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Biogeosciences
Discussions
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Interactive comment on "Enhancement of photosynthetic carbon assimilation efficiency of phytoplankton assemblage in the future coastal ocean" *by* J.-H. Kim et al.

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We would like to thank to anonymous Referee #2 and his valuable comments. Find the detail responses to comments in below.

Comment 1) The main result obtained, that is the efficient use of light under acidification and greenhouse conditions, is well presented and summarized in the first paragraph of the discussion. Nevertheless, the manuscript fails when interpreting this result. The authors continuously comment the results obtained by other authors to explain or support what they found.

Response 1) We agreed on the reviewer's opinion, and lots of space was wasted

on comparing the previous other studies. This problem will be removed in revised manuscript.

Comment 2) The manuscript also fails when the authors attempt to connect this higher efficiency in light utilization with the lack of differences in primary production and (seemingly) significant differences in chlorophyll concentration. This apparent inconsistency is quickly solved invoking grazing and arguing that more research is needed.

Response 2) Photosynthetic enhancement under greenhouse could have influence on primary production. In the graph below (Fig. 1), chl a normalized primary production, represent that contribution of chl a to primary production enhanced significantly under greenhouse treatment. However, no difference was found in result of community production. Thus, depressed biomass could negatively influence community production, and community production seems to have neutral responses under greenhouses. We suggested that an important factor for regulating biomass bloom in future environment was grazing activity (in supplementary materials, Fig S4.). Also figure 1 will be added into main manuscript.

Comment 3) Species succession, with winners and losers, as G. Hallegraeff and the reviewer 1 indicate, is only mentioned as a possibility at the end of the last (summary) section. In my opinion, 21 days is a long time period, long enough, to allow species succession not only in the phytoplankton community but also within the whole microbial community. Surely, the succession was different for each experimental condition and the study of these 3 successions would help to interpret the dataset, since changes in the photosynthetic responses can also be due to changes in species composition.

Response 3) We agree on the referee's opinion, but the data (about whole microbial community) are too extensive to describe on this manuscript. We worried that the manuscript could lose its originality if it was more focused on community structure. We have provided some data published in series papers such as abundance of autotrophic and heterotrophic plankton (Kim et al. 2010, ES&T). Based on series paper, we will add

a brief paragraph about phytoplanktonic and grazer composition in revised manuscript.

Comment 4) In addition, the differences observed in photophysiological (Table 1 and Figure 2 right) and photosynthetic (Table 2 and Figure 3) parameters mainly occurred during the last phase of post-bloom, when divergences among the 3 treatments were more evident (Fig. 1). According to these comments, not only should the discussion be reorganized, the motivation of the research (in the introduction) should also be reformulated.

Response 4) Performances of photosynthetic activity are largely distinguished by the timing of phytoplankton bloom. For example, 14C incorporation rate was significantly higher than other conditions during the post-bloom period. Also the overall efficiency of rETR was reduced during the experimental period in acidification than in other conditions. Consequently, these responses could be caused by species composition of phytoplankton. We will try to rebuild motivation and rationale of this study.

Comment 5) Without complementary information on species composition and succession, the manuscript looks like a short note reporting the interesting results presented in tables and figures

Response 5) We added more information through the supplemental materials. They include composition of taxonomic groups (Fig. S3), grazing results (Fig. S4), and experimental conditions (carbon chemistry, nutrient concentration, Figs S1-2). We are sure this information will helpful improve our manuscript.

Comment 6) -The results on rETR (Fig. 2) are at least surprising. Differences in rETR among treatments only depend on differences in quantum yield of PSII, because irradiance is the same for the 3 treatments (Chl a fluorescence measurement, line 17). However, differences in quantum yield between treatments (Fig. 2, left) were not so evident. In fact, these differences are not mentioned in the text.

-Then, why rETR at high irradiance is significantly lower for acidification (in blue) than

for greenhouse (in red) on days 15 and 19 whereas maximum quantum yields were similar?

-Moreover, why rETR was higher for acidification than for the present conditions?

Response 6) -Effective quantum yield (ΦPSII) under acidification was also lower than other condition, but difference of ΦPSII was too small among the experimental conditions. Although, difference of ΦPSII was small, difference of rETR could be greater than ΦPSII because extremely high light intensity causes the supplying amount of quantum into the light-harvesting center. We emphasized on the rETR result more than ΦPSII in the previous manuscript, but result of ΦPSII will be explained in revised version.

-Reduction of rETR under acidification was caused by lower ΦPSII (effective quantum yield; not maximum quantum yield) under acidification when phytoplankton exposed to high light intensity.

-Finally, last question incorrect. rETR under acidification was lower than other conditions.

Abstract

Comment 7) The percentages given here are not stated in the main text. Line 15. There is a missing word here:: : : were not significantly different between??? and greenhouse conditions,..

Response 7) This is a similar comment suggested by referee #1 (2nd comment). In this study, data extensive, thus we think that enumerate and simple comparisons among the experimental treatments are not meaningful in the abstract. We will change parts of abstract with simple and universal expressions based on the tendency of experimental parameters among the treatment. We also checked the overall part of the manuscript to avoid any similar mistakes.

Introduction

Comment 8) Page 4613, line 14: On the contrary to some microalgae: : : What microalgae you refer? Phytoplankton are microalgae

Response 8) We will change microalgae to phytoplankton

Comment 9) Page 4614, lines 16.18: Higher abundance of small size phytoplankton in high temperature conditions is not necessarily always true. What commonly occurs is a decrease or disappearance of large phytoplankton. Therefore, I suggest the modification of this sentence to indicate the dominance of small phytoplankton in environments with high temperature.

Response 9) We will delete ambiguous expression, and modify it to "community structure and species composition are strongly affected by elevated temperature with altered and varied bio-physical activities across all trophic levels."

Materials and methods

Comment 10) Page 4615, lines 15 to 18 and lines 24-25: Information on how the CO2 concentration and pH were controlled must be provided. Why such amounts of nutrients were added?

Response 10) We added results of CO2/pH and temperature on supplementary materials (Fig. S1).

Comment 11) Page 4616, line 7: KST means Korean Solar Time?

Response 11) KST means Korean Standard Time

Comment 12) Lines 20: The sentence "the curves were fitted to a model with photoinhibition parameter of Platt" is repeated below in line 24.

Response 12) We will delete the repeated sentence.

Results

Comment 13) Page 4618: Nutrient levels are not shown

Response 13) Nutrient levels were represented on supplementary materials (Fig S2).

Summary

Comment 14) -Page 4625, lines 6 to 8: The sentence "The main objective of this study was to investigate the physiological and ecological affects in the phytoplankton community under future climate conditions" looks too ambitious. Physiological and ecological domains are larger than photosynthetic responses. -Tables 1 and 2. Clarify what the letters (a, b, ab) indicate.

Respons 14)

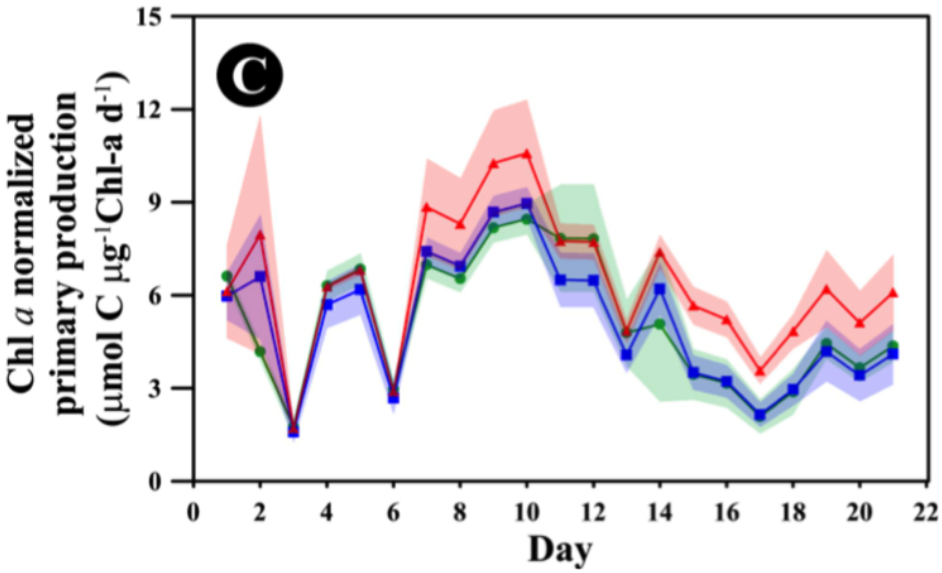
-We will change that sentence to "The main objective of this study was to investigate the photosynthetic performance for evaluating physiological and ecological effects in the phytoplankton community under future climate conditions." -Meaning of different letters was already expressed on the caption of table. "Different letters indicate a significant difference based on Tukey's multiple comparison ($p < 0.05$)."

Reference

Kim, J.-M., Lee, K., Yang, E.J., Shin, K., Noh, J.H., Park, K.T., Hyun, B., Jeong, H.-J., Kim, J-.H., Kim, K.Y., Kim, M., Kim, H.-C., Jang, P.G., and Jang, M.C.: Enhanced production of oceanic dimethylsulfide resulting from CO2-induced grazing activity in a high CO2 world, Environ. Sci. Technol., 44(21), 8140-8143, 2010.

Figure 1. Chlorophyll a normalized community production during the experimental period in the present (green), acidification (blue) and greenhouse (red) conditions. Colored shading represents the standard deviation from the mean of replicate enclosures.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C1793/2013/bgd-10-C1793-2013-supplement.pdf



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