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Discussions

Interactive comment on "Seasonal and spatial comparisons of phytoplankton growth and mortality rates due to microzooplankton grazing in the northern South China Sea" *by* B. Chen et al.

Anonymous Referee #1

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The MS by Chen et al. investigates de trophic impact of microzooplankton on the phytoplankton communities of South China Sea, comparing the grazing effects on the vertical scale, along a shelf to open ocean gradient, and during two contrasted seasons. I should say the authors presented a comprehensive and thorough study. However, as in any study there are always things to be criticized and others to be improved.

The MS addressed several hypothesis, such as the higher microzooplankton grazing impact in oligotrophic than in eutrophic waters, the enhancing effect of temperature on microzooplankton grazing activity and impacts, and the higher relevance of microzooplankton grazing at depth, compared with surface waters. These hypotheses are

based on previous knowledge and ecological theories, as explained in the introduction. However, some of the concepts are in my opinion incorrect, or at least debatable.

Hypothesis (1) higher microzooplankton grazing impact in oligotrophic areas: As already stated in the introduction evidences exit to prove this is not the case (see review by Calbet and Landry 2004, among others); therefore, there is no actual argument to write this hypothesis. As a matter of fact, the data of the authors corroborates there are no differences in the grazing impact along a trophic gradient.

Hypothesis (2) temperature effect: The authors claim there should be a higher grazing impact ($m/\mu o$) in the warm summer than in winter because the different temperature growth coefficient for phytoplankton and microzooplankton growth (Rose and Caron 2007). Actually, the cited paper embraces a rather large gradient of temperatures, observing the theoretically major differences above and below 15°C, temperature at which maximal growth rates of herbivorous protists equaled or exceeded maximal growth rates of phototrophic protists. The average temperatures for the seasons studied here spanned from 21.3 to 29.7°C, range in my opinion too narrow to observe any effect on well-adapted communities. As expected the authors did not find such effect, even presenting evidences of the opposite. I suspect a methodological artifact here that I will discus below.

Hypothesis (3) grazing impact should be greater at depth than in surface waters: This hypothesis is based on the light dependence for phytoplankton growth. The hypothesis seems to be essentially correctly articulated; however, it ignores that the biomass of microzooplankton does not have to be necessarily evenly distributed along the vertical column, and the growth inhibitory effects of light at surface.

Therefore, I recommend the authors to readdress their hypotheses in a more convincing way. Furthermore, these hypotheses should be developed properly, not as questions.

Specific comments

Methods I see in the methods the authors used 5 non-replicated dilution levels in 1.2 L bottles, being the most diluted level 15% of natural seawater. For oligotrophic waters, such small volume may not capture correctly the variability of grazers, especially in the diluted treatments. This could be at least corrected for by using replicates, which seems was not the case. It is true, most of the rates obtained in the study are based on high regression coefficients, but we do not have information of the significance of any of these regression lines. I ask the authors to comment on this and to include significance levels.

Units: in many occasions L and m3 are mixed in the same line. I would recommend choosing one.

The incubations were conducted at surface temperature, irrespectively of the depth the water was collected from. This should not represent a problem in winter, when the water column was mixed, but could represent a severe thermic shock for the communities inhabiting in these waters in summer. Perhaps this can explain the reduced m/Bz at DCM in summer. Please, comment on that.

Include details on how the microzooplankton samples were collected.

Regarding the latter, the authors write" dinoflagellates known to have phagotrophic ability (such as Gyrodinium, Protoperidinium) were included in the biomass of microzooplankton". I understand there is a need for tracing a line to distinguish phytoplankton than microzooplankton. However, the phagotrophic capacity may not be the right one, given most (if not all) of the dinoflagellates may have phagotrophic abilities. If the authors consider only the two previously indicated genus of dinoflagellates, I suggest changing the term phagotroph to heterotroph.

Please, indicate the cases with positive slopes.

I am happy to see the authors made an effort to correct the phytoplankton growth rates for photoacclimation, even if simulating actual light conditions. However, I do not re-

ally see if these corrections were applied to μo, and what was their magnitude. This information is required. As a matter of fact in the results there are extremely high phytoplankton growth rates (e.g., S9, 10 summer, surface, among others), considering the chlorophyll biomass, the nutrient (only nitrate is reported) availability, and the biomass of grazers that could be actively recycling inorganic nutrients. Basic mass balance calculations show that is rather unrealistic to expect such growth rates. Please, comment on this. Please, explain the reasons for choosing the mixed layer definition.

Results

Include DCM data on table 1 as well.

When presenting table 1 refer to the appendix to show where the actual data are.

Define stratification index.

I suspect a mistake in the p value at page 16012, line 9 (p should not be < 0.05 if insignificantly different). I do not understand the last sentence on page 16012

Discussion

Please, rephrase first 5 lines of the discussion to improve for clarity and style.

It makes sense light limits phytoplankton growth. However, I also wonder what would be the consequences of the thermal shock indicated above.

Chapter 4.3. I do not really see the point when referring to phytoplankton size-structure. Were are the data? Besides, I urge the authors to carefully read this section, and others, to revise the differences between m and m/μo. It seems both concepts are mix-up and they are, obviously, very different.

In the same chapter. Introduce better the study by Liu et al (2002).

Page 16018 line 20. Please, make some basic calculations to back up microzooplankton recycling is enough to sustain the observed phytoplankton growth rates.

END OF REVIEW

Interactive comment on Biogeosciences Discuss., 9, 16005, 2012.