Comments to the author:
Dear Prof. Gao,

We have received two reviews of your revised manuscript from the referees who reviewed the former version of your manuscript. Both referees were pleased to find significant improvements, but they still have some concerns (see below). So we would appreciate it very much if you could make a further revised manuscript following their helpful comments.

Thank you again for your excellent efforts to improve the manuscript.

Kind regards,

Koji Suzuki
Associate Editor

Response: We appreciate the Associate Editor and two anonymous reviewers very much for their help in improving our paper. We have further revised our manuscript based on the reviewers' comments.

- Referee #1

General comments
The paper is significantly changed in response to review comments. Although the dataset shown in this paper is valuable, the authors fail to show a powerful conclusion. The paper still draws attention in this field's researchers but may not be enough for the journal Biogeosciences.

Major comments
1. Conclusions, L313. "short SA treatments induced changes in PP were mainly related to pH, light intensity and salinity" Yes, Fig. 6 shows us this. However, I consider that these are not causal relation. Phytoplankton community structures will determine SA induced changes as discussed in Discussion section. In this regard, the present conclusion may be weak.

Response: We agree with that. The following sentence has been added "In addition, phytoplankton community structures may also modulate SA induced changes".

Specific comments
2. L20. In the present abstract South China Sea is not needed to be abbreviated because the term is used only here.

Response: South China Sea and SCS were interchangeably used in the text. To be consistent, we have changed all SCS to South China Sea.

3. L60. In this manuscript abbreviations are not well managed. For example, the "SCS" appears first time here in the main text. SCS is defined firstly in line 67 and secondly

in line 72.

Response: Please see the response above.

4. L78-79. Ocean acidification --> SA?

Response: Corrected.

5. L175 and 196. uM/kg --> umol/kg

Response: Corrected.

6. L265-268. SA “increased” photosynthetic carbon fixation of three diatoms under lower light intensities but “increased” it under higher light intensities.

Response: It has been corrected to "SA increased photosynthetic carbon fixation of three diatoms (*Phaeodactylum tricornutum*, *Thalassiosira pseudonana* and *Skeletonema costatum*) under lower light intensities but decreased it under higher light intensities" at line 270.

7. L275-276. “100 % incident solar irradiances may have high light stress on cells”, but PP was enhanced under higher PAR? Are these consistent?

Response: We added this statement as required by a reviewer. It does seem inconsistent with the results. We have revised it to "It is worth noting that the samples were not mixed down in the water bath in the present study and exposed to 100% incident solar irradiances. Lower incident solar irradiances or some devices can be used to simulate seawater mixing in future studies" at line 281.

8. L337. Some papers in the list are not referred to in the revised text.

Response: We appreciate the careful review of the referee. We have double checked the references and made corresponding corrections.

9. Fig. 2 caption. The unit for TA is still umol/L. The scale of pH should be shown.

Response: Corrected.

10. Fig. 5 and its caption. The authors mainly use the term seawater acidification rather than ocean acidification in the revised manuscript. Here also should be the case. Similar is the case for Fig. 6 and its caption. In Fig. 6 salinity should be non-dimensional.

Response: Corrected.

- Referee #2

The authors have done a very good job addressing my and the other reviewers concerns and criticism, so that the manuscript improved a lot in the revised version. There are just a few minor edits I would suggest.

Response: We appreciate this comment and have further revised the manuscript as suggested.

L28: should read ‘vulnerable to a drop’

Response: Corrected.

L57: The differences of SA relative to the before mentioned OA needs to be explained/made explicit, it is currently not clear that the authors use SA for short-term changes in carbonate chemistry

Response: The text has been clarified to "In addition to the slow change of ocean acidification, some processes, such as freshwater inputs, upwelling, typhoon and

eddies, can lead to instantaneous CO_2 rising and short-term changes in carbonate chemistry, termed seawater acidification (SA) (Moreau et al., 2017; Yu et al., 2020)" at line 41.

L78-83: Jumping back and forth between OA and SA, this needs to be made consistent, or explicit (i.e. if referring to different time scales)

Response: Corrected.

L126: should read 'reached values around 4.50'

Response: Corrected.

L127-130: the abbreviations AC and HC need to be explained and written out when they first appear

Response: Corrected.

L129: should read 'Samples were incubated'

Response: Corrected.

L188: should read 'productivity ranged from 99 to 302'

Response: Corrected.

L189: should read 'from 17 to 306'

Response: Corrected.

L195: please always use the same names for your treatment throughout the manuscript

Response: Corrected.

L195-196: should read 'pH_total decreased by […], while pCO2 and O2 increased by'

Response: Corrected.

L199-202: similar to the first version of this manuscript, it is still not clear which regions have been compared for the statistical test. Please clarify that you compare continental shelf, slope and deep-water basin stations here

Response: It has been clarified to "It was observed that instantaneous effects of elevated pCO_2 on primary productivity of surface phytoplankton community in all investigated regions ranged from -88% (inhibition) to 57% (promotion), revealing significant regional differences among continental shelf, slope and deep-water basin (ANOVA, $F_{(2, 98)} = 3.747$, $p = 0.027$, Fig. 5)" at line 202.

L208: space missing in 'to14'

Response: Corrected.

L214-217: In a linear regression analysis, the p value only indicated whether the slope of the fit is different from 0, but does not give any indication on how good the fit is. R values of 0.3-0.4 are low, and the fact that most of the data points in figure 6 lay outside of the confidence intervals nicely illustrates this. Therefore, I would not trust these SA-effects too much. See later comment.

Response: We agree with this.

L253: 'limited' should read 'limiting'

Response: Corrected.

L261-262: should read 'The nutrient levels in the basin are usually lower than on the shelf'

Response: Corrected.

L264: should read ‘with increasing light intensity’

Response: Corrected.

L264-268: Given the weak correlation (r=0.31) I would not overinterpret this observation

Response: We agree with that. This point has been underlined and the text reads " Meanwhile, the weak correlation (r = 0.311) between light intensity and SA effect suggests the deviation from linear relationship in the context of multiple variables needs to be further illuminated in future studies" at line 279.

L277: should read ‘A negative correlation’

Response: Corrected.

L283-286: I don’t fully understand this sentence, this needs to be formulated more clearly. Are you trying to say that the correlation with salinity seems to be an autocorrelation between salinity and insitu pH? If yes, please say so explicitly. This sentence also needs some grammar editing.

Response: The text has been clarified to "In this study, the negative relationship between salinity and SA effects seems to be an autocorrelation between salinity and in situ pH (Fig. S1) because lower salinity occurred in coastal waters where seawater pH was higher while the basin zone usually had higher salinities and lower pH" at line 291.

L293-294: should read ‘with high abundances of phytoplankton, which is consistent’

Response: Corrected.

L303-306: it is not clear what you mean with the term ‘seasonality’, as you are now discussing some of the environmental variables in more details before. I think species succession should be explicitly mentioned

Response: One reviewer asked us to discuss the effect of seasonality, because the Taiwan Strait cruise was conducted in July while the cruises of the South China Sea basin and the West South China Sea were conducted in September, which may be classified as summer and autumn, respectively. As suggested, we have also discussed the species succession and it reads "In addition, species succession of phytoplankton with season may also affect the response to SA (Xiao et al., 2018)" at line 320.

L318-320: it needs to be made explicitly mentioned that your predictions only hold true if responses to very short-term pH changes are representative for responses to long-term OA trends. I also find these sentences a bit to broad, I don’t think you can claim based on your data that ‘PP in coastal waters would be increased’, there are e.g. a lot of unaffected stations. These statements need to be toned down significantly.

Response: We agree with the reviewer. These statements have been toned down to "The negative effect of SA in basin zones may further reduce primary productivity. Meanwhile, PP in some coastal waters may be increased by SA" at line 330.