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Interactive comment on "Seasonal and size-dependent variations in the phytoplankton growth and microzooplankton grazing in the southern South China Sea under the influence of the East Asian monsoon" *by* L. Zhou et al.

Anonymous Referee #1

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General comments:

This paper is an ordinary study that reports the results of dilution experiments and discusses every point relevant with the results. The study area is interesting and was indeed not often investigated before. There are some of the weak points that I need to address before recommending for publication for 'Biogeosciences'. The data themselves are certainly useful (although some of the nutrient and growth rate data seem weird as I discuss below). But, unfortunately, there are no data for microzooplankton biomass. The approach of analysis is crude and breaks down sometimes (certainly

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needs improvement). Some of the discussions are interesting, but some are unfounded and highly speculative. I would suggest narrowing down to one or two main points.

One weird result is the relatively low m/μ values in such an oligotrophic and tropical environment. It is not very likely that mesozooplankton grazing consumed the rest of the primary production left by microzooplankton, given that the majority of the phytoplankton are smaller than 3 μm (The authors' Table 2). Direct sinking is not possible either, also because of the small size of the phytoplankton. Of course, it is possible that μ can highly exceed m in non steady-state conditions. But for the long run, growth must largely balance mortality (including microzooplankton and mesozooplankton grazing, viral lysis, direct sinking, etc.).

One potential cause is that the authors had inadvertently overestimated the growth rates of the phytoplankton even in nutrient non-amended bottles. This can occur for two reasons. The first is that the light level was not well controlled. In the "Materials and Methods" section, the authors have indicated that "All of the bottles were incubated for 24 h in a deck incubator cooled by running surface seawater and covered with neutral-density screens to simulate in situ light regime. These measures have been proved effective to avoid phytoplankton photoacclimation during the incubation (Zhou et al., 2015a).". However, the authors did not report how they estimate the "in situ light regime", which is not so easy to estimate if one needs to take into account the surface irradiance, the depth of surface mixed layer, the light attenuation coefficient and the mixing turnover time in the water column, etc.. The authors did not provide any of the information in the paper. Because all the factors may vary day-to-day, the carbon-to-chlorophyll ratios of the phytoplankton cells could change even if the simulated light environment perfectly matches the "in situ" condition and the growth rates could be estimated with biases (if not errors). The experiments in Zhou et al. (2015a) were done in different areas and at different times and could not be used to justify the results in the present study. It is a bit weird why the authors did not do similar checks on the cellular fluorescence in this study. The second possibility for overestimating the growth

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rates is that inadvertent nutrient contamination in the incubation bottles in oligotrophic waters. This is hard to verify, but this possibility cannot fully ruled out.

My another concern is on the nutrient data. From Table 1, my first impression is that this area might be a "high-nitrate-low-chlorophyll" (HNLC) region! If it were true, this could be a big issue since, to my limited knowledge, iron limitation has not been reported in this area. My first response for Table 2 is that: could the authors mistakenly swap the N column with the P column since the P concentrations were so high? The authors need to double check these data.

I would argue that the RPI index cannot be used to infer whether microzooplankton grazing contributes to the dominance of picophytoplankton in oligotrophic waters, because the RPI index does not include growth rate. The variations of phytoplankton biomass are determined by both growth and loss (including grazing, sinking, etc.) rates. Higher m on larger phytoplankton does not directly lead to the dominance of smaller phytoplankton. In eutrophic waters, we can also observe higher m and RPI index on larger phytoplankton, which does not necessarily indicate the dominance of picophytoplankton in eutrophic waters. It is simply because larger phytoplankton also grow faster.

Particularly, one point that needs to be addressed is that, when inferring the mechanisms controlling the growth and grazing on phytoplankton, one must bear in mind that correlation does not lead to causation. There are so many factors that may affect the growth and grazing of phytoplankton. It would be misleading to attribute most of the variations to one or two environmental factors (e.g. rainfall) only based on correlation.

Finally, the English writing also needs to be improved.

Specific comments: 1. Abstract P. 6286, line 6-9. I am a little confused by this sentence. Does this mean m/μ did not vary significantly between the two seasons?

2. Introduction

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P. 6287, line 2-3, change to: “Phytoplankton provide almost . . .” Line 15, change “indicates” to “induces” Line 17-19, in fact, the microzooplankton studies in tropical oceans are not so few. Please also see Landry et al. DSR II 1995 and Quevedo & Anadón MEPS 2001.

P. 6288, line 1, delete “phytoplankton” and change to “pico-sized prey”.

3. Materials and methods

P. 6290, line 1-2, the authors did not set up a dilution level below 20%. It is recommended by some authors (e.g. Gallegos 1989; Strom and Fredrickson 2008) to use a highly diluted bottle to deal with the possible grazing saturation.

4. Results P. 6293, the last paragraph. Please take into account the standard errors of each μ and m measurements when comparing the large size and small size fractions. I would guess many of the differences were insignificant.

P. 6294, line 14, change ‘exclude’ to ‘excluding’.

Line 16, “Taking all the data. . .”.

5. Discussion P. 6300, line 1-3. What does this mean? Does it mean that the physiological effect of temperature is strong in the SSCS? But in the text above, you already wrote that the temperature variation was small.

Table 4: The correlations between μ and m/μ (and μ/μn, μ–m) make little sense since these variables are not independent with each other.

Interactive comment on Biogeosciences Discuss., 12, 6285, 2015.