

Biogeosciences Discuss., author comment AC2
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Reply on RC2

Guang Gao et al.

Author 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.,
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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.

Response: We appreciate these comments and have revised the manuscript based on the reviewer's suggestions.

Major comments

- The authors claim that higher NO_x concentrations are responsible for the primary productivity enhancement under ocean acidification conditions. However, I consider that we cannot differentiate direct effect of NO_x levels on phytoplankton physiology from the effect of different phytoplankton community structures derived from different NO_x availabilities. Furthermore, NO_x data are very few relative to other parameters observed in this study. NO_x data are not at the surface but at the bottom of surface mixing layers. No direct link between the surface PP and NO_x 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.

Response: We have realized this issue and removed the data of NO_x as suggested by two reviewers.

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.

Response: We did treat only one driver of seawater acidification but investigate its effects

in different environments (temperature, salinity, solar irradiances, nutrients, etc.). To take the reviewer's suggestion, we have revised the text to "In order to examine effects of SA in changing environments" at line 17 and "it is important to understand the responses of the key players of marine biological CO₂ pump, the phytoplankton, to seawater acidification" at line 52.

- L23-24. Are the digits of four reasonable?

Response: We have changed to two digits throughout the text.

- 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.

Response: It has been revised to "Investigation areas include the continental shelf (0–200 m, 22 stations) and the slope (200–3400 m, 44 stations), and the vast deep-water basin (> 3400 m, 35 stations). In the continental shelf, the areas with depth < 50 m are defined as coastal zones (9 stations)" at line 103.

- 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.

Response: What the reviewer mentioned is exactly right. We usually keep the electrode in seawater for half an hour before the measurement. This information has been added and it reads "After the calibration, the electrode of pH meter was kept in surface seawater for half an hour and then the formal measurements were conducted" at line 113.

- How did the authors define the quantitative limits?

Response: It was determined by compared measured values with certified reference. However, this part was removed since the reviewer 1 does not think it makes sense.

How were the nutrient samples stored until analysis?

Response: The nutrient samples were measured within 24 h upon sampling and stored in a 4 °C refrigerator before analysis. However, this part was removed since the reviewer 1 does not think it makes sense.

- Was the CO₂-saturated seawater filtered through 0.22 μm before saturating? How was the CO₂-saturated seawater made? The authors should describe the typical volume of the added CO₂-saturated seawaters with the volume of pre-filtered seawater for incubations.

Response: It has been clarified to "Seawater that was collected from the same location as PP and filtered by cellulose acetate membrane (0.22 μm) was used to make the CO₂-saturated seawater, which was made by directly flushing with pure CO₂ until pH reached around 4.50. When saturated-CO₂ seawater was added to the HC treatment, equivalent filtered seawater (without flushing with CO₂) was also added to the AC treatment as a control. The ratios of added saturated-CO₂ seawater to incubation seawater were about 1:1000" at line 143.

- The authors should explain the reason of selecting 0.4 pH unit.

Response: It has been clarified to "To study the impacts of projected SA (dropping by ~0.4 pH) by the end of this century (RCP8.5) on marine primary productivity in different

areas (Gattuso et al., 2015)" at line 97.

- In my understandings, whether ¹⁴C incubation for 24 h measures net production or not is under debate. In such condition, CO₂ enrichment in HC treatments may alter ¹⁴C incorporation manner in phytoplankton.

Response: We would like to take this suggestion and remove the term of "net". In HC treatments, there are more CO₂ and HCO₃⁻ and less CO₃²⁻ compared to AC treatments, which may alter ¹⁴C incorporation manner. However, ¹⁴C exists in all carbonate forms and theoretically the ratio of ¹⁴C to ¹²C is consistent in all forms. Therefore, this alteration of carbonate system may not affect the accuracy of carbon fixation using ¹⁴C technique.

- chlorophylls a --> chlorophyll a

Response: Corrected.

- The unit for Talk of umol/L?

Response: It has been corrected to "μmol kg⁻¹ SW".

- The unit for PAR of W/m2/s?

Response: The unit of W/m2/s can be used for light intensity, particularly for solar radiation (Wild et al., 2005; Yuan et al., 2021; Proutsos et al., 2022)

Proutsos, N., Alexandris, S., Liakatas, A., Nastos, P. and Tsiros, I.X., 2022. PAR and UVA composition of global solar radiation at 72a high altitude Mediterranean forest site. *Atmospheric Research*, 269, 106039

Wild, M., Gilgen, H., Roesch, A., Ohmura, A., Long, C.N., Dutton, E.G., Forgan, B., Kallis, A., Russak, V. and Tsvetkov, A., 2005. From dimming to brightening: Decadal changes in solar radiation at Earth's surface. *Science*, 308(5723), 847-850.

Yuan, M., Leirvik, T. and Wild, M., 2021. Global trends in downward surface solar radiation from spatial interpolated ground observations during 1961–2019. *Journal of Climate*, 34(23), 9501-9521.

- The results of manipulation in carbonate systems should be described first. The readers need to know actual pH reduction, pCO₂ and CO₂ increase. The 0.4 pH reduction, even if this is identical for all experiments, would result different pCO₂ increase depending on ambient temperature, salinity, and carbonate system.

Response: We have supplied relevant description and it reads "A series of onboard CO₂-enrich experiments in the investigated regions were conducted during three cruises. In the high CO₂ treatments, pH_{total} had a decrease of 0.34-0.43 units, while pCO₂ and CO₂ had an increase of 676-982 μatm and 17-25 μM kg⁻¹ SW, respectively (Table S1). Carbonate chemistry parameters after 24 h of incubation were stable (ΔpH < 0.06, ΔTA < 53 μmol kg⁻¹ SW), indicating the successful manipulation (Table S1)" at line 215.

- The samplings of the bottom of upper mixing layers are not described in Materials and Methods section.

Response: This part has been removed based on the comments of reviewer #1.

- Regardless of the importance of NO_x availability in this study (L220), spatial NO_x

distribution was not shown. Furthermore, NO_x 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?

Response: We have realized this issue and removed the data of NO_x.

- The paper shows nutrient data only for NO_x. We would like to know phosphate, silicic acid, ammonium, and probably iron levels.

Response: We agree with the reviewer that other nutrient elements may also affect the effects of OA on primary productivity. However, due to the limit of manpower and finance, other nutrient elements were not measured because most of them have very low levels in surface seawater of the South China Sea, requiring advanced instruments and well-trained technicians.

- 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.

Response: Honestly, we are not sure which part is inconsistent with the discussion on a negative effect on diatoms in L237-238. In this study, OA showed positive, neutral and negative effects on primary productivity. We discussed the possible reasons that resulted in different effects. To clarify it, we have added some words and it reads now "Therefore, the net impact of OA depends on the balance between its positive and negative effects, leading to enhanced, inhibited or neutral influences, as reported in diatoms (Gao et al., 2012, Li et al., 2021) and phytoplankton assemblages in the Arctic and subarctic shelf seas (Hoppe et al., 2018), the North Sea (Eberlein et al., 2017) and the South China Sea (Wu and Gao 2010, Gao et al., 2012). The balance of positive and negative effects of OA could be regulated by other factors, including nutrient, salinity, light intensity, population structure, etc. (Gao et al., 2019)" at line 268.

- The "harmful algal blooms" is sudden and unacceptable, not discussed in the Discussion section.

Response: It has been removed.

- I am not sure whether the unit of total alkalinity is $\mu\text{mol/L}$ and that of PAR is $\text{W/m}^2/\text{s}$.

Response: As responded above, we have changed the unit of TA to $\mu\text{mol kg}^{-1} \text{SW}$ and hope to keep the unit of PAR as $\text{W/m}^2/\text{s}$.

- 5 caption. L534-536. This is described only for this figure and can be removed because of the repetition of the Result section.

Response: It has been removed as suggested.