Biogeosciences Discuss., 6, C4050–C4052, 2010
www.biogeosciences-discuss.net/6/C4050/2010/

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.

L. Farias (Referee)

lfarias@profc.udec.cl

Received and published: 19 January 2010

The paper by Hermman Bange and co-workers presents the results for dissolved methane and other variables measured at the BE time-series station over two and a half years (monthly resolution). The data are solid and they are presented as a short and very concise paper. As the authors mentioned, very few time-series studies include CH4 among their measured variables, making this data highly valuable in terms of long-term comparisons. 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. I have concerns with respect to some conclusions. Thus, I would recommend publication after revision. 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. 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?

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 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.

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. 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

BGD

6, C4050–C4052, 2010

5 (b) and 4 (lines of Secchi disc) in the Conclusion. Different units (mL L and uM) are used to express oxygen levels in the manuscript. Choose only one and use it consistently. Please rewrite the Abstract with respect to a strong conclusion (see above) and include data on methane fluxes across the air-sea interface.

Laura Farias

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