

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.

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

The manuscript is well structured and for the most parts easily readable. 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 are reported; not clear any were measured. Hence, the entire manuscript must be clarified that the values represent “net” values and not production, nor consumption or degradation, nor emission.

Hence, I strongly suggest removing most comments from the discussion that pertain to “climate change” and simply state that concentrations remained the same regardless and emphasize that we (and especially modelers) need to have rates of production, consumption, (photo/chem)-degradation or even “net” rates to include in our prognostic and predictive models.

I recommend publishing pending changes. I also suggest shortening some of the longish speculative paragraphs; it’s hard to explain why there is no apparent change! In general, I think the manuscript would profit if it was a bit more structured based on hypotheses, rather than being purely descriptive. You must have had some expectations when the experiment was started (and the proposal written!), especially since you had results from previous mesocosm experiments. Especially since no (?) rates were measured.

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

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

Line 247, 248: Inconsistent placing of units, 10m (no space) but 486 nm (space), e.g. line 247, 248 vs 261. You might wanna check if there are more.

Line 70: “Both DMS and DMSP are major routes of sulphur and carbon flux through the marine microbial food web”. I wouldn’t call them a route, they could be called transporters, or they provide the basis for major routes. Or DMS and DMSP based metabolic pathways are the route...

Line 72: Where do they state that in this reference? I think Simo *et al.*, 2009 should be the reference for phytoplanktonic demand (pages 50-51 e.g.), and Vila Costa *et al.*, 2006a for bacteria (page 653)? Or you put them in as combined references for both (after sulphur demand)

Line 142: That was the standard deviation at the beginning? ~50%, ~7%, and ~75%? That's a lot to start with... Any thoughts on how that could potentially affect the outcome of the experiments on the bacterial metabolism side of halocarbon and DMS production?

Line 169: replace ; with a ,

Line 171: why no comparable pigment analysis, what's the rationale behind it?

Line 179: Is that shown anywhere? Otherwise please state what the precision was, and that it is not further shown.

Line 237: careful. The del Valle et al samples were DMSPd and DMSPt; not DMSPp. The Kiene group estimates DMSPp by difference between the total and dissolved pool.

Line 247: 17mLWS space needed

Line 248: Chl-a the a is superscripted

Line 300: Mixing of the mesocosms after closure prior to t-3 did not trigger a notable increase in Chl-a in Phase 301 0; in previous mesocosm experiments, mixing redistributed nutrients from the deeper stratified layers 302 throughout the water column

I get what you are saying, but I think you should add what redistributing nutrients did- I am assuming here that it lead to an increase in Chl-a?

Line 282: "mainly through air-sea gas exchange" – isn't that usually considered to be limited by the small surface area / volume ratio? Please comment on why this should not be the same for your analyzed gases.

Line 302: no direct result of the CO₂ additions because there was no significant difference between controls and treatments?

Line 309: chlorophytes (largest contributor to chl a) are not exactly known to be high DMSP or DMS producers; you may want to mention that given stated link to pico and nanoeukaryotes as possible sources. This is why bringing in the Fig, S3 as Fig 3c somewhere actually shows that there are differences among treatments.

Line 311-312: so between the opposing trends for pico I and pico II, the next effect on DMS in the system is zero?

Line 331: Please explain F-test or at least the H₀ you used in one sentence in the methods section.

Line 348-369: Simply there was no relationship between patterns (or lack thereof) in DMS concentrations and any other measured variable. And no rate measurements available. Please say so. Too many possibilities, too many unknowns. This section reads a bit like "filler"; sorry.

Line 354: synthesis should be synthesise

Line 358: Correlations between DMS and the cyanobacterial equivalent Chl-a 359 ($p=0.42$, $p<0.01$) indicate that the methylation pathway may be a potential source of DMS within the 360 Baltic Sea community.

Reference? Data shown anywhere?

Line 367: Stop! What rates of net DMS production? Did you measured or estimate them? If you did, please indicate and discuss!

Line 371: but I thought that Syn does not make DMS?! There never is high DMS concentration reported along with it in subtrop regions (DiTullio et al., others). Didn't Vla-Costa et al. 2006 report uptake of DMSPd (not DMS) by Syn and other picoeukaryotes?

Line 372: Why is it unlikely?
Line 379: just one period.

Line 386: “However, these experiments limit our ability to generalize”... I don’t think it’s the experiments limiting, but rather the varying responses, is that what you are saying?

Line 410-411: no data on consumption, no bacterial rates described, then what is the basis for this statement? Confusing.

Line 412: “Synechococcus has been identified 412 as a DMS consumer in the open ocean“ Reference, please. Syn consumed labelled DMSPd, not DMS (Vila-Costa et al 2006)

Line 431: Sections 3.3 and 3.4: No rates of anything for the halogenated compounds either? Just checking.

Lines 518-522: well, was the region isolated from the coastal environment or not? You can’t have it both ways. I understand that the mesocosm bags were closed so they wouldn’t have a macroalgal component. This will come back in the discussion

Line 548: I agree that the comparison between the mesocosms and the outside is inappropriate. The outside underwent its own and different “experiment”

Line 557: please delete sentence about DMSP as it implies that there was none because none detected when it is an analytical issue

Line 558-569: given the statement in Line 548, please remove this paragraph as it mixes mesocosm conditions with outside conditions. It is pure speculation as a lot more changed with the injection of upwelled water than fCO₂- i.e., particles, nutrients, DOM, etc, etc

Line 576: Is CH₂Cl₂ really polyiodinated?

Line 584: Check your manuscript for Chl-a, the α is alternating between superscript and normal

Line 586-590: It is above indicated that macroalgal beds were not a source. Now, it is implied that those macroalgals beds were close? or far? in location w/r to the mesocosms. And the prevailing circulation was from the beds towards the mesocosms?

And what about vertical input?

The entire DMS section is predicated on upwelling, ie, water injection from below NOT lateral advection. Can't have vertical input for one gas and horizontal input for the other one.

Line 593-607: good

Line 599: I think you want to stress here, that the values are high enough to be considered an already adapted site, rather than stressing that they are lower than elsewhere, correct? “[...] at such a location with a relatively low fCO₂ excursion compared to some sites [...]”, maybe rephrase to “[...] at such a location with a relatively high fCO₂ excursion, however still relatively low when compared to some sites [...]”

Line 609-611: Not all the time, only after the decline of Chl-a, right? I wouldn’t stretch it out, then.

Line 614: production was not measured, only concentrations. Please change production for cycling because the levels measured are a net result

Line 615: since rates were not measured, you don't know whether was a response (ie, prodn and/or cons), only that the measured concentrations did not change

Line 617: no change IF under similar meteorological conditions as during this sampling

Line 617-621: NET production or availability. Again, same issue. Also, rather simplistic as meteorology must be considered.

L621-625: This is a weak concluding paragraph. It says nothing at all. Keep it honest and simple by saying that no changes in concentrations were seen and that next time it would be best to measure rates so these rates can be included in models to have better predictions!! So sorry that you didn't see any changes nor anything "exciting".

Figures in general:

I find it very irritating how the units are given, e.g. $\text{fCO}_2/\mu\text{atm}$. I read the "/" as 'per', which makes it confusing. I would very much prefer if you put $\text{fCO}_2 (\mu\text{atm})$ or $\text{fCO}_2 [\mu\text{atm}]$

Fig 3: The Legend is misleading. It sounds as if you were showing an integration, but you are actually showing the mean from a water sample integrated from the top 10m. "Dashed lines show the Phases of the experiment as given in Fig. 2," should be moved to the a0 part of the legend, as it is not shown in 3b.

Supplement Figures:

Fig. S2: Top left y axis is formatted differently. Also t vs T as abbreviation for time between S1 and S2

There are two Tables 1 in the supplement.

There is a Fig. S3 that is never mentioned in the text which I suggest actually be moved into the main section as Fig 3c as it shows a difference of DMS/chl among mesocosms!