Biogeosciences Discuss., 4, S2120–S2123, 2007
www.biogeosciences-discuss.net/4/S2120/2007/

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 #3

Received and published: 10 December 2007

What happens to the production of climate active gases in the surface oceans in the face of increasing ocean acidification is a highly topical subject and certainly of relevance to this journal. This manuscript has two clearly stated goals: 1. to investigate differences in DMS dynamics under elevated $CO_{2;}$ and 2. to address the factors that may cause the altered DMS dynamics. The study makes a careful and thorough use of the mesocosm experiment PeECE as a basis to achieve the first of these aims but explanation of the causes of the altered DMS dynamics are confined to a rather speculative Discussion, rather than any informative process measurements or definitive

correlations. The manuscript is well written and results well presented but the study rather unambitious in my view. The principal observation is a more rapid decline from the peak in DMS concentrations under a Present CO_2 environment compared to Future and Far Future treatments; despite consistent trends in DMSPt and DMSP-lyase in all three treatments and apparently similar phytoplankton composition and biomass.

Does the paper help us to predict what may happen to DMS production in a more acidic ocean? I don't think so, and in fairness the authors themselves clearly state that the implications of their findings for a future global ocean and climate is unclear. What would be more informative, is greater emphasis on comparison of these results with reports from similar experiments (Avgoustidi et al. 2007, particularly as the two studies share three of the co-authors) and the relevance of the in-water observations to atmospheric measurements of DMS made during the same experiment that found a significant difference between treatments (Wingenter et al., 2007, Sinha et al., 2007). In their conclusion, the authors make a fair and honest critique of the short comings of their mesocosm study and suggest alternative approaches and the need for specific process measurements. I certainly agree with the need to understand the functional role(s) of DMSP (and DMS) if we are going to predict how DMSP and DMS production and consumption by the planktonic microbial community is going to change as their environment alters. One additional factor, often stated but seldom addressed, is our need to understand how microbes may be able to adapt their physiology to changing pH in the oceans. This will also only be achievable if we understand the physiological relevance and value to these cells of DMSP and its breakdown products.

In conclusion, the manuscript is acceptable for publication but only after revisions that make a more thorough attempt to address the causes of the differences between Present and Future treatments. This should include reducing the extent of the speculative Discussion. In addition the revisions need to relate the measurements reported in this study to the previous similar study of Avgoustidi et al., 2007 and to the air measurements of DMS made during PeECE.

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Specific points:

1. P3682. L 19. The two-way ANOVA illustrates a difference exists within the three treatments but I think a more specific test is required to illustrate that the real difference is between in the Present treatment whilst the Future and Far Future trends in DMS were similar.

2. P3682. L 23. These are time integrated averages of DMS concentration, not production. Without appropriate rate measurements you cannot determine levels of production.

P3683. Little effort is made to relate DMSP-lyase activity (DLA) to other parameters. The reason given for measuring DLA in the 3 bags shown was because 'most other measured parameters from collaborating groups were available for these bags' but no use is made of this data. In the Discussion P3689 L8, the authors 'speculate that a significant part of the measured DLA . . . is due to coccolithophorid or other planktonic DMSP'. This is weak; does DLA significantly correlate with chl a or *E. hux* abundance, with DMSPt or even with DMS/ DMSPt? Does presenting DLA activity help tell us anything about the steep decline in DMS concentration in the Present bag? More use needs to be made of this data if it is to be included.

3. P3685. L7. Figure 4 illustrates the temporal change in ratios of DMS to DMSPt, DMSPt to chl a and DMS to chl a. 'Monotonously increasing/decreasing part of the graphs' do not imply temporal correlation, i.e. high Spearman values. Firstly, Spearman's rank correlation does just that, compare the ranks of each data set, nothing more. Increasing or decreasing trends do the opposite, illustrate anti-correlation, and a tendency towards low Spearman's values.

4. P3685 L25. The authors hint that the possible difference between treatments is because of the relative abundances and temporal trends in lithed *E. huxleyi*.cells: P3687 L17, P3689 L5; So, why not illustrate the timing of DMS peaks and *E. huxleyi* abundance that we are told differed between Present and the two Future treatments? My

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rough calculations suggest that if lithed *E. huxleyi* was the main contributor to DMSP production then the cell quota would be something approaching 24 pg DMSP cell^{-1} at the height of the DMSPt peaks; considerably higher than measurements made in laboratory cultures (normally around 0.5 pg DMSP cell^{-1}) and suggesting other components of the phytoplankton may have been more important. The same goes for DLA, if a cell-specific rate is calculated for *E. huxleyi* then how does this compare to previous laboratory and natural populations and can the DLA really be attributed to *E. huxleyi*, P3689 L5, during the peaks in DMS?

5. P3688 L17+. If the occurrence of a distinct viral population between days 15 – 22 explains the decline in phytoplankton between days 10 – 16, would this not cause an increase in DMS during this period? Instead DMS declines rapidly from day 10.

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