

## ***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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This paper by Steinle et al. examined the effect of O<sub>2</sub> concentration on aerobic methane oxidation rates at a 2-year time-series station in the Baltic Sea. They found that CH<sub>4</sub> oxidation rates increased with increasing water depth; methane oxidation rates were highest when O<sub>2</sub> concentrations were lowest and water temperatures highest. Overall, I found this to be an appropriate paper for Biogeosciences. It addresses the eventuality of increased marine methane release due to climate change and how potential simultaneous changes in physico-chemical parameters (e.g. increasing temperatures, decreased O<sub>2</sub> availability) might possibly create a positive feedback to quench atmospheric methane release, in at least the Baltic Sea. However, I was left

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frustrated that the authors continuously postulated hypothetically on their results without delving into actually exploring them in their discussion. For example, the simple act of investigating the aerobic methanotrophic community structure in their samples would allow them to do more than speculate about (1) temperature effects and water inflow for the North Sea causing certain time events to have dissimilar methane oxidation rates, (2) temperature optima for different aerobic methanotrophs, and (3) different metabolic functions of different communities. Was there a reason the genetics were not performed? At the very least, why not investigate the fatty acid content of these experiments to see if there is indeed a shift in functioning and/or community. I feel that the manuscript would be greatly improved with methanotroph community and biomarker data. However, if there is a valid reason for the lack of community data or these data will appear in a future paper, and the text is revised to explain this, I believe the paper is publishable with minor revisions, below.

Specific comments: There seems to be a lack of consistent acronym for aerobic methane oxidation in our community. I feel that the acronym chosen here (MOx) is not specific enough to aerobic methane oxidation. Perhaps AMOx could be used instead?

Fig.1 I'm not sure if it's just my pdf version but the figure is incredibly small and it could be more detailed

Page 4 line 16: change determinations to measurements

Page 4 line 26: what is the in situ temperature? Did it change seasonally?

Page 6 line 3: what temperatures exactly?

Page 10 line 32: insert "in the Baltic Sea" between "evidence that MOB" and "are well adapted".

Figure caption for Fig 3: panel (d) is mislabelled (b). Alternatively to 3c and 3d, a table with initial and final O<sub>2</sub> concentrations for both sets of experiments might be more

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informative?

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