

Biogeosciences Discuss., referee comment RC1  
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## **Comment on bg-2021-326**

Anonymous Referee #1

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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-RC1>, 2022

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**Review on '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' by Guang Gao and coworkers under review for Biogeosciences**

### **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.

Firstly, the data is interpreted as if these were acclimated OA responses and not short-term low pH assays investigating instantaneous CO<sub>2</sub>/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 pCO<sub>2</sub>, 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.

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  $\mu\text{mol L}^{-1}$ , respectively (L112-113), adding up to 0.14 for  $\text{NO}_x$ . This value is considerable higher than most of the  $\text{NO}_x$  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 (LL210-211). 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.

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.

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.

### **Specific comments**

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

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

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 needs to be rewritten accordingly.

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

L102ff: In Biogeosciences, total alkalinity is usually abbreviated as A<sub>T</sub>. Please use this term throughout the manuscript.

L126-129: A lot more info needs to be given regarding the methods of OA manipulation. Was the CO<sub>2</sub>-saturated seawater 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.

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.

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?

L178: Results from nutrient measurements are missing

L192-193: please adjust to 'we observed that instantaneous effects of elevated pCO<sub>2</sub>'

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

L197: 'was' should read 'were'

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

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?

L210-211: As explained above, this NOx statistics approach is not valid  
L219-220: Based on your data this statement cannot be made!

L234: should read 'reducing the removal rate'

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

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

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