Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-345-RC1, 2018

Biogeosciences
Discussions
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Interactive comment on "Controls on zooplankton methane production in the central Baltic Sea" *by* Beate Stawiarski et al.

Anonymous Referee #1

Received and published: 24 August 2018

Comments on 'Controls on zooplankton methane production in the central Baltic Sea' by Stawiarski et al; ms submitted to Biogeosci Discussion; doi: 10.5194/bg-2018-345.

General comments:

The processes which lead to the oceanic methane paradox (i.e. the unexpected accumulation of methane in the oxic surface/subsurface layers of the oceans) are still under debate. Various microbial, biological, and chemical processes have been suggested as possible explanations for the oceanic methane paradox during the last decades. Among these is the suggestion that zooplankton grazing on marine phytoplankton produces significant amounts of CH4. In the ms under review the authors present new results of experiments to decipher the CH4 production by zooplankton in the Baltic Sea. The results presented shed new light on the still enigmatic accumulation of methane in

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oxic marine environments. The manuscript is well written and concise and the majority of the conclusions are justified by the presented data. However, I have a few major concerns (see pts 1), 9)-11) below)and, therefore, I can recommend publication only after major revisions.

Specific comments:

1) Introduction: The introduction looks more like a review. A lot of details are given; however, they do not help at all to place the new data/results in a broader context. I suggest to shorten the introduction and to focus it on the main points which are relevant for the discussion of the results.

2) Introduction: The photochemical source of CH4 is not mentioned. This source might be important in the Baltic Sea in view of the fact that the surface layers of the Baltic Sea are heavily influenced by rivers and rich in CDOM, see [Zhang and Xie, 2015].

3) Introduction: DMSO can be a precursor for CH4 as well, see e.g. [Zindler et al., 2013]. This source should be mentioned.

4) Page 3, lines 5/6: The statement that ocean and lakes contribute about 20% to the global natural CH4 emissions is strongly misleading. The Baltic Sea is not a lake, thus, its emissions contribute to the oceanic/coastal CH4 emissions which, in turn, contribute only 1% or less to the global CH4 sources. Oceanic emissions are, therefore, a minor source of atmospheric CH4. In the latest IPCC Report (which is also cited in the manuscript) oceanic emissions are not mentioned as a separate source because of their minor role for the CH4 budget. Please rephrase this statement.

5) P3L14-20: The role of methylphosphonate (MPn) as source of CH4: There are contradicting results: Valle and Karl [del Valle and Karl, 2014] showed that CH4 is not produced from dissolved MPn but rather from particulate/particle-bound MPn (pMPn); which seems to be in contrast to the results by Repeta et al., 2016.

6) P9L30: ‘ . . . since it represents.’ I think a part of the sentence is missing.

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7) P12L7: Please remove ‘sea/air exchange’ from the subtitle. The air/sea exchange of CH4 (i.e. CH4 fluxes) is not presented or discussed at all.

8) P12L9/10: please present an equation (incl. a ref for the solubility of CH4) for the calculation of the CH4 saturations. (the equation should be placed to the Method section).

9) P15L5-7: In view of the large standard deviations, the given averages do not seem to be significantly different. Therefore, the conclusion of a composition-depending CH4 production is not justified. Please provide the results of a statistical test which shows that the two averages are significant indeed.

10) P16L4/5: ‘. . . may have reflected a response of the animals to stress of being removed from their natural environment’. This statement questions all of the presented results. When the zooplankton is that much stressed, how did the authors make sure to get reliable/representative results?

11) P17, discussion. I agree with the line of arguments for DMSP as a potential CH4 precursor. However, there might have been other particle-bound potential CH4 precursors around, e.g. DMSO and MPn. As said above, del Valle and Karl (2014) showed that pMPn is resulting in CH4 production. So, I wonder whether the discussion is only considering DMSP.

12) P17L19: The authors ‘believe’? We are scientists, that means we discuss results on the basis of arguments. Please rephrase.

13) P24L23-27: This part of the text should be moved to the Conclusion section.

References:

del Valle, D. A., and D. M. Karl (2014), Aerobic production of methane from dissolved water-column methylphosphonate and sinking particles in the North Pacific Subtropical Gyre, Aquatic Microbial Ecology, 73(2), 93-105.

Zhang, Y., and H. Xie (2015), Photomineralization and photomethanification of dissolved organic matter in Saguenay River surface water, Biogeosciences, 12(22), 6823-6836.

Zindler, C., A. Bracher, C. A. Marandino, B. Taylor, E. Torrecilla, A. Kock, and H. W. Bange (2013), Sulphur compounds, methane and phytoplankton: Interactions along a north-south transit in the western Pacific Ocean Biogeosciences, 10, 3297–3311.

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