Biogeosciences Discuss.,
doi:10.5194/bg-2016-403-RC2, 2016

Biogeosciences
Discussions
Open Access

Interactive comment on "Combined effects of elevated pCO_2 and temperature on biomass and carbon fixation of phytoplankton assemblages in the northern South China Sea" *by* G. Gao et al.

Anonymous Referee #2

Received and published: 18 November 2016

The MS "Combined effects of elevated pCO2 and temperature on biomass and carbon fixation of phytoplankton assemblages in the northern South China Sea" by Gao et al. describes one of the first studies trying to understand changes in productivity at two stations (on shore and off shore) the South China Sea to a combination of both enhanced temperature as well as CO2.

The hypothesis to be answered is an important one and we do need high quality datasets to understand potential changes in productivity in a future ocean. Unfortunately I find this manuscript to be written very confusingly with some significant issues on experimental design, data evaluation and interpretation.

Please find my detailed comments below.

Abstract:

Please keep "near shore" and "off-shore" as descriptions of the experimental sites. It becomes very confusing reading SEATS and D001 over and over again.

The flow of the text is constantly interrupted by parentheses – please change.

What does the last sentence mean? "...being more sensitive to these two global change factors" . What does more sensitive mean? In comparison to what? Please clarify.

Introduction:

Line 58 to 67: The authors state different changes in SST increase over time (global SST, South China Sea, global mean rate. At least one of these temperatures is redundant. Also define if you mean South China Sea SST or average temp. Line 73: correct the typo in phytoplankton. (in general please only submit a manuscript after carefully revising it – obvious typos as well as incomplete sentences -see below) should have been revised by at least one co-author!) Line 79: this is not a sentence. – also this non-sentence needs a reference. Line 82/83: Add a reference. Line 85: Define RCP scenario Line 95: change the wording "problem" Line 98: Gao et al 2012a was certainly not the first suggesting energy and metabolite allocation from the down-regulation of the CCM. Despite -to my knowledge the cited study (being a great study) did not investigate any CCM parameters. Line 101: The cited papers represent only a minor fraction of papers with a "neutral" CO2 response – maybe use a review paper as citation instead adding "and references therein" Line 125: what do the authors mean with "assimilation number"?

Methods: Some general remarks: Why did the authors not shield the incubations from the very high light intensity? Intensities of >1000 μmol photons m-2s-1 as they correctly stated are pretty high and most (all) other studies used much lower light intensities. And phytoplankton change their vertical position over the day!

Did the authors monitor the temperature in the incubation continuously? The tank can easily heat up several degrees over the day if not monitored carefully. Please add this information to the MS.

For future reference, it is best practice to measure at least DIC or TA additional to pH for these kinds of experiments. I know several reviewers who would not accept this MS just based on the "sloppy" characterization of the carbonate chemistry.

Detailed comments:

It is unclear when the DDT was actually incubated. When was the sample taken and when was the 14C added. Was the experiment run every day or was it run once at the end of the incubation.

Please revise the method section of the experimental setup and the corresponding measurements in order to understand the timeline of the measurements during the incubation.

Urgently needed additional information: Nutrient concentrations (specific values and not > or < (see Table 3)) prior to the acclimation start as well as nutrient concentrations at the end of the incubation.

Regarding the statistical analysis: I assume that the authors had a maximum of three replicates. The authors state that the data "were conformed to a normal distribution". This seems to me pretty much impossible. How can n=3 be considered a normal distribution? Did the authors verify the outcome of the Shapiro-Wilk test? Please clarify!

Results:

Line 208-215: Most of the results listed here are basic carbonate chemistry responses – please shorten this section.

The authors compare the initial chl a concentration of both stations. As the authors

know – the chlorophyll concentration is changing daily – even hourly, can be different 500 meter next to the sampling spot and obviously will vary with season. This whole section including the discussion does not mean anything if you don't look at long-term changes and differences. Please revise!

Line 255 -276: I feel that without the information on nutrient concentration in the different acclimations any data obtained are oblivious. Please revise if nutrient data are available.

Discussion: Line 284-288: The authors state that phytoplankton growth commonly increases with temperature This statement is simply wrong as phytoplankton which growth at its optimal temperature will be heat stressed at even higher temperature. Please revise. Also – the authors did not test a full temperature growth response curve for the phytoplankton in the North China Sea.

Line 293: Please change the citation (Wu et al. 2008) to a more original work. I also feel that the citation culture in this manuscript should be improved.

In the conclusion the authors state that the study demonstrates that ocean warming would stimulate DPP and that the effect can be dampened by OA. This would be an important result – but I feel that the data quantity and quality does not support this very general conclusion. The authors also disqualified their results with their own discussion in line 378-402 when they talk about the shortcomings of short time vs. long time acclimations. This manuscript is a short time incubation and should be discussed as is including the potential shortcomings.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-403, 2016.