

Review of “Effect of ocean acidification and elevated fCO₂ on trace gas production by a Baltic Sea summer phytoplankton community”
Author(s): A.L. Webb et al.
MS No.: bg-2015-573

1-An initial paragraph or section evaluating the overall quality of the discussion paper ("general comments"),

The manuscript quality has improved since the last review and the reviewer comments were addressed adequately. The manuscript is much more concise and less speculative.

The results show a lack of response of gas concentrations to the experimental design and no linkage to the external conditions due to the outside undergoing its own “experiment”, i.e., upwelling.

No rates were measured, which has been clarified, concentrations (mostly) remained the same, and the need to have rates of production, consumption, (photo/chem)-degradation or even “net” rates to include in prognostic and predictive models has been emphasized more.

I again recommend publishing pending changes. Most of the (many-sorry!) small corrections (see below) are easily done. For me personally it would be important to have a clearer number agreement for the mean CO₂ values (see comment on Line 29). Also I think the firm and clearly stated replies to the editor comments under Point 1 and 2 would strengthen the discussion, as they nicely address the criticism that could still emerge. It could be worth to have a (short, taken from the comments) discussion section on methodological caveats/challenges.

2- section addressing individual scientific questions/issues ("specific comments"),

Unchanged: The manuscript addresses the influence of ocean acidification on the production of dimethylsulfide (DMS) and 7 halocarbons in a Baltic Sea mesocosm experiment.

The authors effectively found no differences in DMS and halocarbon concentrations over time among the various fCO₂ treatments; and no obvious relationship to any other environmental (biological or chemical) variable measured. Difficult to explain without knowing whether turnover is fast. The authors found a decrease of DMS concentrations for highest fCO₂ treatments vs. controls only in the last phase (when Chl-a declined) and none of the other detected differences in halocarbons were CO₂ related.

The outcome of this study is a relevant piece of information, indicating that most likely there will be no major changes to halocarbon concentrations in the Baltic Sea anytime soon, and the authors conclude that this might be due to the already well adapted community in the unstable Baltic Sea environment with regards to S, T, CO₂ and many other factors. The results are interesting by themselves, and valuable for modelers, though modelers need rates.

The DMS results again confirm results from a range of mesocosm studies.

3- compact listing of purely technical corrections at the very end ("technical corrections": typing errors, etc.).

Comment upfront: I didn't actively look for any typing errors et cetera at this state of the manuscript, these are just the things that “jumped at me”.

Line 25: additional stressor facing the pelagic community of the already challenging Baltic Sea. Doesn't the community have to face the stressor? I tried to find this formulation anywhere else, but failed.

Line 29: I am confused by the mean CO₂ concentrations given. Here it is 350 μ atm (which I would read as a rounded value if the highest wasn't given as 1333, so as the correct number). In Table 1 the two means are 331 and 334 (= ~ 332-333 if combined). In Figs. 1, 2, 3, 4 and S1, S2 they are 346 and 348 (= ~ 347 if mean of both). In S3 the mean is 346.

Line 104: separates CO₂-rich, bottom waters from fresher, lower... , after CO₂-rich?

Line 49-50: “however emissions of biogenic sulphur could significantly decrease from this region” sounds strange to me – (no native speaker, though). Shouldn't it read: however emissions of biogenic sulphur from this region could significantly decrease. Nitpicking.

Line 111-112: are largely unstudied in 112 terms of their community trace gas production during the **summer bloom**. You introduce a knowledge gap during the bloom (which you expected to happen) and then (line 117-118) state that you “report the concentrations of DMS, DMSP and halocarbons from the 2012 summer **post-bloom season** mesocosm experiment”. I think you should match these two.

Line 113-114: “a low dissolved inorganic nitrogen (DIN) to dissolved inorganic phosphorous 114 (DIP) ratio combines with high temperatures and light intensities to encourage” I get what you’re saying, but I am fairly sure they don’t combine TO encourage, but “combined with... **encourages**” or “, encouraging” instead of “to encourage”?

Line 184: below 10°C in the dark: you have a space in between everywhere else.

Line 216: 7 mL sample vial, really? Or 8 mL as the one before from Labhut?

Line 219: purged with 1 mL of 10M NaOH for 5 minutes at 80 mL. Pretty sure you didn’t purge WITH NaOH, but you purged after NaOH addition with ??? right? OFN maybe?

Line 262: in 90 % acetone with: NO space before % or everywhere else

Line 274-276: In analysis of the measurements **between mesocosms**, one-way ANOVA was used with Tukey’s post-hoc analysis test to determine the effect of different *f*CO₂ on concentrations measured **in the mesocosms and the Baltic Sea** – with or without the Baltic?

Line 317-318: this decrease was attributed to a temperature induced decreased in

Line 363: 7).Recent studies: Space before Region.

Line 383-384: “**neither** the cryptophytes **or** chlorophytes as the largest contributors of Chl-a **were** identified as significant producers of DMSP.” Neither...nor, and during this study? Or **have been** identified – before and general?

Line 392-395: “The variation 393 in inorganic nutrient concentrations between mesocosms at the start of the experiment did not have 394 an effect on DMS concentrations during Phase I, and by the start of Phase II the variation between 395 mesocosms had decreased.” If they started at varying levels and then were more similar, doesn’t that indicate a different usage in the different mesocosms? And wouldn’t that imply a different effect on the respective communities then? Impact on later development?

Line 462: “surface **10m** of the” Space please.

Line 487: “However, given the **lack of response** of these compounds to elevated *f*CO₂ (F=1.7, **p<0.01**), it” p<0.01 = lack of response? Confused.

Line 494-496: “Hughes et al. (2008) did not identify this route as a pathway for 495 CH₂I₂ or CH₂Cl₂ production, but Carpenter et al. (2005) suggested a production pathway for these 496 compounds through the reaction of HOI with aggregated organic materials.” This sentence seems a little lost here. Maybe a concluding sentence would help, even if it is (again) just a statement that you (unfortunately) can’t solve this due to a lack of measurements on these routes/compounds?

Line 508-509: “**decreased** steadily in all mesocosms from t-3 through to t31, **over the range** 4.0 to 7.7 pmol L⁻¹”. It decreased over a range sounds a little awkward to me.

Line 519-524: “Production of all three bromocarbons was identified from large-size cyanobacteria such as Aphanizomenon flos-aquae by Karlsson et al. (2008), and in addition, significant correlations were found in the Arabian Sea between the abundance of the cyanobacterium Trichodesmium and several bromocarbons (Roy et al., 2011), and the low abundance of such bacteria in the mesocosms would explain the low

variation in bromocarbon concentrations through the experiment.” That is one long sentence. Why not split it after (Roy et al., 2011). **The** low abundance...

Line 526: “suggested **as** of greater importance than” Again, I am no native speaker- this sounds weird to me: “suggested **to be** of greater importance **than**” or “suggested **to be** more important **than**”?

Line 528: “growth rates and low net increase in total Chl-a” during the experiment described herein or the like?

Line 539: “a greater concentrations gradient” change to “a greater concentration gradient”

Line 530: “bacterial breakdown; **which** could explain” comma or this

Line 555: the **associated** pH, as well as having communities **associated** with the

Line 576: L⁻¹but peaked: Space.

Line 578: I still couldn’t find a reference to Fig S3, here seems about right? “ratio of DMS: Chl-a at 1.6 (± 0.3) nmol μg⁻¹ (**Fig. S3**)”

Line 583: after t17the DMS. The last time I write the word space...

Line 586: “DMS deep water” Period please.

Line 600: “with maximum concentrations 191.6 pmol L⁻¹, 10.0 pmol L⁻¹ and 5.0 pmol L⁻¹ respectively” with maximum concentrations **of** 191.6 pmol L⁻¹, 10.0 pmol L⁻¹ and 5.0 pmol L⁻¹, respectively

Line 647: mesocosm, field, and laboratory

Tables:

Table 1: All CO₂ means given here differ from the ones given throughout the rest of the manuscript. I am guessing that this might be due to the inclusion of the t- days? If so, what is the rationale in having the days before CO₂ addition (t0) in the averages for the CO₂ treatments? I would start with t0. Again, I personally find it very irritating if I find different numbers used. And: The Baltic had a target (?) but the controls not (=Baltic?)

Table 3: I like the information given here, however I do not like the table very much. It has a lot of empty space. And Chl a has 100% contribution to Chl a? ☺ I know this kind of table is hard to make nice, but maybe you could restructure it... Or at least fill some of that space by indicating ND for the taxonomy in the 10 m integrated samples? Use some abbreviations to get narrower columns? And isn’t it still mesocosms?

Table 4: Again, I like the Information- and in this case also the table. Trying to do any comparisons from these ranges in controls and highest fCO₂ treatments is really hard, though. Hmm, It would be great to have sort of a standard case, like 750 vs. control or the like, but I see that it would be a lot of work/calculations given all the compounds and experiments. Not insisting on you changing it, just saying.

Figures:

Fig. 1: Missing: fCO₂ shown in the legend are mean fCO₂ across the duration of the experiment (you have it in all other legends).

Supplement Figures:

Fig. S1: “mesocosms **Dashed**” Add period

Fig. S3: “Paul *et al.* (2015) **”** Add period.