

Interactive comment on “Effect of ocean acidification and elevated $f\text{CO}_2$ on trace gas production by a Baltic Sea summer phytoplankton community” by A.L. Webb et al.

Anonymous Referee #2

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General comment

This paper presents data from an acidification experiments conducted in large mesocosms in the Baltic Sea during the 2012 summer. The mesocosms system used here has been described in the past and used in previous successful ocean acidification experiments. This is considered as the state-of-the-art system for that type of experiments. As usual in multidisciplinary experiments, many different papers were produced, some of which are already published. This particular paper focuses on the impact of acidification on the production of biogenic trace gases (dimethylsulfide and a suite of halocarbons), but makes several references to other papers related to the same study.

C1

Few general remarks:

1. The upwelling event that took place in the middle of the experiment (t16) certainly confused the issue by cooling the water of the mesocosms. For that reason, the changes in biogenic gases concentrations observed after this event result from both the cooling and the acidification of the water. This is recognized by the authors and properly discussed in this version of the paper.
2. Measurements made outside the mesocosms are interesting by themselves, and as they are in this version of the paper, should not be compared with the results from the mesocosms where the upwelling event only translated into a decrease in temperature, but no change in salinity and more importantly no change in plankton composition. These are two independent stories which need to be treated as such. In that regard, in situ data could be presented in a separate figure to emphasize this point. A reason to do so is that the Phases indicated in figures 1 and 2 are not relevant to the in situ measurements. This would also allow to rescale the Y-axis of figure 1c and 2a and make the changes in chl-a and DMS concentrations in the mesocosms more visible.
3. The lack of detectable DMSP concentrations is obviously surprising. Although the authors offer possible solutions to this conundrum, the fact remains that they are able to detect a by-product of DMSP degradation but not DMSP itself, known to be, in many circumstances, orders of magnitude higher than DMS. It is difficult to believe that 30 days worth of samples within a diverse community of phytoplankton did not generate a single detectable nmol of DMSP. Some loss can be explained through the presence of acid-sensitive species (colonial *Phaeocystis* etc.), but the authors rule this out themselves as an important process by specifying that this type of phytoplankton accounted for less than 10% of the community. In fact cryptophytes and chlorophytes dominated the community. Various species of these two groups are known to produce DMSP (Keller et al 1989) but not known to be sensitive to the acid treatment. As stated by the authors, a methodological problem can probably explain these results.

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Specific comments

P1, 25: ...challenged Baltic Sea.

P2, 55: ...the global ocean has absorbed... P2,41: Would it be possible to come up with a 'dilution' factor? Using salinity as a conservative parameter perhaps? This would allow to roughly estimate how much of the variability of the parameters measured at the surface needs to be explained by other factors (production/consumption).

P4, 110: Suggestion: replace 'Post-spring bloom' by 'Following the spring bloom'.

P4, 114: ...2012 summer post-bloom season...

P5, 132: ...such as fish...The removal of large zooplankton is probably more relevant here than fish.

P6, 163: ...with 100% absorbance of UV light...Later in the manuscript, it is mentioned that some UV light could affect the processes taking place close to the surface in the mesocosms. This seems to be in contradiction that 100% UV is removed.

P8, 230: ...turnover of DMSPD...Replace by 'dissolved DMSP'.

P8, 246: Measurements of carbonate chemistry and community dynamics.

P10, 281: ...decreased over Phase 1 in the ...The phase numbers are not properly aligned in figure 1c (on my printed copy at least), and absent in figure 2, 3 and 4 (which are by the way wrongly numbered).

P10, 287: ...no variation with depth (data not shown)...

P10, 297: ...a significant effect on phytoplankton growth (and biogases production), explaining...

P11, 324: ...that light availability and surface water temperatures...Delete 'environmental conditions of limited' and 'lower'.

P11, 330: A significant 34% reduction...These results could be better explained taking

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into account the temporal variability which is significant. Actually, DMS concentrations increased as Chl concentrations decreased, and the increase in DMS was less important at high PCO₂. After day 21, DMS decreased gradually in all treatments until the end of the experiment.

P11, 333: (Fig. 3a) to be replaced by (Fig. 2a).

P11, 336: (Fig. 3b) to be replaced by (Fig. 2b).

P11, 337: Furthermore, increases in DMS...were delayed by three days...This 3-day delay is not obvious in Fig. 2a. Am I missing something?

P12, 348: Although the majority...This paragraph needs an introduction sentence. As in my previous review of this paper, I still think that there is too much emphasis on a rare pathway of DMS production considering that the problem is most probably a methodological one. This paragraph is important but could be shortened.

P12, 358: Correlations between...Only one P value is presented. Should it be 'correlation' instead of 'correlations'? I am also wondering if all the data were pooled (all treatments) to compute this statistic.

P12, 373: The peak in DMS concentrations is unlikely to be a delayed response... But the increase in DMS coincided with the decline in Chl-a concentrations (t15-t21), something frequently observed in nature in response to higher DOC production and bacterial activity during bloom decline. My point here is that the results should be presented and discussed in term of temporal changes, not only correlations.

P13, 379: ...2009). DMS and DMSP...

P13, 398: This is relevant...I don't understand the logic here. In the absence of DMSP values, whatever the reason, I don't think that one can conclude that 'DMS concentrations were likely more affected by the change in Δ PCO₂ than the production of the precursors'.

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P13, 405: ...and therefore lower DMS microbial yield from DMSP and/or greater consumption of DMS and conversion to DMSO. ... DMS yields may vary from 5 to 40% depending on the S and C demand of the bacteria and the quality of DOM. There are many references on variations in DMS yields. A good starting point is the paper by Kiene and Linn 2000 (Distribution and turnover of dissolved DMSP and its relationship with bacterial production and dimethylsulfide in the Gulf of Mexico. *Limnol Oceanogr* 45: 849-861).

P15, 441: ...where some UV light was able to pass... This seems to be in contradiction with the statement that 100% of UV radiation was absorbed by the cover (P6, 163). This requires clarification.

P15, 455: The peak of CH₂I₂ coincided with the decline of the bloom, as observed for DMS. I am not convinced that the positive correlations observed between these compounds and the abundance of the different taxa are relevant if the production of the compounds is related to processes linked to the decline of the bloom (ex. increase in DOC).

P15, 466: The cleaning of the walls of the mesocosms and the associated apparent released of DOM as mentioned here seem to be an important potential artifact. As noted, this could be very important for photochemically and microbially driven processes. This potential problem, which could also be important for DMS production, should be discussed in more details in this paper. Would it be useful to indicate on the different figures when these cleanings took place? Overall, providing more details on the impact of these cleaning events would be of great value for colleagues planning to conduct similar long term mesocosms experiments.

P16, 490: ... indicators of algal biomass. PP was not measured here.

P17/177, 503/504: ...low net increase in total Chl-a...

P18, 550: Typo: Two dots before 'but peaked'.

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P18, 558: As the CO₂ levels increased during Phase II... As mentioned by the authors at the beginning of this section, comparing the mesocosms results with the in situ ones is inappropriate. The different Phases (0, I, II) make only sense for the mesocosms experiment where they indicate either treatments or events. They are irrelevant to the in situ measurements. Keeping this comparison is confusing.

P18, 562: ...this decrease in DMS may also be attributed to CO₂ levels...

P19, 577: ...that production was probably not limited...

P19, 598: ...living and acclimated to...

P20, 603-607: These two sentences would benefit from a rewording.

P20, 615: For the concentrations of halocarbons, ...of the Baltic Sea. I am not sure about this conclusion. This is very speculative since deep water upwelling and ocean acidification through air-sea CO₂ exchange are two different processes. Upwelling brings nutrients, microbes, etc. ... in surface water in addition to high CO₂.

P 35. This should be Figure 2 (instead of 3).

P 36. This should be Figure 3 (instead of 4).

P 37: This should be Figure 4 (instead of 5).

END

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