Biogeosciences Discuss., 4, S1975–S1977, 2007
www.biogeosciences-discuss.net/4/S1975/2007/

Biogeosciences Discussions

Interactive comment on "Dynamics of dimethylsulphoniopropionate and dimethylsulphide under different CO_2 concentrations during a mesocosm experiment" *by* M. Vogt et al.

Anonymous Referee #1

Received and published: 22 November 2007

General comments: The paper deals with one of the hot topics in the climate change discussion: the response of the marine biota to changed pH as a consequence of elevated CO2 levels. Ocean acidification might have an impact on a whole suite of biochemical processes. This paper describes changes in compounds of the marine sulfur cycle including pH induced perturbations in the concentrations of the climate-relevant organic gas DMS. Overall this is an interesting piece of work fitting well within the scope of BG. But I hope the experiments have been performed with greater accuracy than the writing of this paper. Labels and captions of several figures are wrong.

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Interactive Discussion
Discussion Paper

Even the numbers of the pCO2 level of the different treatments vary throughout the text. Some results are presented without any further explanation. Contradictory statements to other publications are made, but not further discussed. This paper needs a careful major revision; then I would recommend it for publication.

Specific comments:

page 3682 I am not an expert on statistics, but it seems insufficient to simply write down a name of the statistical method and some of the resulting values without explanation. Either more general information on the chosen analysis has to be provided or remarks should be reduced to the statistically significance of the tested property. I found it very confusing that statistical quantities like F, df or sigma are given without further explanation. Especially, using letter "F" in the current context where experiment names like P, F, and FF are used.

same page After reading Wingenter et al 2007 I puzzled about some statements being made here. The presented DMS data set is taken basically from the same experimental setup (different sampling time and different lab) as that discussed in Wingenter et al. The time series of DMS for the 3 treatments look very similar. Integration by eye of your Fig.1d gives the impression that more DMS is produce in F than in FF. This would be in accordance with a statement by Wingenter et al(p3) but not with your statement in line 23-25 on p 3682. This must be clarified.

page 3685 Does fig. 4 really shows the ratios of the mean as written in the caption? Is this a useful quantity or should one rather look at the mean of the ratios?

page 3886-3887 Please carefully revise and straighten out the section Discussion; and eliminate repetitions, especially in the introduction part and part 4.1.

page 3691 I had a hard time trying to understand the meaning of the last sentence of the section Summary and Conclusion. Can we really derive from the knowledge about changes in DMS under elevated pCO2 anything that concerns the effects of ocean

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acidification on the marine ecosystem? Or should it be the reverse - we have to look at the ecosystem itself to understand what might change DMS? How can the understanding of pH induced DMS changes give information on "the feedback of climate change on DMS"? Please clarify these statements.

p3696 ff Please correct figure captions:

fig. 1: the given pCO2 values differ from text P= 350 ppmV, F= 700 ppmV, FF=1050 ppmV according to p 3678

fig. 2: selected bags must be 2 (FF), 5(F) and 8(P). Only for a)-c) you show the range of data, not for d)

fig. 4: please correct units on y-axis of graph b) and c)

technical comments:

The labeling in all figures is rather small.

Interactive comment on Biogeosciences Discuss., 4, 3673, 2007.