

Interactive comment on “Dimethylsulfide (DMS) production in polar oceans may be resilient to ocean acidification” by Frances E. Hopkins et al.

Anonymous Referee #2

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General comments:

Hopkins et al. present a large dataset on DMS(P) production by phytoplankton in short-term OA experiments from the Arctic, the Southern Ocean and the North Atlantic. This is an interesting and important dataset. I especially acknowledge the importance to publish ‘negative results’, i.e. absence of significant effects of experimental treatments, which is often neglected in OA research but should receive a lot more attention.

I find the hypothesis that then environmental history of organisms will determine their sensitivity to environmental change very convincing. Currently, the data (or its presentation) is not really suited to convincingly convey this message though. This does not mean that the hypothesis should not be mentioned, but it should be clearly marked as a hypothesis rather than a finding.

Furthermore, I would argue that the significant OA effects observed in the two cited coastal communities really question the validity of this hypothesis, as the degree of carbonate chemistry variability is much more pronounced in coastal vs. open ocean compared to temperate vs. polar. Therefore, your conclusions need to be more specific to the current study, and not towards polar systems in general.

One of my general methodological concerns that need to be addressed in the discussion is the fact that especially in short-term experiments, 50% variation in the experiment duration can have a huge impact on the outcome, especially if the phytoplankton initially show a lag phase as often observed in such experiments with natural communities. It makes a huge difference if OA effects are compared after 48h or 4d or 7d. While after 2 days, physiology most likely is not fully acclimated to the treatment conditions yet, 4d or 7d duration most likely show acclimated responses but potentially also reflect shifts in the composition of the communities. Also the differences in the number of hours at T1 and T2 should be accounted for by always referring to the number of hours rather than the time point throughout the manuscript.

It should also be included into the discussion that the significant impacts that Hussherr et al (2017) observed were measured over a much larger pCO₂ range (up to 3000 μ atm).

One major problem with this dataset is that the experimental carbonate chemistry was not well controlled. For example, at the 1000 μ atm pCO₂ level, T2 pCO₂ levels vary between approx. 400 and 1000 μ atm (Table S2). Therefore, the data should be represented using the real carbonate chemistry instead of the assigned values. I understand that this implies replotting and reanalysing most of the data, but currently the levels that are tested against each other are actually not separated when it comes to carbonate chemistry.

In conclusion, I get the impression that the authors really try to tell a story that does not fit their data. I think that the hypothesis (more variable carbonate chemistry causes

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organisms to be less sensitive) presented here makes a lot of sense, but for various reasons the data set is not suited to prove or disprove it.

Specific comments:

Titel and throughout: To my knowledge, the term “resilience” refers to the ability of a system to return to the initial state after disturbance. Therefore, I do not think that the experimental setup and the response pattern (or its absence) in your study allows for statements on resilience. I suggest to use “insensitivity”, “resistance” or something along these lines instead.

L22-27: As you refer to the other studies conducted in the Arctic, you also need to include their results in your statement, or be more specific that you only refer to the presented dataset and not the polar evidence in general.
L24-31: In the discussion, you do not refer to “geographical” or “regional” differences but compare temperate vs. polar systems. I would try to be more consistent here.

Introduction: The introduction is quite long, especially DMS(P) biogeochemistry is described in a lot of detail, even though most of this is not referred to in the discussion. I would suggest to shorten it. If your discussion does not focus at all on biogeochemistry, do you really need all this detail here?

L92-95: This is correct, but one shouldn't forget that it is the coastal areas that are the most productive and therefore important ones. In my opinion you do not even have to somehow restrict the importance of these two previous studies, your study is a valuable contribution even though two other ones exist.

L118: Here and in a few other instances you refer to your incubations as being “identical”, but in the methods you state that the day length was adapted to the respective in situ conditions. Therefore, I would not use the term “identical”.

L119-120: I think the differences in nutrients and incubation temperatures play a big role in understanding the results, so they need to be shown in one of the tables. Refer-

ring to a paper under review is not sufficient for such important information. Generally, the authors should provide all relevant information (at least in the supplement) if the other manuscript is not publically available yet.

L122-125, L130: While I do agree that differences in environmental variability most likely have an impact on the adaptive capacity of communities, you cannot estimate this adaptive capacity in short-term incubation experiments that run for several days only.

L229-231: I am wondering if it wouldn't make more sense to normalize DMSP concentrations to biomass? This is especially the case if you want to test for "stress-induced algal processes" (L135-136) rather than biomass-dependent effects.

L252-259: I do not think that you can infer growth rates from the Chla measurements, given that there was probably strong photoacclimatory processes happening in response to the change in light fields (naturally varying to constantly high). You do not really need these rates for your story, so I suggest to omit this parameter all together, i.e. also from results and discussion.

L278: The results from the Atlantic experiments are used a lot in the discussion, they should therefore also be included in the results (and methods), especially but not exclusively the previously unpublished ones.

L284-287: Methods are missing for the nanoflagellate and bacteria abundances data,

L291: Methods for irradiance measurements are missing

L314: This is important information that really helps your line of argument, I would therefore put stronger emphasis on this in the discussion.

L328-335: This comparison of standing stocks is highly dependent on the time of sampling. You therefore need to include information about and discussion on the timing of sampling relative to bloom phenology. I.e. if the Arctic and Southern Ocean samples were taken in (macro and/or micro) nutrient depleted waters after a bloom, can you re-

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ally make such general statements on polar vs. temperate waters? Was the temperate sampling also conducted in similar phases of biomass dynamics? If not, you have a problematic bias towards low productivity in the polar samples that needs to be taken into account.

L340-342: This is a strong indication for the importance of other drivers (nutrients, species composition, . . .). You need to show these and check whether there are significant effects here.

L360ff: I really like this way of presenting the data. You should, however, also show the same plot with pCO₂ instead of TA/DIC for comparison because I do not agree with you that this ratio gives a full overview of the in situ carbonate chemistry.

L372 and throughout the entire manuscript: Report the time points in days or hours instead of T1, T2 etc. because this is not consistently the same time point as well as for better readability and consistency throughout the text.

L377-282: This strongly suggests that, due to temperature-driven differences in metabolic rates and their effect on how fast the communities can acclimate to changed conditions, the experiments emerge out of measurement noise at different times.

Discussion: A discussion of stress vs. acclimated response is missing

L399: Everything until here reads more like results than like a discussion section. Please consider rearranging.

L410-412: The authors seem to imply that CO₂ sensitivity is only occurring in form of negative effects, even though there are many studies that show beneficial effects of increased substrate availability for photosynthesis, which is particularly true for picoeukaryotes (e.g. Schulz et al. 2017). Please take this aspect into account.

L436-439: I do not agree that your data really shows this: Figure 9 indicates the Arctic Ocean carbonate chemistry to be actually more similar to the Atlantic than to the Southern Ocean.

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L444-448: Such a comparison only makes sense if the same geographical and temporal ranges, and phases of biomass cycle (pre-bloom/ bloom/post-bloom, before/after winter convection etc.) were covered in the different study areas. Please clarify if this was the case.

L451-455: In the Southern Ocean, several studies have shown strong OA-effects on species composition (e.g. Tortell et al. 2008, Feng et al. 2010, Hoppe et al. 2013, Trimborn et al. 2017).

L455-457: Similarly, you are missing previous work done in the Arctic (Coello-Camba et al. 2014, Holding et al. 2015, Thoisen et al. 2015, Hoppe et al. 2017a,b) that need to be considered.

L460: n=3 is not “highly” replicated

L469: Why are you comparing your data in detail with Archer et al. (2013) but not Hussherr et al. (2017)?

L475: I would rather refer to the most common not the maximum duration.

L482-488: Is this difference really due to different sensitivities, or differences in biological rates, that lead to the fact that small physiological changes are detectable at different time points?

L515-521: You first imply that the short duration of the experiments would render changes in species composition rather unlikely, but then you report one case where you indeed observed changes. I would say that this indicates that the timescales in general would have allowed for changes in composition also in the other experiments.

L543-550: I agree that it is an interesting finding that coastal DMS production seems to be more sensitive to OA than that from the open ocean. This finding does, however, really hint against the proposed mechanisms of insensitivity, because coastal systems are a lot more variable in carbonate chemistry compared to the open ocean (e.g. Thoisen et al. 2015). Thus, the interpretation of and conclusions from the dataset

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have to be reassessed.

Figures 3, 4, 5, 7, 8, S3: Given the lack of control in carbonate chemistry in many experiments (Table S2), this representation is misleading. The data needs to be presented accounting for the real carbonate chemistry in the incubations.

Technical corrections:

L11: I suggest replacing “we increase” by “to increase”

L12: I suggest referring explicitly to climate change instead of environmental change. Otherwise, the step to OA is kind of abrupt.

L28: Do you really mean “region may vary in response to OA” or rather “region may vary in their response to OA”?

L190: replace “made” by “taken”

L207: omit “all” as in the caption of figure 5 you state that these data are not available for two of the stations.

L237-238: According to the Journal style, it would be A_T and C_T for total alkalinity and total dissolved inorganic carbon, respectively

L372: Omit “identical” as irradiances and temperatures were not the same

L497-500: Something does not seem correct in this sentence, please rephrase

L532: Insert “low and” between “periods of” and “stable productivity”

L539: “is insensitive to OA during multiple short term microcosm” instead of “is resilient to OA during multiple, highly replicated short term microcosm”

L542: add additional references mentioned above

L559: Replace “results from our study indicate” by a more honest “we hypothesise” or something similar.

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Table 1: Add macro nutrient (at least NO₃) levels and incubation temperatures (will be more variable than in situ). Also “Comment” should read “Reference”. Shouldn’t “Sample depth” read “Sampling depth”?

All Figures: Please indicate number of replicates and type of error estimate in the caption

Figure 2: Replace “ $\mu\text{E m}^{-2} \text{s}^{-1}$ ” by “ $\mu\text{mol photons m}^{-2} \text{s}^{-1}$ ” or “ $\mu\text{mol quanta m}^{-2} \text{s}^{-1}$ ” in figure and caption. Also, the panels are so close together that the top and bottom axis descriptions get messy, please move them apart a bit.

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