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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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The authors have studied the impact on phytoplankton population growth and community size structure from the shore side to the sea side of a strong salinity front, set up by freshwater discharge from the Pearl River Estuary (PRE), over the shelf waters in the northern South China Sea. Apart from the extensive hydrographical study over the area impacted by the river plume, they also conducted several experiments at different stations along the PRE to marine gradient. These were designed to assess phytoplankton nutrient limitations, by the addition of inorganic nitrogen and phosphorus, as well as the influence on productivity and community structure of either vertical (upwelling) or horizontal mixing (river discharge) of water masses.

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They found that the region studied was highly dynamic over the months studied, but that the phytoplankton community showed P-limitation at, and shore-side, of the salinity front, whereas sea-side waters tended to be N-limited. However, upwelling of sea-side bottom water would promote growth in the surface populations on both sides of the front as suggested by the mixing experiments. Discharge of plume water also promoted growth of the larger cells. The authors conclude that the physical dynamics of the river plume could deeply affect phytoplankton physiology and growth.

This is predominantly a well written manuscript with mostly smaller points and edits. However, I do have two main criticisms 1) that nutrient concentrations were not measured from the incubation experiments to assess nutrient drawdown over time, and 2) the design of the plume water mixing experiment. 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. 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 of 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.

Other comments:

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)

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

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?

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

P5, ln 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.

P5, ln18. Suggest adding at what stations and at what dates these N+P nutrient addition experiments were performed.

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

P7, ln 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?

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.

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

P12, ln 10. Have the effect of changing salinity on phytoplankton growth for the sea

side versus plume water plankton been considered?

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

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

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.

End review

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