Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-115-RC2, 2018

Biogeosciences
Discussions

Interactive comment on "The ability of macroalgae to mitigate the negative effects of ocean acidification on four species of North Atlantic bivalve" *by* Craig S. Young and Christopher J. Gobler

Anonymous Referee #2

Received and published: 21 May 2018

"The ability of macroalgae to mitigate the negative effects of ocean acidification on four species of North Atlantic bivalve"

This paper evaluates the effect of the presence of the macroalga Ulva rigida on the growth of four North Atlantic bivalve species, Mercenaria mercenaria, Crassostrea virginica, Argopecten irradians and Mytilus edulis. The authors have used small and larger sizes of three out of four species, specifically the three obtained from hatcheries. The pCO2 levels the bivalves are exposed to are high, but conceivable for estuarine systems. The authors claim that "saturation states for calcium carbonate (Ω) were

significantly higher in the presence of Ulva under both ambient and elevated CO2 delivery rates ($p<0.05$)", and that "alkalinity was increased by the presence of Ulva". This might be statistically significant, but as alkalinity actually decreases (or is similar) in some treatments (small Mercenaria, large Mercenaria control pH, small Crassostrea, large Crassostrea low pH) it would be interesting to see the relationship between these parameters and growth directly and visually.

In treatments with Ulva additions, one would expect the variability in pH to be higher due to respiratory activity and production. However, the average pH is higher but the variability in pH seems similar to treatments without Ulva. In fact, I would expect the algal-addition treatments to have a fluctuating pH and the control treatments to be stable, which could arguably have caused the differences. However the authors do not discuss this and the tables do not show these differences in variability of pH. Was the pH fluctuating on a day-night scale in the Ulva treatments? Or was the gas flowrate so high this was not discernable, and what causes the variability in the control treatments?

The nutrient and algae addition to the vessels might cause different nutrient concentrations in the treatments, with Ulva taking up nutrients while they remain suspended in the control vessels, which could have influenced results.

It is unclear what time of the year the experiments have been done (presumable summer due to hatchery times), and how the results might vary in other seasons (i.e. when Ulva is not productive).

The various sizes and the amount of different species of bivalves used in this study make it an interesting read, even though it is not entirely clear what causes the beneficial effect of the presence of Ulva (its effect on the carbonate chemistry, nutrient concentration or something else).

Specific comments:

Methods P.3, line 9 "light intensity ($\sim$200 μmol photons m-2 s-1)", how does this com-

pare to ambient conditions?

P.3, line 23: Isochyrsis should be Isochrysis

P.4, line 17: “some estuarine environments” – representable for the environments of the study organisms and their origin?

P.4, line 32-33: “Well-pigmented, circular sections of Ulva ($\sim$3.5 cm and $\sim$7 cm for experiments in small containers and large vessels, respectively”. These small containers where 1L, while the large vessels had a volume of 8L. The biomass of Ulva however, is 2x as large for the larger volume, which does not respect the ratio biomass/water volume. The authors state that the weight was consistent with the benthic coverage in Shinnecock Bay, would that mean that the 8L vessels had 2x the diameter of the small containers and would water volume not be more important than surface in this case? Or was there more than 1 disk per container (p.5., line 23 states “disks”)? This section is a bit unclear.

P.5, line 16-17: “with discrete and continuous measurements of pH, dissolved oxygen, and temperature”, which measurements were discrete and which continuous?

Results P.6, lines 19-20: “For the larger-sized cohort of M. mercenaria (5.00 $\pm$ 0.41 mm), Ωcalcite and Ωaragonite were significantly higher in treatments containing Ulva and significantly lower in high CO2 treatments” Throughout the manuscript’s result section this way of describing the differences between high CO2 / Ulva treatments is confusing. In the highCO2+Ulva treatment the Ωcalcite is actually lower than the control-Ulva treatment (as expected), however from the text it appears at a first glance that all Ulva containing treatments are higher, the sentences might be clarified to prevent confusion.

Discussion Could the fact that Mytilus seems less sensitive to addition of Ulva be related to the more “natural” (no hatchery) origin of the juveniles and their exposure to environmental fluctuations vs. the more stable hatchery conditions?

If the presence of algae buffered the carbonate chemistry (p.9, line 23) and this is the mechanism for enhanced growth, this should be visible when Ωcalcite/aragonite is plotted vs. growth. However, the saturation state with Ulva is still considerably below 1 in the highCO2 treatments and the SD is high.

Did the authors measure nutrients at the end of the incubations? It would be interesting to explore their theory that through Ulva presence “the nitrogen assimilation effects on alkalinity outweighed the effects of photosynthetic consumption of DIC” (p.9, line 33)

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-115/bg-2018-115-RC2-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-115, 2018.