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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 #2

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This paper described the spatial distribution of growth and grazing rates of picoplankton in the subtropical continental shelf water, and tried to evaluate the relative importance of top-down and bottom-up controls on them on the assumption that the spatial variance of the parameters in similar conditions reflecting temporal dynamics of the microbial assemblages involved in microbial food webs. I believe that the methods are clear and robust, and data quality and quantities can satisfy the scope of the journal Biogeoscience. However, I am not fully satisfied with the description, in which explanations are not sometimes satisfactory, and some logics were difficult to understand.

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Reading this manuscript, I got so confused with the conception of “control”. When one system was amended with full resources and one of the components increases, we can say that the component is mainly under “bottom-up control”. And if another component does not increase, the component is more under “top-down control”. However, if the gross (intrinsic) growth rate is stimulated by the amendment, the bottom-up control is also effective. However, dichotomy between the bottom-up and top-down is only seen in laboratory cultures or the water with high load of resources, where the carrying capacity can be reached. Here I would like to emphasize, the top-down and bottom-up controls are a matter of relative importance in open waters, where resource supply is scarce and grazing pressure is considerable. We can evaluate the relative importance just by comparing intrinsic growth rates, grazing rates, and net growth rates within a range of environmental parameters (such as nutrient supplies) varying temporally or spatially. At this point, I feel that data presentation in the present manuscript is not sufficient to discuss this issue.

P235L6 “a linear food chain” is intuitively not appropriate to describe “the microbial loop”. The way back to bacteria (through decomposition of organic matter) is an important component of the loop.

P237L5 “microscopic counting of picoplankton” is not mentioned in the other parts of the manuscript.

P237L13 Add “C” after degree signs (also in the other parts).

P239L7 Micrometers should be subscript.

Other comments on Methods section: Why are picoeukaryotes and Prochlorococcus not mentioned in this study?

Are there any data on nutrients? They would be very helpful in examining controlling factors.

Did you confirm that the fractionation procedures did not induce nutrient contamina-

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tion?

P240L3 “significant difference” Which statistical test did you undertake? (Also in P240L16 and P240L24)

P240L7 Units are unnecessary for salinity.

P240L23 Nanoflagellates seem to have varied over 1 order of magnitude (rather 2 orders of magnitude).

P240L27 How do you explain the negative grazing rate?

P241L1 Show the data of nutrients to validate this description.

P241L4 Show the data for “growth rate was not affected by upwelling water”.

P241L9 $p > 0.05$. . . The coupling relationship is not significant.

P241L24 “from June 2011 to September 2006” It is unreasonable. . .

P241L25 This truncation experiment should be written in methods section in more detail. At least, I failed to comprehend the whole image of this experiment from this description. And if it has been already published in Lin et al. (2009), it should not be demonstrated here as a new result and it should be referred to in Discussion section with appropriate citation.

P242L5 “in aread” should be “in areas”.

P242L11-L25 This should be in Introduction.

P243L12 I think it is important to separate the effects of temperature itself and of nutrients advected with the upwelling water. There have been some reports on Q10 values of bacteria and *Synechococcus*. Thus you can calculate the expected effect of temperature itself on growth of these picoplankton assemblages.

P243L17 Is the model by Word and Coffin (1984) based on varying abundance and growth rate at similar nutrient environments? The situation in your study is based on

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varying nutrient environments (and temperature), so the extrapolation of this model is difficult to interpret. (1) In the upwelling area, water temperature was low. (2) In the colder water, the bacteria grew faster. (3) Where the bacteria grew fast, its abundance was low. I think that these three facts just suggest that the bacteria in this area may be limited by nutrient supply, but that the grazing limitation (top-down) is so high as well to suppress the effect of nutrient amendment by upwelling.

P244L25 This is the description on the phenomenon generally known. Thus you can omit “In the present study” and add appropriate citation.

P245L2 “Growth rates of both. . .” This is a repetition of the precedent sentence.

P245L17 “smaller” should be “larger”.

P245L22 “the growth rate of picoplankton is controlled by its abundance” contains leap of logic. You just observed a negative relationship between the growth rate and abundance.

P245L21 How about *Prochlorococcus* and eukaryotic phytoplankton?

P247L1 Describe how you corrected the grazing rate in more detail.

P247L5 Show the corrected figures to validate this description.

Fig. 3 Where is a border between TCWW and KW? And this graph is almost blank space. Rearrange the scales of the two axes.

Figs. 4-9 These graphs can be drawn in black and white.

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