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Biogeosciences Discussions

Interactive comment on "Coupling of the spatial dynamic of picoplankton and nanoflagellate grazing pressure and carbon flow of the microbial food web in the subtropical pelagic continental shelf ecosystem" *by* K.-P. Chiang et al.

Anonymous Referee #1

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This study reports on spatial variations of growth of bacteria and Synechococcus spp. and predation rate of nanoflagellates in the subtropical surface water of the Taiwan Warm Current Water. The authors' main findings are smaller spatial variations of nanoflagellate abundance compared to those of picoplankton abundance, significant decrease of bacterial growth rate with increasing temperature, significant increase of predation rate on picoplankton with increase of picoplankton growth rate, significant decrease of picoplankton growth rate with increase of picoplankton abundance, and significant increase of nanoflagellate predation rate with increase of picoplankton pro-

duction. Based on these findings, the authors argue the shift between top-down control and bottom-up control on picoplankton, and the importance of nanoflagellates to control picoplankton biomass in the study area.

Main comments

I have much difficulty to understand this article despite potential importance of the dataset.

Firstly, the objective or the main goal of this study is not clear.

- Although the authors mention "This study investigated the impact of the substrate supply and the grazing of nanoflagellates on picoplankton communities. . ." (P235, L24-27), it is questionable if the methods used in this study were proper to investigate the impact of the substrate supply on picoplankton community. The authors should clarify what kind of substrates (e.g., inorganic nutrients, vitamin, dissolved organic carbon, and trace metals) are potential limiting factor for picoplankton community (i.e., mainly bacteria, Synechococcus) in the study area, and which method was applied to investigate this aspect.

- The authors use the concept of "predator-prey eddy" of heterotrophic nanoflagellate-bacteria association (cf. Tanaka et al., 1997) to understand the spatial consistency or variability of the close predator-prey association between nanoflagellates and picoplankton. This concept is proposed to describe that abundances of bacteria and HNF vary seasonally over one and two orders of magnitude, respectively, overlaid by a higher frequency oscillations of smaller amplitude throughout the year in a coastal system (e.g., Thingstad, 2000). On the other hand, the complied dataset from a variety of plankton ecosystems shows that both bacteria and heterotrophic nanoplankton vary about 3 orders of magnitude, in which the numerical relationship between them is remarkably consistent (1000 bacteria : 1 heterotrophic nanoplankton) (e.g., Sanders et al. 1992). Hence I do not understand how this concept can be applied to a dataset which has large spatial scale but much less temporal resolution.

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Secondly, there are many ambiguous descriptions in this article.

- As the authors mention in the article (P235, L9-10), picoplankton generally consist of heterotrophic bacteria, Prochlorococcus, Synechococcus and picoeukaryotes in marine pelagic ecosystem. The authors focus on bacteria and Synechococcus as the important preys for nanoflagellates in this study without explaining why the other picophytoplankton groups were not investigated.

- Nanoflagellate community consists of autotrophic, mixotrophic, and heterotrophic flagellates. The authors should explain why they treat whole nanoflagellate community as an important predator of picoplankton community. They also should consider if abundance and/or predation rates of mixotrophic nanoflagellates were minor or if abundance of autotrophic nanoflagellates were dominant in this study. If so, how the conclusion can be modified?

- The authors collected water samples at six water depths (5, 10, 25, 50, 75 and 100 m) (P237, L5-8). However because the authors use "surface", "surface water" and "surface layer" whose depth or depth range are not defined, it is very difficult to understand the results and discussion. Growth and grazing rates were measured using "surface seawater" samples at 7 stations. The authors should discucc how growth and grazing rates in surface water can be related to the data on abundance of picoplankton and nanoflagellates down to 100 m depth.

- Spatial variations of environmental condition between 7 stations need to be reworked. As the authors mention, T-S diagram suggests that the study area was mostly characterized as Taiwan Current Warm Water (Fig. 3). However, vertical profile of temperature was different between the stations, likely because of Kuroshio subsurface upwelling. In addition, the coastal stations could be influenced by riverine input. Although the authors present the data on concentration of nitrate and chlorophyll in Table 1, they do not really benefit from these data. Trophic status could be different between stations and depth. To better understand the data on abundance, and growth and grazing rate of the

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microbial community, it would be important to clarify how similar or different the trophic condition was along the coast-offshore transect and between different months/years.

- In measurement of growth and grazing rates (P238-239), the authors prepared triplicate samples at the beginning and end of each incubation. Such a set-up should allow estimating variability in cell counting between triplicate bottles, by which growth and grazing rates are determined with variability (e.g., standard deviation, standard error). The comparison of growth and grazing rates can statistically be tested. Otherwise, the objectivity in interpretation of the data remains unclear.

- It appears that the authors are confused between correlation analysis and linear regression analysis. When the significant relationship between two parameters is discussed, it is necessary to show statistical significance (e.g. $P<0.05$).

- There are many reports on growth rate of picoplankton and grazing rate on picoplankton in oligotrophic marine waters. The authors should compare their data with prevous data reported from oligotrophic waters. Fig. 10 suggests that nanoflagellate biomass turnover is faster (4.32/(9.42+5.22)=0.3 d) than bacteria biomass turnover (21.81/16.09=1.4 d) and Synechococcus biomass turnover (13.78/12.12=1.1 d). If one considers autrotrophic production in the nanoflagellate community and other carbon supply through other preys (Prochlorococcus and picoeurakyotes), the biomass turnover of nanoflagellates will be much faster. Are these values consistent with prevously reported values? The bacterial carbon content (20 fg C/cell: Lee & Fuhrman 1987) is derived from culture bacteria, and could result in overestimation of bacterial biomass and production in oligotrophic water.

Other comments

P234, L11-15: If the upwelling water spplies nutrients, by which bacterial growth is stimulated, one may expect that bacterial control shifts from bottom-up to top-down.

P234, L21: I do not think that the concept of "predator-prey eddy" is well known.

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P234, L25: "increasing number of sizes" needs to be clarified.

P245, L1: "nonoflagellate" should be "nanoflagellate".

P245, L1-2: The authors measured predation rate of nanoflagellates on bacteria and Synechococcus but not other picoplankton groups. Hence it is not logic to mention "the diet of nano flagellate is composed of 64% bacteria and 36% Synechococcus spp."

P237, L13: "-75° for later analysis" should be "-75°C for later analysis".

P240, L15: Seven thousands of bacterial cells per ml and <22 Synechococcus cells per ml in the marine pelagic water appear too small. Please verify these numbers.

P240, L23-24: Nanoflagellate ranged from 6.2 x 10 to 1.04 x 10ˆ3 cells/ml. So it varied two orders of magnitude.

P241, L21-28: Please explain the methods in Materials and methods. As mentioned above, the trophic cascade effect is not evident without proper statistical comparison.

P242, L18: "Ferrier-Pagés" should be "Ferrier-Pagès".

P246, L16: "Hofle" should be "Höfle".

P247, L6: "nonoflagellate" should be "nanoflagellate".

P252, Table 1: - "surface" should be defined here. "surface" means which depth? - Values in the table should be defined (means ± standard deviation or standard error or else?). - Although the data on concentration of nitrate and chlorophyll a were presented in Table 1, there is no description for these parameters in Materials and methods.

P255, Fig. 3: All data points are within TCWW and KW whose space occupies only a small area of Fig. 3. Is it really important to show the other water masses (CDW, YSMW and YSCCW)?

P262, Fig. 10: Production rate of nanoflagellates is not explained in the text, but presented in Fig. 10.

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P263, Fig. 11: "1796-st1", "1809-st1", "1816-st1", and vertical bars need to be explained.

Interactive comment on Biogeosciences Discuss., 10, 233, 2013.