Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-31-RC2, 2018

Biogeosciences Discussions
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Interactive comment on "Experimental assessment of the sensitivity of an estuarine phytoplankton fall bloom to acidification and warming" *by* Robin Bénard et al.

Anonymous Referee #2

Received and published: 4 April 2018

The manuscript of Bénard and collaborators reports on an experiment that has been conducted using indoor mesocosms (2.6 m3) to test for the effect of ocean acidification and warming on the development of a fall phytoplankton bloom in the Lower St. Lawrence Estuary. The experiment setup comprised 2 sets of 6 mesocosms installed in two temperature-controlled containers, that were filled with seawater sieved onto 250 microns. In one container, the water temperature was raised by 5°C compared to the mesocosms installed in the other container (10 vs. 15°C). A gradient approach (no replicates) has been considered for pCO2/pH covering a range of pH from 7.2 to 8.6. The experiment lasted 13 days and covered the development of a bloom and its decline. Major conclusions of this study are that pCO2 has no effect on all measured param-

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eters and processes while increasing temperature led to a faster build-up of chlorophyll and higher particulate primary production rates. Overall, this is a very well written manuscript that deals with an important topic. The introduction is well documented and shows that while this topic is of great importance, a fair amount of studies has already been conducted, including studies using in situ mesocosms in various environments. Although I would like to ultimately recommend this manuscript for publication in BG, I am concerned by 3 major aspects of this work and would like the authors to answer these comments. 1) Realism. The authors clearly mention that the surface mixed-layer pCO2 is strongly modulated by biological productivity, yet they decided to run an experiment during which a bloom is produced and where carbonate chemistry has been maintained as constant. This would be acceptable if well explained and discussed, but the problem is that "control" mesocosms were actually not controlled (consider changing their name. . .) and pH was left increasing while the bloom was forming to (what I consider to be) very high and potentially unrealistic pH (?) values of 8.6. In situ pH was apparently close to 7.8, these "control" mesocosms appear to me as "perturbed"! Besides this major concern, I have to admit I do not understand how carbonate chemistry was controlled. The authors mention that "acidification" was carried out over day -1. On that day, I actually also observe a sudden increase of pH for the "controls", pH8 and pH7.8. . . How did that happen? Naturally? Why was the increase in pH much higher in the controls than for the other mesocosms. Obviously, some information is missing here. Do you know the reason why pH decreased so fast between day -4 and day -3? 2) Timing. The second concern I have is related to the division of the experiment in 2 phases. Phase 1 corresponds to the development of the diatom bloom extended up to the depletion of nitrate (day 0 to 4) and Phase 2 corresponds to the declining phase of the bloom in the absence of detectable nitrate. Except that this is not really true since temperature increased the speed at which chla built-up and nutrients were consumed (this is not really mentioned in 3.2). At 15°C, except for 1 mesocosm, nitrate was exhausted already on day 2 while at 10°C, NO3 in most mesocosms were actually exhausted on day 4. My point is that since T modified the timing of the bloom (and

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its decline), it does not seem correct to me to consider fixed periods. The build-up of chla and all related statistical analyses should be conducted at 15°C between day 0 and 2, and all tests related to the decline of the bloom between day 3 and 13. Would that change some of your results? 3) Grazing. I regret that potentially the most exciting result of this experiment suggesting that pCO2 "positive" effects on phytoplankton were actually masked by significant increases in micro-grazing is not more developed. I understand the politics behind the publication of papers from a joint experiment, it would just bring much more value to your paper if these results were incorporated and discussed. Top-down control is very often neglected in these OA-OW experiments...

Minor comments.

L217: concentrations were L225: "suggesting a faster loss of pigments...". Not really convinced by that... Is the slope different? L230: "The strong correlation" I do not understand this sentence. How a correlation can suggest anything about importance? Figure 1a: label pHT in situ, why in situ?

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