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Biogeosciences Discussions

Interactive comment on "Combined effects of inorganic carbon and light on *Phaeocystis globosa* Scherffel (Prymnesiophyceae)" *by* A. Hoogstraten et al.

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This is an environmentally relevant study, in that the responses of the haptophyte Phaeocystis globosa to future changing pCO2 and irradiance are likely to have a large influence on the harmful blooms this species causes in the North Sea and elsewhere. Although the study is relatively basic and could have benefited from some additional analyses (see below), the responses they recorded- or lack thereof- are significant observations that are worthy of publication. One area that I think could be much better developed is to consider the broader implications of their findings, beyond just the harmful blooms caused by P. globosa. As they correctly point out, the three main

species within the genus Phaeocystis are key players in global biogeochemical cycles, for instance through their production of DMS and influence on carbon and nitrogen cycles. I would like to see a little further consideration of their results in this larger context. For instance, there have been at least two field studies on the responses of Phaeocystis antarctica to pCO2 changes that would make a good contrast and comparison to the results presented here. These papers are Tortell et al 2008 (GRL 35) and Feng et al. 2010 (DSR I 57); our study (the latter one) even includes an examination of interactions between irradiance and CO2 that differs in outcome from the story presented here. There is also a considerable literature on light effects (alone) on P. antarctica growth from people like Kevin Arrigo, Walker Smith, and Jack DiTullo that would be relevant to consider too. Tortell et al. 2008 (L&O 53) and 2010 (J Phycol 46) looked at inorganic carbon utilization as a function of pCO2 in P. antarctica, and Tortell et al 2002 (MEPS 236) showed pCO2-driven community shifts in an Equatorial Pacific diatom/Phaeocystis community (presumably P. pouchetii). My point is that there is a lot of literature on this genus that would be appropriate to discuss and that would bring these results into a larger picture that includes other closely related members of this biogeochemically key genus of haptophytes. I realize that this comment is exactly the opposite of the recommendation of the other reviewer, who would like to see less mention of other Phaeocystis species, but I disagree and think that the discussion would benefit greatly from considering some of these other related studies as well. I do agree with the other reviewer that the discussion of and comparisons with the Wang et al 2010 and Chen and Gao 2011 papers, which both deal with CO2 effects on this same species, could also be developed in much more depth than is the case now. Specific comments: Abstract: The phrase "globally dominating phytoplankton species" seems to imply that it dominates everywhere. I know this is not what the authors mean, how about rephrasing it as "an ecologically dominant species in many areas around the world", or something like that? Introduction, p. 12355, lines 19- 21- Since pCO2 is given throughout the paper as micromol kg-1, this should probably be kept consistent when discussing other papers too. The units could easily be converted from ppm here.

p. 12356, lines 1-3- I agree with this statement, but what is missing is a consideration of how the experimental nutrient concentrations used here compare to typical in situ levels in natural P. globosa blooms. I assume the levels of 3.75 uM phosphate and 60 uM nitrate (p. 12357) are considerably higher than natural levels, so is this important to consider in interpreting these results? P12356, line 19- Actually there have been a number of other studies on HAB responses to pCO2 changes, these include Rost et al 2006 (Plant Cell Env, dinoflagellates), Fu et al 2008 (Harmful Algae, a dinoflagellate and a raphidophyte), Fu et al 2010 (AME, a dinoflagellate), a new paper in PLoS ONE (Tatters et al. 2012, Pseudo-nitzschia), as well as the two previous studies on P. globosa mentioned above. I'm not suggesting that all of these papers need to be referenced here, but this statement is not strictly accurate. p. 12357, lines 5-7- How were these two light levels chosen? Was a complete light response curve available for this species to help choose appropriate suboptimal and saturating light levels, or were these just chosen arbitrarily and assumed to be limiting and saturating? p. 12358, results are presented on cell densities before the relevant methods are given in the text (not until the bottom of the following page). p. 12358, the cultures appear to have been fully acclimated to the experimental conditions during the 6 days pre-experimental growth period, but it would be helpful to readers to know how many cell divisions this represented. p. 12359- It is good that the added 60 uM silicate was considered in the CO2Sys calculations, but it is not clear why it was added to the medium at all, since Si is not a nutrient that is required by Phaeocystis. p. 12361- It is too bad that particulate organic phosphorus was not measured as well as carbon and nitrogen, it is a relatively simply spectrophotometric measurement and would have avoided having to calculate cellular N:P ratios based only on phosphate drawdown. Methods- Likewise, measurements of DMS/P would have greatly added to the biogeochemical relevance of the study. p. 12363- Lines 5-8- Pointing out a trend and then saying it is not significant is probably unnecessary. lInes 17-18- This statement is redundant to the previous sentence, which already gives the information that no effect on Fv/Fm was observed except in the high light cultures. p. 12364 and Fig 3- Is Fig 3 really needed, considering

that there are no significant trends shown on any of the three panels? This could be easily said in the text instead. p. 12364, lines 17-20- Obviously, the changes in C:Chl ratios were driven entirely by changes in Chl a cell-1, since POC quotas didn't change. The authors say this in the discussion but it could be pointed out first here. p. 12365, lines 3-4- Since this paper doesn't have an excess of data in it, is there a need to relegate some results to supplementary tables? Why not show these data in the paper? p. 12365, lines 22-25- Again, it is a bit odd to say that N:P ratios were highest in the low light cultures, then follow in the next sentence with a statement that "There was no significant effect of the two different light intensities on the N:P ratio". p. 12366- The statement that "this trend was opposite to the results presented by Kim et al. (2006)" is confusing without first telling readers what their results were. And why the difference in your study? Some additional explanatory text is needed here. p. 12367, lines 16-19- How much higher are your C:N ratios than the ones reported from single cells from previous work? The text needs to be a little more specific and quantitative here. p. 12367, bottom- Here is where discussion of some of the many previous studies of irradiance effects on P. antarctica would be appropriate. p. 12368, top- Whether these growth rates would allow them to outcompete diatoms obviously depends on the diatom, all diatoms do not grow at the same rate. p. 12368, bottom- The concluding sentences illustrate the reason why some direct measurements of organic sulfur pools in this experiment would have added greatly to its biogeochemical relevance. Summary- This paper should be published after minor revisions. The biggest change I would recommend is to expand the discussion to include other relevant Phaeocystis studies, thus increasing the global relevance of the results. Dave Hutchins.

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