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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.

Anonymous Referee #1

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General comments:

The manuscript by Hoogstraten and colleagues deals with the combined effects of elevated CO2 and high light on single cell assemblages of P. globosa. Although the manuscript presents in general an interesting approach and some interesting data, the authors lack to identify knowledge gaps they are trying to fill and, even more important, to highlight new findings. There are two papers to be found in the literature (Wang et al. 2010 and Chen 2011) dealing with high CO2 and/or light effects on P. globosa presenting data on the same parameters tested here: growth rates, photosynthetic efficiency, POC, PON, chl a etc. In order to discriminate earlier findings from those

presented here, the authors are urged to compare findings more precisely and with more caution (for details see comments below).

A complete and precise description of the experimental approach is mandatory in order to understand the experimental approach. Referring to manuscripts which are not yet available to the reader (Hoogstraten et al. 2011; submitted or in preparation) does not provide the methodical background needed to evaluate the relevance of obtained data.

Furthermore, I strongly suggest implementing additional biological data if available, e.g. DMS, the importance of which is highlighted by the authors. The manuscript discussion even ends with a concluding paragraph on the consequences of changes in DMS production. To summarize, throughout the manuscript, aims, methods, terminology (e.g. consistent abbreviations for the treatments) and purpose should be presented more straight forward and more precise. These general comments and the following remarks and suggestions are given in order to improve the manuscript that calls for a rearrangement and careful revision.

Minor remarks and suggestions:

Introduction: General: I suggest to initially introduce P. globosa, since it is the organism of interest in this manuscript. I think it is not needed to mention P. Antarctica and P. pouchetii. I would rather be interested in more information about the organism investigated here, e.g. concerning the formation of single cells or colonies (mucus layer), bloom dynamics, DMS production and global warming as well as its importance for carbon sequestration and potential strain specific responses (for example strain Pg G "A" vs. CCMP1528 vs. ST 97). Since these are central aspects in the manuscripts discussion it would be important to introduce these issues. p. 12355, l. 2: If I am not mistaken, "cells" should be single "as single cell" p. 12355, l. 17: replace "as" by "into" p. 12356, l. 4-9: Maybe it would be better to combine these sentences to have a better "reading flow". p.12356, l. 7: delete "AD" p.12356, l. 13: I strongly recommend referring to a more detailed/basic/expert work on ocean carbonate chem-

istry like e.g. Zeebe & Wolff-Gladrow, 2001. The work the authors refer to (Hoogstraten 2011) is not available yet and most likely not focusing on carbonate chemistry. p.12356, l.19: The authors should provide more specific information what "this respect" means. Current knowledge and gaps to be investigated should be described more clearly in the introduction. p.12356, l. 20: The authors could specify in more detail what the "socio-economic" relevance of this species is. p.12356, l. 22: Maybe the authors could exchange "environmental changes" with "CO2 and temperature" directly. The effect of CO2 and temperature is focus of this work and should not be "hidden" in parentheses. p.12356, l. 24: "clear yet"? Until now the authors did not present that this species was investigated in the context of global change at all. Please rearrange the structure of the introduction. p.12356, l. 25: "as an accompanying anthropogenic effect" I suggest to mention and describe this in the paragraph about anthropogenic emissions and consequences (l.6-9 on this page). p. 12357, l. 2: Implementing a brief explanation how rising CO2 levels lead to a change in the light intensity in the paragraph about anthropogenic emissions and consequences (l.6-9 on this page) would be beneficial to the reader and clarify the intention for this experimental design. p. 12357, l. 5 and 7: I suggest to exchange the word "classified" by "referred to as". p. 12357, l. 8 and 9: Why were different CO2 concentrations reached for the intermediate treatments? It would be nice to see initial values in the methods and/or results section to compare starting conditions. Differences due to technical constraints or biological activity should be presented and thoroughly discussed in the results and discussion section. For the introduction, it might be sufficient to introduce HL and LL at low, intermediate and high CO2 values and the range of pCO2 in order to give a clear outline to the reader.

2 Material and Methods p. 12357, l. 17: "aged" seawater- The authors should specify time and conditions of "aging" as well as the purpose of the use of "aged" seawater. p. 12357, l. 19 and l. 23: The authors should give the full description of methods and experimental design if the manuscript referred to (Hoogstraten 2011), is not available/ published yet! p. 12358, l. 4: "Natural gas mixes" What does this mean? p. 12358, l. 6 and table 1: Why are there such differences among the same CO2 treatments

(intermerdiate CO2 at HL and LL)? If the variation is a result of algae activity I suggest providing target and initially reached values in the methods section, presenting the variation that occurred due to biological activity in the results section and if this variation has consequences for the interpretation of the data set, this issue should be argued in the discussion section.

2.2 Inorganic carbon p. 12359, l. 14: delete "excel spreadsheet" and write e.g. "by the use of CO2sys".

2.4.2 Chlorophyll a and 2.4.3 POC and PON p.12360, l.14 and p. 12361, l. 2: I don′t understand the procedure. Pore size of Whatman GF/F- filters is 0.7 μm (not 0.2 μm as given in the description), which is typically used for these parameters. The authors should explain precisely which filters they used. Sampling of particulate samples might be better described as to be filtered "on" the filter for analysis instead of "over".

3 Results General: The authors introduced the abbreviations HL and LL for the figures; I suggest doing so for the text as well. Additionally, the text might be easier to understand, if short and precise terms/abbreviations are introduced for the CO2-levels as well. Wang et al. 2010 is cited twice (as seen in the reference list) and the "a" and/or "b" should be added consistently also in the text. p.12362, l. 2: I suggest implementing the initial/target values for CO2 in table 1. I suspect the different values, especially for the intermediate CO2 treatment, are due to biological activity, but to evaluate this (e.g. DIC draw down, cell abundance, photosynthetic efficiency) it would be nice to see the initial CO2 concentration (compare comments above). p.12362, l. 8: exchange "larger" with "higher" p.12362, l.24: if the authors give the μ derived from linear regression of the natural logarithm of the cell abundance I suggest to simply write: The cells grew at μ=0.7 d-1. If you write dividing once a day, as you did, I would rather show growth dynamics as doubling time instead of μ.

3.3 p. 12363, l. 10: I suggest giving a range of photosynthetic efficiency during this experiment. "High", relative to what?

3.5 p.12365, l. 12: I suggest a more neutral word like “stimulate” or “increase” instead of “positive” effect of CO2 on phosphate uptake.

4 Discussion p. 12366, l. 1-9: I suggest moving these sentences to the introduction or simply delete them (see general comments). p. 12366, l. 9 . . .may “act” as? Missing word p. 12366, l. 10 ff: I don’t see a problem in stable CO2-concentrations within a culture, but among cultures of the same pCO2 (especially among the intermediate CO2 HL and LL treatment). Since there were only slight differences for cell abundances, Fv/Fm, and chl a between intermediate CO2 at HL and LL (table 2 and fig. 1) I do not understand what induced the difference in CO2 if nutrient media was aerated with the same commercially available gas. p. 12367, l. 1 The authors highlight corroboration to results obtained by Wang et al. 2010 concerning the combined effect of light and CO2 on growth rates and the authors suggest consistency among different strains of P. globosa. I do not agree. If I am not mistaken, the authors compare data obtained at a light intensity of 240 μmol photons m-2 s-1 (HL-treatment in the present paper) with data obtained by Wang et al. at 80 μmol photons m-2 s-1 (comparable to the LL treatment in the present paper which showed no response). For me, this might indicates a strain specific response and should be discussed in more detail. p. 12367, l. 3: It is the first time you mention strain specific responses of P. globosa. This issue should be introduced earlier (Introduction) to be discussed here. p. 12367, l. 10: The authors argue the contradiction in cellular POC content to findings by Wang et al. 2010a by differences between solitary and colonies forming cells. In order to estimate the importance of different POC contents for solitary or colony forming cells the natural occurrence/dominance of both types of P. globosa in the ocean should be discussed.

The closing paragraph of the discussion deals with DMS production and global warming. Unfortunately the authors did not highlight, what new findings were obtained during their study and what the readers learn about the potential performance of P. globosa in a high CO2, high light ocean.

Figures: Fig. 1, Legend: I assume the lowest line supposed to be high CO2 LL

References Chen, S. and Gao, K. S.: Solar ultraviolet radiation and CO2-induced ocean acidification interacts to influence the photosynthetic performance of the red tide alga Phaeocystis globosa (Prymnesiophyceae), Hydrobiologia, 675, 105-117, 2011.

Wang, Y., Smith, W. O., Jr., Wang, X., and Li, S.: Subtle biological responses to increased CO2concentrations by Phaeocystis globosa Scherffel, a harmful algal bloom species, Geophys. Res. Lett., 37, L09604, doi:10.1029/2010gl042666, 2010a.

Zeebe, R. E. and Wolf-Gladrow. CO2 in Seawater: Equilibrium, Kinetics, Isotopes. Elsevier Oceanography Series, 65, pp. 346, Amsterdam, 2001.

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