

Interactive comment on “Impact of ocean acidification on Arctic phytoplankton blooms and dimethylsulfide production under simulated ice-free and under-ice conditions” by Rachel Hussherr et al.

Anonymous Referee #1

Received and published: 9 January 2017

Summary

Hussherr et al. present an interesting and timely study that addresses the lack of data we have on the response of DMS concentrations in Arctic waters to ocean acidification. Specifically, the paper presents the results of a 9 day experiment in which seawater was incubated in 10 L gas tight bags under a range of pH/pCO₂ treatments, from pHT 7.9 – pHT 7.2, representing a range from ‘present day’ to end of century to extreme far future values. Furthermore, the authors investigated the role of light, dividing the bags into low light and high light treatments, in order to simulate ice free and under ice

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conditions. The pH gradient method is an established and well-used technique, most useful when the possibility of replication is limited. Acidification was performed using the addition of strong acid and base, again another established technique. Samples for a range of parameters were taken on a regular basis over the 9 day experiment. Within 3 days of the start of the incubation period, a bloom initiated in all bags, leading to an increase in phytoplankton biomass and DMS/DMSP concentrations – differences in the response were attributed to the pH treatments, with no clear observed effect of light. DMS concentrations significantly decreased with decreasing pH, which is in agreement with the one other previous study from Arctic waters (Archer et al. 2013), leading to the conclusion that DMS concentrations during Arctic blooms may be lower in the future, with possible implications for the Arctic climate. The paper is generally well written and logically structured. I have identified a number of minor issues that the authors should address, relating to the methods and the bloom dynamics. Assuming the authors make the suggested changes, this paper would be suitable for publication in Biogeosciences.

Key points

1. Methods: L128: the authors state they ‘poured’ seawater into the gas-tight bags. Through a luer valve? Some clarity is needed as to their exact methods. Pouring is not recommended when handling gas sensitive samples as the gas phase equilibrium may be altered. Notwithstanding the difficulty in pouring anything through a luer valve! Some more detailed explanation is required.

L131: samples were incubated at 4.3 ± 1.6 °C. This seems warm for experiments that are attempting to simulate ‘under ice’ conditions. Can the authors provide some justification/ further explanation?

2. What stimulated the bloom in the bags? Were the team expecting a bloom to occur in the way it did in the bags? Did a bloom also develop in the sampled water simultaneously (i.e. was this a natural or artificial bloom)? Many questions...therefore

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some more discussion would be useful to the reader. After all, without such a nice bloom, it is unlikely a DMS(P) response would have been observed. L492: the authors talk about their findings in the context of the Arctic spring phytoplankton bloom – but actually this experiment sampled waters in August, which must qualify as late summer for the Arctic. So how comparable were the starting conditions to the spring bloom?

3. Reference to Richier et al. (2014) (Phytoplankton responses and associated carbon cycling during shipboard carbonate chemistry manipulation experiments conducted around Northwest European shelf seas) is lacking and should be included in the discussions. The work of Richier et al. is the most similar to this study in terms of the experimental techniques used. The authors do cite Hopkins & Archer (2014) which was part of the same study, but only in a DMS(P) context. The shipboard incubations of Richier et al. and Hopkins & Archer also need to be addressed in the context of this study in terms of the phytoplankton response.

Specific comments and suggestions

Title: it would be more accurate to say ‘DMS concentrations’, as ‘production’ implies that the work include rate measurements.

L45 – 49: These two sentences are somewhat ambiguous and need further explanation. Why is climate change ‘faster and more important’ in the Arctic? In what respect?

L50 – 52: this sentence seems detached and slightly out of context. I see what the authors intend by it. Perhaps they could re-phrase so it says something like: ‘Given that the reduction in extent and thickness of sea ice cover and the acidification of surface waters can potentially impact primary productivity, it is important to consider the associated effects on the production of biogenic climate-active gases. . .’ or similar, just to change the emphasis slightly, and provide an impetus for the work.

L70 – 75, and throughout: the authors make no mention of Richier et al (2014), a recent and relevant paper that should be cited.

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L80: re-word. Suggest: 'Emissions of DMS thus can. . .'

L83: add 'atmosphere' at end of sentence (so reads 'summer Arctic atmosphere')

L97: Not necessary to cite Webb at this point as it is not a review paper. Fine to just cite the references as you specifically mention them later in the paragraph.

L99: although Archer et al. is mentioned later in the paragraph in an Arctic specific context, it would be appropriate to add it to the listed references here.

L142: 'submitted' would be better substituted for 'exposed'

L397: should read 'species'

L403: to improve readability, re-phrase: 'The sole exception was the LL control mesocosm. . .'

L452: Rather than staying 'high pHT', it would be useful to state the range of pH over which the response was observed.

L513 – 517: this long sentence needs some re-wording as it is currently hard to follow and the English is poor in places.

L524: should read 'switched'

L527 – 528: needs re-wording. Suggest: 'These results also suggest that diatoms could have more difficulty in efficiently taking up/assimilating. . .'

Section 4.2: some discussion of the results in comparison to the findings of Richier et al. would be useful, as the two studies use very similar techniques – yet yield quite contrasting responses.

L608 – 610: Archer et al (2013) and Hopkins and Archer (2014) report rate measurements – so this statement is not correct, and their findings should be included in the discussion.

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