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Biogeosciences Discussions

Interactive comment on "Dissolved methane during hypoxic events at the Boknis Eck Time Series Station (Eckernförde Bay, SW Baltic Sea)" *by* H. W. Bange et al.

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Bange et al., "Dissolved CH4 during hypoxic events at the Boknis Eck time series station", BGD, 6, 11463-11477, 2009.

We are grateful for the referees' comments which helped to improve the ms considerably.

Reply to referee #1 "1. Some additional information on sampling strategy would be welcome. For example, how often is the sampling? When were the samples collected at each month? How long was the water samples stored before analysis? "

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We modified the text accordingly (see section Methods). We do not see a need to list the dates of sampling. Instead, we added a sentence stating that all data (that includes sampling dates) are archived in MEMENTO (The Marine Methane and Nitrous Oxide Database) and they are available upon request from the corresponding author.

"2.Page 11470, lines 1-4: The author draw a conclusion that additional source or sink terms such as advection or aerobic CH4 oxidation in the water column seem to be negligible at BE. However, there is insufficient detail on the source or sink strength of advection or aerobic CH4 oxidation in the water column, it seems unreasonable to draw such a conclusion only based on the balance between CH4 release from the sediments and the CH4 release to atmosphere."

We accept this argument and, thus, this conclusion has been omitted from the Abstract, Results & Discussion and Summary sections.

"3.Since the CH4 in the water column of the studied region mainly come from in situ methanogenesis in the sediments, I would like to suggest the authors pay more attention to the seasonal variation of sedimentary CH4 release as well as its oxidative loss in future research."

Thanks for making this good point. We are currently looking for future cooperation to address this question.

Reply to Laura Farias (referee #2) "On the other hand, given the high methane levels reported for the water column in this and a previous study, the Baltic Sea seems to be acting as a huge source of methane towards the atmosphere. It could be important to take this ecosystem into consideration when making the global oceanic budget."

Indeed, the Baltic Sea is a strong source for CH4 to the atmosphere. However, CH4 emissions from the Baltic Sea are not significant on a global scale because of the comparably small surface area the Baltic Sea. Nevertheless, we modified the ms and compare our results from BE with other data from the coastal and open Baltic Sea (see

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Results and Discussion). Moreover we added a new Figure to illustrate the pulse of CH4 emissions after ventilation of the water column.

"1.- My first concern is that sweeping conclusions regarding the balance between the CH4 efflux across the air-sea interface and the CH4 flux across the sediment-water interface need to include measurements of both at the same time. The authors are assuming that neither methane production in the water column (see theory of methane production) nor even any aerobic methane oxidation could be modulating the methane efflux in the redox gradient."

We accept this argument and, thus, this conclusion has been omitted from the Abstract, Results & Discussion and Summary sections.

"2.- A second concern is about the conclusion that the sedimentary release of CH4 seemed to be mainly triggered by sedimenting organic material from phytoplankton blooms. However, I observed hypoxia (see Figure 2) immediately after the phytoplankton bloom, whereas the methane distribution was bimodal over an annual cycle, with one peak coinciding with the phytoplankton bloom and another following the period of hypoxia. How do you explain this temporal pattern?"

In order to clarify the justification of our conclusions we modified the Study Site Description by adding a short description of the phytoplankton blooms and the associated sedimentation events at BE. Moreover, we modified Fig.2 by indicating the different phytoplankton blooms. The discussion of the bimodal pattern has been modified and partly rewritten in order clarify our argumentation: We still think, indeed, that CH4 formation by methanogenesis is primarily linked to the bloom-triggered sedimentation events. Hypoxia 'only' enhance the sedimentary CH4 formation and its subsequent release to the water column.

"3.- I think that the criterion used to define hypoxia (2 mL L-1) is not appropriate for a biogeochemical analysis (as the results represent in this paper); perhaps it corresponds to a physiological criterion. Please consider a more biogeochemical concept

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such as that used by Wajil Naqvi, which considers the distribution of N-species and other variables. Additional information such as nutrients, salinity, and other gas tracers could be provided to further validate findings regarding the triggering of methanogenesis in the sediment by organic matter production (observed as Chl-a). You will also need to reinterpret the results, but at present, the hypoxia criterion used herein is a seemingly fatal flaw of the paper."

We disagree because: (i) We think that Wajih Naqvi's classification (see Table 1 in Naqvi et al., Biogeosci. Discuss., 6, 9455-9523, 2009) is not applicable here because it completely fails to identify hypoxic or anoxic events at Boknis Eck (BE): At BE NO2- and NO3- are present even under hypoxic and anoxic conditions. Therefore, we decided to apply the less restrictive criterion given by Diaz and Rosenberg (2008). A discussion why Wajih Naqvi's classification fails is beyond the scope of the article. (ii) No further quantification/calculation is based on the applied criterion, it only serves to identify periods with low [O2]. (iii) We think that there is no need for 'additional information such as nutrients, salinity, and other gas tracers' to justify our arguments/conclusions and, therefore, a discussion of these data is beyond the scope of the article. A detailed discussion of these data will be published elsewhere.

"Minor observations The resolution of Figure 2 is not good. The lowest value (50 uM O2) on the oxygen scale (y axis) is very high. On the x-axis, the months and years are not clear. Include a line to separate each study year."

We agree that the contouring of the O2 concentrations in the central panel of Fig. 2 is rather coarse; however, for clarity of the Figure, we did not choose a higher resolution. We modified the figure caption explaining the date format on the x-axis. We tried to add vertical separators in order to indicate the study years. It turned out, however, that the clarity and readability of the figures were reduced. Thus, we feel that vertical separators are not helpful.

"The relationship between methane and light penetration is very indirect (in fact, it depends on particle concentrations in the water column). This relationship does not contribute to the paper; please remove it; see also figures 5 (b) and 4 (lines of Secchi disc) in the Conclusion."

We disagree. As stated correctly, Secchi depths give us an indirect clue about particle fluxes, thus providing the only available hint for the link of phytoplankton blooms and organic mater deposition to the sediments which is a prerequisite for sedimentary methanogenesis. Therefore, we think that the Secchi depth data presented are necessary because they support our conclusions.

"Different units (mL L and uM) are used to express oxygen levels in the manuscript."

This statement is not correct.

"Choose only one [O2 level unit] and use it consistently."

O2 concentrations are given only once in ml L-1 (given in parenthesis in the introduction) to facilitate comparison with other publications. We do not want change this.

"Please rewrite the Abstract with respect to a strong conclusion (see above) and include data on methane fluxes across the air-sea interface."

The abstract has been modified and now includes general statements on CH4 saturations and emissions. However, we do not see a need to explicitly add CH4 flux data because they are not the main topic of the article.

Interactive comment on Biogeosciences Discuss., 6, 11463, 2009.