

Interactive comment on “Contrasting effects of acidification and warming on dimethylsulfide concentrations during a temperate estuarine fall bloom mesocosm experiment” by Robin Bénard et al.

Anonymous Referee #3

Received and published: 5 November 2018

This manuscript describes the DMS/P results from a mesocosm experiment during which both CO₂ levels and temperature were manipulated. The authors found that changes in CO₂ and temperature did not influence DMSP values, but did impact DMS concentrations. DMS concentrations were linearly anti-correlated with CO₂ levels and positively correlated with temperature. Their results indicate that changes in bacterial production are the cause for the changes in DMS between treatments. The scientific work reported is well done and is important contribution to our understanding of DMS/P dynamics in the surface ocean under changing environmental conditions. It appears

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to be the first mesocosm paper to report the influence of multiple stressors on surface ocean DMS production. The authors also do a good job of outlining the limitations of the experiment. This manuscript should be published in Biogeosciences after the minor revisions stated below have been adequately addressed. Specific comments:

Abstract – not all acronyms are spelled out

Line 56 - Is Larouche the best reference here? Did someone do this work before?

Lines 60-69 - Why is DMSO production not considered as part of the surface ocean cycling processes?

Line 138 – Typo, are should be is

Lines 147-149 - Why did the pH adjustment procedure stop working after the bloom?

Line 167 - Typo, should say saturated

Section 2.3.3 - Were the samples sparged before measuring cleaving the DMSP to DMS?

Line 355 - What were the other PFTs? Were they significant DMSP producers, potentially leading to a lot of DMSP in the water despite their low abundance?

Section 4.3.1 - Were there contrasting studies? Why are they not discussed?

Line 431 – Doesn't this mean that lowered conversion rates (from DMSP to DMS) are not responsible for the lower DMS concentrations? See also the comment to the conclusion section below.

Lines 434-439 – I think these sentences should be saved for the conclusions to avoid summary/redundancy.

Line 482 – Why is it stated that the lower DMS concentrations are likely caused by less conversion from DMSP when the calculated conversion rates are within the normal range (see comment for line 431)?

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Conclusions - I would have liked to see more discussion about what the authors would like to test next (e.g. pathways that cause lower DMS under high CO₂, longer experiments to see if the community adapts to the changed environmental conditions).

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-338>, 2018.