and a combination of field data with experiments. While the former should be highlighted as it involves a lot of logistical work, the latter should be stressed as a solid scientific approach to analyze field data and evaluate the results by implementing laboratory experiments. Therefore, I recommend publication of the manuscript after considering my few comments and remarks.

I start with some major remarks:

1. Section 4.2 of the discussion: I don't understand why the authors do only shortly discuss different methanotrophic bacteria as the source of higher/lower methane turnover. It is known that type II methanotrophs can utilize methane better at low oxygen concentrations whereas type I methanotrophs utilize methane at higher oxygen concentrations (Amaral and Knowles, 1995). There is also a difference of biomass incorporation between these two types; type II can assimilate much higher portions of CO2 as carbon source (up to 50%) than type I methanotrophs (up to 15%) (see reference in Strong et al., 2015, Environmental Science and Technology, 49, 4001-4018). Taking both

aspects together could explain the different assimilation patterns and turnover rates obtained.

2. Section 4.3.1 of the discussion: By using the temperature dependence, you can calculate the Q10 factor and compare with the one derived in Bussmann et al., 2015 (L&O Methods, 13, 312-327). Taking the Q10, you can further evaluate the in- or decrease of MOx-rates due to temperature and compare those with your field data. If you know the MOx-rate change due to temperature, you can differ between the temperature and oxygen effect in your field data. It would be great to know if temperature or oxygen has a stronger effect on methane turnover.

3. Section 4.4 of the discussion: The fraction of MOx (FMox/Ftot in %) is high during stratification, because Fatm is low, not because of higher MOx-rates (attached Fig. 1). Water column stratification clearly affects the sea-air flux (right graph of Fig. 1). I see a better distinction between the seasons by solely comparing FMOx. If I list FMOx from lowest to highest, I get: spring 11.7, summer 12.7, winter 14.5, summer 27.3, fall 28, fall 29.6, fall 33, fall 82.3 μmol/m2d, that is FMOx is always higher in fall compared to the other seasons. As this already illustrates that 'MOx exhibits a seasonal variability', why do you calculate the MOx-fraction of the total loss terms of methane (FMOx + Fatm)? I recommend to stick to your results of FMox and Fatm and delete the Ftot assumption as the flux of methane from the ground was not measured. Ignoring dispersion and advection of methane in the water column might be an oversimplified view, which can easily produce wrong results.

4. Figure 4: Do you have any explanation, why so less carbon was assimilated? I know ratios of biomass to CO2 of 0.12-0.4. Your results appear much lower.

Minor remarks:

Page 1, line 21: Please change 'always' to 'generally' as you write later in the manuscript that in Nov. 2013 MOx-rates were higher in the water column than just above the ground.

Page 2, line 6: Here you could use an 'old' reference in addition, which describes the degradation of organic matter by methanogenic archaea, otherwise it seems like it is a newly discovered process.

Page 4, line 17: The use of mercury chloride solution should be avoided as it is very toxic. In addition, mercury chloride can be transformed to less toxic substances by methanotrophs and thus might not poison all methanotrophs (Boden and Murrell, 2011, FEMS Microbiol Lett 324, 106–110).

Page 5, line 25: It would be nice to include the difference of the temperature of the experiments to the in situ temperature in this section, otherwise the reader has to search the text for it.

Page 6, line 11: The value 600 is the Sc of CO2 at 20°C in freshwater. The Sc of CH4 is slightly different: 617 at 20°C in freshwater. Wanninkof, 2014 (L&O Methods, 12, 351-362) forwards an equation to derive Sc for both seawater and freshwater from temperature. Please include an error evaluation, what effect does this change have.

Page 6, line 17: The numbers of kW are for CO2, please state this in the text.

Page 8, line 2, 3, 5: Why is there a difference in oxygen concentration between 3H-CH4 and 14C-CH4 experiments? For 3H-CH4 experiments you differ between above and below 15 μmol/L, but for 14C-CH4 you differ between above and below 0.5 μmol /L.

Page 9, line 28: Are the results of the pearson and two-tail's student test derived for the relationship k v. CH4 or MOx v. CH4? I assume it is for the relation k-CH4, could you include the test results for MOx-CH4, too.

Page 12, line 25: This unusual oxygen profile is not visible in Fig. 2.

Tab. 2: The units are wrong. It should be flux units per unit square area (μmol m-2 d-1), not per volume.

Overall, the scientific outcome of the manuscript is sound and will remain; I suggest only to add some valuable details. I enjoyed reading the manuscript and look forward to its final publication.

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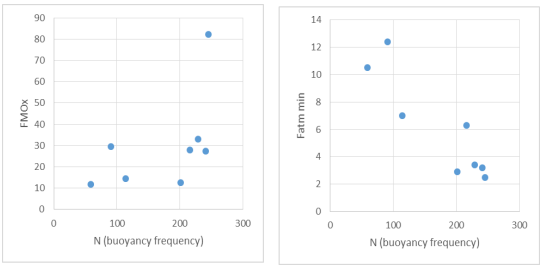

Fig. 1: Plots derived from your data in Table 2.

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