Reply on RC1

Yuan Dong et al.

Author comment on "Biophysical controls on seasonal changes in the structure, growth, and grazing of the size-fractionated phytoplankton community in the northern South China Sea" by Yuan Dong et al., Biogeosciences Discuss.,
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Response to Anonymous Referee #1

General comments

- The manuscript reports a study of size-fractioned phytoplankton growth and grazing rates in the northern South China Sea. The authors used the well established dilution method to measure community grazing rates and growth rates of the different size fractions --- micro-, pico- and nano- components of the phytoplankton. The results were then discussed in the context of the different environmental parameters.

The experiments appear to be done and the data analyzed carefully. The amount of work involved is quite impressive, and it generates quite interesting insights into the dynamics of the different phytoplankton size fractions in the region. It is a valuable contribution to the basic biological oceanography of the northern South China Sea.

Response: Thanks to the reviewer for positive comments on this work.

Specific comments

- Introduction: The hydrographical conditions described are not unique to NSCS. To give the paper a broader appeal, perhaps the authors can explain better the ecological or biogeochemical significance of the studied area?

Response: We thank the reviewer for the good suggestion. We have rewritten the relevant part of the introduction section to better present the ecological and biogeochemical significance of our study area.

- Likewise, the background biological information seems lacking. The authors only briefly cited a few papers on diatom blooms, nutrient limitation and microzooplankton grazing, but no details are provided. It would be helpful to say more about the plankton community in the area (if known), and give a stronger justification (than just "it remains largely unknown…") how this study can improve our understanding of the area in a meaningful way.

Response: We thank the reviewer for this suggestion. We have provided the detail biological background of the Wanshan station to the revised manuscript. The relevant sentences are rewritten as "Previous studies have suggested that phytoplankton community in the coastal waters near Wanshan was dominated by diatoms with intense blooms occurring in response to strong eutrophication (e.g. Li et al., 2013). The dominant diatom species here were *Skeletonema costatum* in the summer and *Eucampia zoodiacus* in the winter. There were also intense grazing of phytoplankton by microzooplankton (mainly ciliates and dinoflagellates) and mesozooplankton (mainly copepods) (Chen et al., 2015)".

- Materials and Methods: Initial Chl-a was estimated based on the dilution factor, instead of direct measurements (line 135). This seems rather dubious for a study that so critically depends on accurate Chl-a measurements for calculating growth rates and grazing rates. Can the authors provide any ancillary data to confirm the reliability of their estimation?

Response: Actually, we have conducted experiments to verify that the calculated initial Chl-a concentration was not significantly different from that of the direct measurement (t=0.5, n=22, p=0.31). We have clarified this in the revised manuscript.

- While it is commendable that the authors used 5 dilution levels (line 127), can the authors confirm how well the data points fit on a linear regression for calculating grazing rates (line 145)? Perhaps the authors can display the actual "apparent growth rate vs. fraction seawater" and the corresponding statistics in supplementary?

Response: Actually, the statistic details (including R-value and p-value) of the linear regression between apparent growth rate and dilution factors for each experiment had been shown in the supplementary Table S2.

- Results: Please include and explain the "nutrient limitation index" (line 252) in the Method section.

Response: Done. We have added the detail description of "nutrient limitation index" to the revised manuscript.

- Line 322: "The negative effect of… salinity and nutrients." This part is a bit confusing; please revise.

Response: Done. The sentence has been rewritten as "A negative correlation of salinity with the abundance of smaller zooplankton in the Jiaozhou Bay of the South Yellow Sea has been attributed to the discharge of eutrophic freshwater (Wang et al., 2020). This may likely be also true at the Wanshan station given the negative correlation between salinity and nutrients. The input of low-salinity/high-nutrient water stimulates phytoplankton growth and thus the

growth of small zooplankton grazing on them".

- Conclusion: Line 359: Perhaps change "in the ocean" to "in the studied area". After all, the measurements are limited to a rather small area.

Response: Done.

- Presently, the data are discussed rather narrowly within the confine of data patterns and trends, but it is missing what the data tell us about the bigger picture. In the conclusion, the authors state "our findings… ocean biogeochemical modeling… carbon fluxes… microbial food web… future environmental and climate change" (line 329). Related to my comments about the Introduction section, it would strengthen the manuscript if the authors introduce some of these issues in the Introduction, then discuss the results in these context in the Discussion. I believe, doing so will elevate the overall quality and significance of the paper.

Response: We thank the reviewer for the constructive comments. We have rewritten the relevant parts of the introduction and the discussion sections according to his/her suggestions to provide a broader discussion on the impacts of our findings on the general carbon cycle and ecosystem dynamics of the shelf-sea.

- Technical corrections: Overall clearly written, notwithstanding a few minor typos or grammatical errors.

Response: The manuscript has been proofread by a native English speaker and the typos and errors have been corrected in the revised manuscript.