Reviewer #1

It is obvious that the authors have made great efforts in addressing the reviewers' comments. Although this study still has some (serious) limitations, it might be unfair to demand every point to be satisfactorily addressed. However, I think that there are two points that the authors should address before acceptance:

1) The nutrient data are really weird. You cannot publish these problematic data without good explanation on BG.

2) It is not difficult to calculate the standard errors of growth and grazing rates of large and small phytoplankton. These standard errors must be taken into account for comparing the rates of different phytoplankton.

In this manuscript Zhou et al. use the dilution technique to estimate the phytoplankton growth and the microzooplankton grazing rates in the SSCS zone. They compare the rates between seasons and phytoplankton size fractions. The data are interesting and offer value to the scientific community; there are few studies that simultaneously analyze the seasonal variation of phytoplankton growth and microzooplankton grazing rates. However, I think that some aspects should be modified for the publication of the manuscript in Biogeosciences, especially in the discussion section, which includes some unlikely or wrong explanations and argumentations. Below are few suggestions that may improve the quality of the manuscript.

Introduction

The authors should provide a more complete description of the differences in the sea-water properties (salinity, mixed layer depth, etc.) between winter and summer. The analysis of the differences between both seasons is one of the objectives of the study and is one of the main topics in the discussion. The authors should include at least one sentence after P. 4, line 15, and provide some references. Longhurst (2007) defined a biogeochemical province for an area including the area of study. This reference could be useful in case there were not other specific articles. The following lines (P. 4, line 19- P. 5, line 2) do not provide any relevant information for understanding the system; they seem to be only a tribute, certainly deserved, to the scientists who previously studied that area.

Material and methods

Just out of curiosity, why the authors prepare different dilution treatments depending on the season?

P. 6, lines 5-6: The use of light filters do not avoid the occurrence of photoacclimation, which could occur due to the variation between days in the cloudiness and the incident light, or associated to the incubation at a constant irradiance (phytoplankton suffer vertical displacements in the sea).

On P. 6, lines 26-29, the authors indicate that “When saturated or saturated-increasing grazing was observed as a departure from the assumed linear model…”. How were those departures from the linear model detected? If they were estimated by visual inspection of the plots, the authors should indicate it. Some statistical analyses could also be conducted to support the detection of non-linear responses. In this way, Chen et al. (2014) fitted the data using a second-order polynomial. When the second order term was statistically different from zero they determined that the relationship between the phytoplankton net growth rate and the dilution factor was non-linear. Model selection using a first-order and a second-order polynomial to fit the data could be also carried out to check non-linear relationships.

Chen, B., Laws, E. A., Liu, H., & Huang, B. (2014). Estimating microzooplankton grazing half-saturation constants from dilution experiments with nonlinear feeding kinetics. Limnol. Oceanogr, 59(3), 639-644.

P. 7, lines 6-7: The authors indicate that “Net growth rate was also used as a proxy for the actual trophic state of the system being investigated (Calbet et al., 2011)”. Probably I missed something, but I find the citation inappropriate. Calbet et al. (2011) relate the trophic state with the quotient between heterotrophic and autotrophic carbon, but not with the net growth rate.

Results

Vertical profiles of environmental variables during the days in which experiments were performed would be more informative than the values showed in tables 1 and 2. Those graphs should be drawn for salinity (or density), and if it were possible for nutrients, in the seven stations sampled in both seasons (winter and summer). Vertical profiles could be grouped by station and variable (2 vertical profiles in each graph).

Nutrient data are odd. How is it possible a ten-fold increase in phosphate and silicate concentrations in winter accompanied by a ten-fold decrease in nitrate plus nitrite concentration? This should be clarified in the discussion (see below).

Why is the correlation between microzooplankton grazing rate and N or P interesting? In which way nutrients may affect microzooplankton grazing? I would only show it if some explanation would be provided in the discussion.

P. 10, line 15: Give the r^2 for the relationship between μ and m in winter.

Discussion

The authors explain on P. 11, lines 3-5, that "Our measures to mimic the in situ light and temperature during incubation exclude light and temperature from the factors for the substantially negative μ". Were those measurements made in this study? If those measurements were conducted in a previous study the authors should complete the sentence and include the reference.

Section 4.1.1: The authors give several and interesting explanations for the negative phytoplankton growth rates observed at KJ53. One of them is related with the silicate concentration. However, considering that the silicate concentration was higher than 3 µM, I think that it is unlikely that silicates were associated with the negative phytoplankton growth rate. I would remove it.

Section 4.1.2: The authors suggest that "Microzooplankton may reach a maximum ingestion rate at high food concentration, and the maximum ingestion rate may remain constant despite further increase in prey abundance, which is often used to explain the occurrence of saturated feeding responses in dilution experiments (Gallegos, 1989; Moigis, 2006; Teixeira and Figueiras, 2009), and could explain those in our experiments". This explanation is very unlikely taking into account the very low Chla concentration observed in the area of study, as Lessard and Murrell 1998 suggested for the Sargasso Sea. The cited articles (Gallegos, 1989; Moigis, 2006; Teixeira and Figueiras, 2009) describe a situation observed in eutrophic waters, but not in oligotrophic waters. I would delete those lines or at least I would indicate that the explanation was proposed for eutrophic ecosystems.
On the other hand, considering the low Chla concentration observed in the area of study, a potential explanation for the saturated-increased responses could be the one suggested in Lessard and Murrell (1998) based on Gifford (1988) and Gallegos (1989): "If the ambient phytoplankton density is at or near the threshold level where a reduction or cessation of feeding occurs, then further dilution will not result in an increase in net growth rate. This situation would manifest itself as a flat (non-significant slope) dilution curve. If ambient phytoplankton density

is above a threshold level but is diluted below it, the dilution curve would flatten at the highest dilutions (Gifford 1988, Gallegos 1989)".

Lessard, E. J., & Murrell, M. C. (1998). Microzooplankton herbivory and phytoplankton growth in the northwestern Sargasso Sea. Aquatic Microbial Ecology, 16(2), 173-188.

The sections 4.1.3 and 4.2 might be shortened. Although one of the purposes of discussion is to put the obtained results in context, I found them too long and descriptive.

The higher correlation between μ and m for the large phytoplankton size fraction is an interesting result, but I think it is not discussed.

The authors suggest that the slight temperature variation between seasons could not account for the seasonal differences in μ and m. I agree with them. However, the next lines (P. 14, line 25- P. 15, line 1) talking about the temperature effects in the Artic Ocean are from my point of view out of context; they could be deleted.

P. 15, line 7: Change "divers" to "drivers".

P. 15, line 21- P. 16, line 3: The authors describe a decrease in the SSS associated with the rainfall. Despite SSS is shown in tables 1 and 2, graphs showing the vertical profiles would be more informative and would bring consistency to the discussion.

P. 16, lines 14-21: In my opinion the decrease in salinity reported in the present study cannot affect mesozoolankton in the magnitude required to promote the cascading effects mentioned by the authors. The cited articles (Grindley 1964, Zhou et al., 2015b) describe this effect in estuarine and river plumes, where the salinity gradient is more marked. Therefore, the authors should indicate that this salinity effect on mesozooplankton was observed in estuarine waters, but not in the open ocean, unless they could provide any reference. Nevertheless, I recommend removing this section.

Section 4.4: Why P increases whereas N decreases in winter? This is the key issue that the authors should clarify. The discussion is not convincing and fails to address the question. It is easy to understand that vertical mixing increases nutrient concentrations and that stratification promotes nutrient depletion. However, how can vertical mixing increase the P concentration while a tenfold decrease in the N concentration occurs simultaneously? Or, how can the stratification promoted by the differences in salinity be associated with low N and high P concentrations? Why phytoplankton deplete N but not P? Could vertical mixing be important enough to promote the increase in P and Si concentrations taking into account the strong thermocline that possibly exist? (Again, vertical profiles would help to analyze and understand how the system works). Could those high P and Si concentrations be associated with river discharge in winter? On the other hand, the hypothesis about the role of nitrogen fixation could explain an anomalously high N concentration respect to the P concentration in summer, but not the observed seasonal pattern. Have this seasonal pattern been observed in the area of study before? And in other tropical, subtropical or temperate areas? The plotting of vertical profiles with nutrient data would give support to the discussion.

P. 18, line 8: Add "growth" after phytoplankton.

P. 18, lines 13-16: The authors indicate that "…the comings of strong northeast monsoon supply nutrients from deep water to the surface by enhancing vertical mixing. This episodic input of nutrients could break the coupling between phytoplankton and microzooplankton by stimulating μ overwhelming corresponding m (Irigoien et al., 2005)". However, nutrients did not limit the phytoplankton growth during summer, as it is showed by the μ/μ_n ratios, and μ was higher in summer. I would indicate that the input of nutrients could stimulate the growth of phytoplankton groups which are rare in summer, changing the phytoplankton community composition and breaking the coupling between μ and m (especially if there were any article reporting the change in phytoplankton community composition).

P. 19, line 1: Delete one point.

Tables and figures

Table 5: What measurement of variability is shown in the table? Indicate it.

Figure 1: What does the oval drawn with dashed line show? Indicate it in the figure caption. Why the NanSha islands, which are colored in grey in the global map, are not colored in the detailed map?