

Biogeosciences Discuss., referee comment RC2  
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## Reviewer's comments

Anonymous Referee #2

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Referee comment on "Contrasting responses of phytoplankton productivity between coastal and offshore surface waters in the Taiwan Strait and the South China Sea to short-term seawater acidification" by Guang Gao et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-326-RC2>, 2022

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February 14, 2022

Reviewer's comments for

Ms. Ref No.: bg-2021-326

Title: Contrasting responses of phytoplankton productivity between coastal and offshore surface waters in the Taiwan Strait and the South China Sea to future CO<sub>2</sub>-induced acidification

Authors: Guang Gao et al.

Submitted to: Biogeosciences

General comments

This paper examines the impacts of ocean acidification on primary productivity using a number of deck incubation experiments. A dataset of 101 experiments is unique and valuable. I consider that an approach using multiple experiments under a protocol is reasonable to assess phytoplankton responses to ocean acidification. The dataset can be useful to draw some response patterns of primary productivity to ocean acidification under any key environmental conditions. However, I found some issues in the current manuscript, so these should be clarified before publishing this paper in the journal Biogeosciences.

#### Major comments

- The authors claim that higher NO<sub>x</sub> concentrations are responsible for the primary productivity enhancement under ocean acidification conditions. However, I consider that we cannot differentiate direct effect of NO<sub>x</sub> levels on phytoplankton physiology from the effect of different phytoplankton community structures derived from different NO<sub>x</sub> availabilities. Furthermore, NO<sub>x</sub> data are very few relative to other parameters observed in this study. NO<sub>x</sub> data are not at the surface but at the bottom of surface mixing layers. No direct link between the surface PP and NO<sub>x</sub> at the bottom of surface mixing layer are not explained in the manuscript. I do not consider that a conclusion is not reasonably supported by the results presented in this study.

#### Specific comments

- L17 and L46. The authors mention "multiple drivers", but this study treats only one driver of ocean acidification (pH). This mention does not fit to this study's introduction.
- L23-24. Are the digits of four reasonable?
- L95-96. It is helpful to show the number of stations examined in each category of the coastal zone, continental shelf, slope, and deep basin.
- We need to know how the pH electrode was treated after the calibration. Just after the calibration using NBS buffers, electrodes usually do not work appropriately in seawaters because of the quite different ion intensity between the NBS buffers and seawaters. Such electrodes may be kept in a seawater for a certain period.
- How did the authors define the quantitative limits?
- How were the nutrient samples stored until analysis?
- Was the CO<sub>2</sub>-saturated seawater filtered through 0.22 μm before saturating? How was the CO<sub>2</sub>-saturated seawater made? The authors should describe the typical volume of

the added CO<sub>2</sub>-saturated seawaters with the volume of pre-filtered seawater for incubations.

- The authors should explain the reason of selecting 0.4 pH unit.
- In my understandings, whether 14C incubation for 24 h measures net production or not is under debate. In such condition, CO<sub>2</sub> enrichment in HC treatments may alter 14C incorporation manner in phytoplankton.
- chlorophylls a --> chlorophyll a
- The unit for Talk of umol/L?
- The unit for PAR of W/m<sup>2</sup>/s?
- The results of manipulation in carbonate systems should be described first. The readers need to know actual pH reduction, pCO<sub>2</sub> and CO<sub>2</sub> increase. The 0.4 pH reduction, even if this is identical for all experiments, would result different pCO<sub>2</sub> increase depending on ambient temperature, salinity, and carbonate system.
- The samplings of the bottom of upper mixing layers are not described in Materials and Methods section.
- Regardless of the importance of NO<sub>x</sub> availability in this study (L220), spatial NO<sub>x</sub> distribution was not shown. Furthermore, NO<sub>x</sub> data are shown only 15, few data relative to other parameters, and this should be explained in the Materials and Methods section. Six out of 15 data are located out of 95% confidence intervals in Fig. 6. Does this situation actually support your conclusion?
- The paper shows nutrient data only for NO<sub>x</sub>. We would like to know phosphate, silicic acid, ammonium, and probably iron levels.
- This discussion with a positive effect on diatoms is inconsistent with the discussion on a negative effect on diatoms in L237-238. Interpretations of the impact of OA on diatoms are not so straightforward as discussed here.
- The "harmful algal blooms" is sudden and unacceptable, not discussed in the Discussion section.
- 2. I am not sure whether the unit of total alkalinity is umol/L and that of PAR is W/m<sup>2</sup>/s.
- 5 caption. L534-536. This is described only for this figure and can be removed because of the repetition of the Result section.