

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-226

Anonymous Referee #1

Referee 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.,
<https://doi.org/10.5194/bg-2021-226-RC1>, 2021

General comments

The manuscript reports a study of size-fractionated 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.

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?
- 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.

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?
- 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?

Results:

- Please include and explain the “nutrient limitation index” (line 252) in the Method section.
- Line 322: “The negative effect of... salinity and nutrients.” This part is a bit confusing; please revise.

Conclusion:

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

Technical corrections

Overall clearly written, notwithstanding a few minor typos or grammatical errors.