

Interactive comment on “Phytoplankton growth and physiological responses to a plume front in the northern South China Sea” by Qian P. Li et al.

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Response to Reviewer #1 (by K. Bjorkman): 1. "... However, I do have two main criticisms 1) that nutrient concentrations were not measured from the incubation experiments to assess nutrient drawdown over time. Having the nutrient's fate in the incubations may have shed light on if the loss of chlorophyll at day 2 was due to nutrient limitation, or something else. This would have added much value to this study."

Response: Nutrient concentrations were actually measured during incubation experiments. We have added these data to the revised manuscript to discuss the changes of chlorophyll and nutrients over incubation time. P-limitation at stations S1, S2, S3, and

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S4 was confirmed by change of N and P during incubations: there were enhanced N consumptions by addition of P, but P consumptions were not stimulated by addition of N. For S6, N-limitation was supported by enhanced P consumptions by N-addition (but N consumption was not enhanced by P-addition). The nutrient data also suggested that N and P co-limitation at the second day of incubation found at S7 was due to running out of N by P-addition during the first day of incubation.

2. "2) The design of the plume water mixing experiment. The design of the plume water mixing experiment makes it difficult to interpret the relative importance of the seed community (structure, biomass) and the influence of nutrients and salinity changes. As it is presented here I do not think the results support the conclusions drawn. The mixing of whole plume water with whole 'sea-side' water at different ratios, is in effect a dilution of one community by the other, with the 100% and 0% being the end members. From data in Fig 7A, the chlorophyll based growth rates (μ) are more or less identical for all additions of plume water, indicating that the results are reflecting the dilution, not changes in growth rates, even if the final chl a is increasingly higher with higher plume water addition. I suggest that the authors carefully reexamine this experiment and its outcome in a revision of this manuscript."

Response: We agree with the reviewer that the design of the plume water mixing experiment cannot separate the different effects by the changes of nutrients, salinity, and phytoplankton population. However, we don't intend to separate these factors by the plume water mixing experiments. What we focus on is the combined effects of varying community, salinity and nutrients during the mixing experiments, given the relatively short distance of these two waters at the frontal zone. To separate the effects of nutrients and seed population inputted by PW, we had to chose a small percentage of FPW addition (12.5%) at station S6 to ensure that the initial chl-a concentration after FPW treatment is comparable to that of the control experiment (Figure 8A1-A4). In that case, the difference between the FPW treatment and the control should reflect the impact of nutrients, whereas the difference between PW and FPW treatments should

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reflect the impact of seed population inputted by PW. The chlorophyll-based community net growth rate ($\bar{\mu}_{chl} = \ln(\text{Chl}_t/\text{Chl}_0)$) is -0.6 d^{-1} for S4 (local water at the front), but it is 1.0 d^{-1} for S2 (the plume water). Since the initial chl-a concentration of S2 was about 6 times that of S4, the mixed community (for 25%, 50%, 75% PW treatments) should be dominated by population from the plume community. That is why we can see a similar positive net growth rate for all additions of plume water. On the other hand, the apparent net growth rate ($\bar{\mu}_{app}$) should be determined by the specific growth rate (A) and the grazing mortality rate (B) based on the equation of $\bar{\mu}_{app} = A - B$ (e.g. Landry and Hassett 1982). Therefore, if the growth and grazing of phytoplankton are tightly coupled during the dilution, we should not expect to see a large change of net growth rate. We have carefully discussed these results in the revised manuscript.

3. P3, ln14. Suggest adding Mahaffey et al. (2012) here. This paper contains data on mixing experiments too that may be informative for this manuscript too. (Mahaffey et al., 2012. Phytoplankton response to deep seawater nutrient additions in the North Pacific Subtropical Gyre. MEPS 460:13-34)

Response: Done.

4. P4, ln 18, 19. What about station 8? Should it be listed here too? (p5, ln 22?)

Response: S8 is located at the same place as S4 but at different time, which had already been clearly stated in Table 1.

5. P5, ln 1-3. Choice of filter types. Why were the filters of such different materials? Do they have different retention characteristics apart from pore size? Where the chlorophyll fractions determined by difference or where these from sequential filtrations?

Response: The three types of filters have all been previously used for phytoplankton size-fractionation in the South China Sea (Huang et al., 2008; Chen et al., 2009). Chlorophyll fractions were determined by sequential filtrations using these filters during our cruises. The GF/F filter ($0.7 \mu\text{m}$) can be used for collecting picoplankton due to its

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high particle absorption ability leading to similar retention ability as 0.2 μm Millipore membrane filter.

Huang et al., Spatial and temporal distribution of nanoflagellates in the northern South China Sea, *Hydrobiologia*, 605:143–157, 2008. Chen et al., Trophic interactions within the microbial food web in the South China Sea revealed by size-fractionation method, *Journal of Experimental Marine Biology and Ecology*, 368: 59–66, 2009;

6. P5, In 4. Did you see any Si contamination from the glass fiber filter used?

Response: We did not see Si contamination for GF/F filter. Silicate concentration after GF/F filtration was not much different from that after 0.2 μm membrane filter.

7. P5, In 13. What determined the final concentrations of N and P added? Perhaps also add that these are at $\sim 16:1$ or Redfield ratio for N:P.

Response: Thanks for pointing out this. The final In the revised manuscript, we have clearly state that the additions of N and P for incubation experiments were based on the Redfield N:P ratio of 16 to 1.

8. P5, In18. Suggest adding at what stations and at what dates these N+P nutrient addition experiments were performed.

Response: Done.

9. P6, In 1. Please describe what question this experimental design was meant to answer, or test? Also, please add when these where performed.

Response: Done. The mixing experiment conducted on June 19th, 2016 is to simulate phytoplankton response to the intense mixing process by the dispersive river plume.

10. P7, In 18-20. This sentence is confusing to me. What is meant by “...east of the PRE by eastward plume dispersion...”? That the low salinity tongue from the PRE was cut off by another water mass with low temperature and high salinity?

Response: We are sorry for confusing. We have rewritten these sentences in the revised manuscript. The surface low salinity tongue in the coastal water east of the PRE (generated by eastward plume dispersion) was cut off by another water mass of low temperature but high salinity during the June

11. P9 – plume water mixing experiment. This is my main problem with this manuscript. The design of this experiment does not allow for testing what I think was the intention to test. Which is to say, the effect on plume water mixing (with its extant community and nutrients) with seaside water (with its extant community and nutrients). However, the way this is set up, it is difficult to separate what changes in chl is derived from the seed population or the changes in available nutrients. This would have needed to also include reciprocal dilutions using filtered PW and/or surface seawater.

Response: The reviewer is right about that our design of the mixing experiment between S2 and S4 could not separate the effects between seed population and nutrients. Actually, we don't intend to separate these two as they should be both important for phytoplankton chl-a change at the frontal zone given the relatively short distance of the two waters. The including of reciprocal dilution experiments with the filtered plume water (FPW) and/or filtered surface sweater of S4 cannot separate the effect of varying nutrients from that of the change of seed phytoplankton. The reason is that the initial chl-a concentration will be largely diluted along with the increase of nutrients. To separate the effects of nutrients and seed population inputted by PW, we had chosen a small percentage of FPW addition (12.5%) at station S6 to ensure that the initial chl-a concentration after FPW treatment is comparable to that of the control experiment (Figure 8A1-A4). In this case, the difference between the FPW treatment and the control should reflect the impact of nutrients, whereas the difference between PW and FPW treatments should reflect the impact of seed population inputted by PW.

12. P9, In 21. Are the nutrients running out? Are there data to show this?

Response: Yes, phosphate was almost running out during the second day of incuba-

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tions. We have added nutrient data to the revised manuscript.

13. P12, In 10. Have the effect of changing salinity on phytoplankton growth for the sea side versus plume water plankton been considered.

Response: We have added discussions of the impact of salinity on phytoplankton growth in the revised manuscript. Coastal phytoplankton species can generally tolerate a much larger range of salinity than estuarine and oceanic species (e.g. Brand 1984). The salinity of 6.6-30.7 during the mixing experiment at the frontal zone is higher than the lethal level of ~ 5 for most estuarine phytoplankton species due to osmotic pressure (Kies, 1997; Floder et al., 2010). However, inter-specific differences in salinity tolerances of phytoplankton may be important for phytoplankton growth at the lower range of the PRE where fluctuating salinities between 0-10 were found.

14. P13, In 3. Suggest citing Mahaffey et al 2012 here too

Response: Done.

15. P18 Table 1. Should data from station 8 be included here too?

Response: Done. Data of station S8 was added to table 1.

16. Figures 6-9. It would be helpful to see the chl concentration of the size fractions at t0 in these graphs. Also, it would be good to add when each of these experiments were carried out.

Response: We decided to not show the initial size-fractionated chl-a data in these Figures. The initial size-fractionated chl-a concentrations for S1-S8 (Figure 6) had already been shown in Table 1. For other experiments in Figures 7-9, the initial size-fractionated chl-a concentrations were simply calculated based the fractions of waters mixed for these stations. The start dates of incubation experiments have been added to figures in the revised manuscript.

Please also note the supplement to this comment:

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