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Reply on RC1

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

The authors present a comprehensive set of short-term carbonate chemistry manipulations of natural phytoplankton communities from coastal and pelagic stations acquired during 3 different cruises. The manuscript is generally well written and clearly structured, and presents a large dataset. I have, however, major concerns about the interpretation of the data.

Response: We appreciate these comments very much, particularly for understanding the difficulty in obtaining a large number of data through in situ investigation and deck-based incubation covering a large area.

Firstly, the data is interpreted as if these were acclimated OA responses and not short term low pH assays investigating instantaneous CO₂/pH effects. These are two very different things than doing acclimated OA experiments, and show the response of the physiological machinery under current CC to short-term elevation on pCO₂, so an interpolation to future OA is invalid. Also, cells are probably stressed by change in environment, especially as 100% incoming light was used in this study, which is really high. Both of these aspects need to be made clear in abstract, discussion and conclusion.

Response: We have revised the manuscript based on the two points. Please see the specific response below.

My major concern is related to your interpretation of nutrient effects. While I would agree with your general hypothesis, this dataset is not at all suited to show and conclude this: In the methods section, the analytical limits for the nitrate and nitrite measurements are given as 0.1 and 0.04 μmol L⁻¹, respectively (L112-113), adding up to 0.14 for NO_x. This value is considerably higher than most of the NO_x values shown in Figure 6, potentially even higher than all except 2 or 3 values. It is not clear to me whether these are the surface values, or the ones from the bottom of the upper mixed layer the authors refer to in the results section (L210-211). Regarding the former, this is an invalid approach. The samples you have collected and experimented with are from surface waters, so you cannot use the nutrient values from the bottom of the mixed layer as a reference. The latter have nothing to do with the physiological rates occurring in your short-term incubation assays! Also, you cannot conclude that the regional differences you observed

are driven by nutrients if you have only sampled for nutrients on one of the three cruises (L115-117). Accordingly, large parts of the discussion and conclusions have to be completely re-written, and the other environmental drivers (e.g. PAR and salinity effects, that are way more significant according to Table S1!) have to be considered more carefully.

Response: We agree with the reviewer that the nutrient values from the bottom of the mixed layer cannot be used to explain primary productivity in surface seawater. We have thus removed those data. As suggested, we have re-written the discussion and conclusions, focusing the effects of pH, PAR and salinity. Please see relevant sections for details.

Seasonality vs regionality: In L92-93 the timing of the three cruises is mentioned. While the Taiwan Strait cruise occurred in July, the other two cruises took place in September. These differences in the seasonal timing of sampling are not at all considered in the interpretation and discussion of the results. This should be included and discussed. Specifically, to which extent could the regional differences you find be caused by the differences in timing of sampling? Also the term 'region' and the separation between nearshore vs pelagic is not clear, you need to be more specific on how these were defined.

Response: We have added the discussion of seasonality and it reads "It is worth noting that seasonality may also lead to the differential effects of SA on primary productivity since the Taiwan Strait cruise was conducted in July and the cruises of the South China Sea basin and the West South China Sea were conducted in September. The SST and solar PAR intensity of the Taiwan Strait in July was 2-3 °C and $22 \pm 22 \text{ W m}^{-2} \text{ s}^{-1}$ higher than that in September (Zhang et al., 2008, 2009; Table S3). Although the effects of SA were not related to temperature as shown in this study (Table S2), the higher solar radiation in July may contribute to the positive effect of SA on primary productivity" at line 381. We have also redefined the "region" as suggested, we now use the terms of "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)" instead of nearshore and pelagic at line 103.

Zhang C, Zhang X, Zeng Y, Pan W, Lin J. Retrieval and validation of sea surface temperature in the Taiwan Strait using MODIS data. *Acta Oceanologica Sinica*, 30:153-160, 2008.

Zhang C, Ren Y, Cai Y, Zeng Y, Zhang X. Study on local monitoring model for SST in Taiwan strait based on modis data. *Journal of Tropical Meteorology*, 25:73-81, 2009.

Currently the carbonate chemistry data from the incubations is missing, so that it is impossible to judge if the treatments were successful. Without them, an interpretation of the results is not possible.

Response: We have added these data and the relevant description 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 209.

Specific comments

L1-3: include inf that this is short-term exposure and not acclimation into the title

Response: The title has been revised to "Contrasting responses of phytoplankton productivity between coastal and offshore surface waters in the Taiwan Strait and the South China Sea to short-term expose of seawater acidification"

L25-28: As explained above, this statement is not valid

Response: The ocean acidification has been corrected to seawater acidification throughout the text.

L28-32: I don't think you can make such general statements on long-term OA effects based on 24h incubations that did not allow for any acclimation. This need to be rewriting accordingly.

Response: It has been corrected to "Contrasting responses of phytoplankton productivity in different areas suggest that SA impacts on marine primary productivity are region-dependent and regulated by local environments" at line 30.

L36-38: I suggest including that this process is ongoing and likely intensifying.

Response: Corrected.

L102ff: In Biogeosciences, total alkalinity us usually abbreviated as A_T. Please use this term throughout the manuscript.

Response: We presume that the reviewer's suggestion is to change "TALK" to "TA". We have corrected it.

L126-129: A lot more info needs to be given regarding the methods of OA manipulation. Was the CO₂-saturated sweater taken from the same location as the sample? If yes, how much time passed between sampling and start of incubation? How was the carbonate system manipulated (e.g. TA or DIC manipulation?), the decrease of pH units by approx. 0.4 units sounds like a unprecise approach. You need to at least provide a table in the appendix with pH values at the start and the end of each incubation to prove that your OA treatments were successful, ideally a fully constrained carbonate system with measured TA and DIC values.

Response: As suggested, the text 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. Seawater was incubated within half an hour after they were collected" at line 143. We have also supplied a table of carbonate system as suggested. The relevant description 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.

L133: 100% incident irradiance is really high, as samples don't get mixed down in an

incubator. Please consider and discuss if OA effects may be driven by high light stress in those incubations with high PAR intensity.

Response: We have added this point and it reads "It is worth noting that the samples were not mixed down in the water bath and the 100 % incident solar irradiances may have high light stress on cells. Lower incident solar irradiances or some devices can be used to simulate seawater mixing in future studies" at line 303.

L133-134: On many ships, underway seawater supply still ends up being considerable warmer than SST due to the water running through a ship. Can you provide measurements of incubator temperature and offset to SST?

Response: That is true. The following information has been supplied "Due to heating by the deck, the temperatures in the water bath were 0-2 °C higher than in situ surface seawater temperatures" at line 155.

L178: Results from nutrient measurements are missing

Response: Nutrient levels in the surface water at most stations were undetectable. That is why we used the values at the bottom of upper mixing layers. We removed the data of NO_x since it is invalid to use them in explaining PP of surface seawater.

L192-193: please adjust to 'we observed that instantaneous effects of elevated pCO₂'

Response: Corrected.

L194: not sure which regions were compared. Please clarify.

Response: This is the general description of OA effects in all investigated regions. It has been clarified to " It was observed that instantaneous effects of elevated pCO₂ on primary productivity of surface phytoplankton community in all investigated regions ranged from -88% (inhibition) to 57% (promotion), revealing significant regional differences (ANOVA, $F_{(100, 404)} = 4.103$, $p < 0.001$, Fig. 5). Among 101 stations, 70 stations showed insignificant SA effects. SA increased PP at 6 stations and reduced PP at 25 stations" at line 220.

L197: 'was' should read 'were'

Response: Corrected.

L199: should read 'approaching the Mekong River plume' and 'A reduction'

Response: Corrected.

L203-206 and elsewhere: Again this region-related effect is not 100% clear, be more precise here, please. How did you define pelagic vs. near-shore, and is this the same definition you always use when talking about regions?

Response: We have redefined the regions and it reads "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)". Therefore, the text here was revised to "Overall, the elevated pCO₂ had neutral or positive effects on primary productivity in the continental shelf and slope regions, while having adverse effects in the deep-water basin" at line 103.

L210-211: As explained above, this NO_x statistics approach is not valid

Response: We have removed the NOx data as suggested.
L219-220: Based on your data this statement cannot be made!

Response: We have removed the information of nutrient and it reads now "Our results suggested that the enhanced effects of the SA treatment on photosynthetic carbon fixation depend on regions of different physicochemical conditions, including pH, light intensity and salinity" at line 247.

L234: should read 'reducing the removal rate'

Response: Corrected.

L237: RuBisCO abbreviation written differently in introduction. Please make consistent

Response: Corrected.

L245-276: all of this needs to be removed as you cannot conclude anything on nutrient effects based on your dataset.

Response: We have removed all of this as suggested.

L295-299: This statement cannot be made based on your dataset, rewrite following your actual data.

Response: We have rewritten it that reads "By investigating the impacts of the elevated pCO₂ on PP in the Taiwan Strait and the SCS, we demonstrated that such short SA-treatments induced changes in PP were mainly related to pH, light intensity and salinity based on Pearson correlation coefficients" at line 376.