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## ***Interactive comment on “Dynamics of phytoplankton community structure in the South China Sea in response to the East Asian aerosol input” by C. Guo et al.***

**C. Guo et al.**

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We thank the reviewer for her/his constructive suggestions for our paper. Please see our detailed response to reviewer’s comments below.

P6642L19 For how many days and into what seawater the aerosols were settled to dissolve? This information is very important to extrapolate the results into natural environments.

Pre-filtered natural seawater in the South China Sea was used to make the leachate. We choose the leaching time of about 1 h according to previous studies (Martin et al. 1991; Aguilar-Islas et al. 2010, Hsu et al. 2010), and the leachate was stored in dark

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under 4 degree. For more detailed explanations about the aerosol leachate, please refer to our response to Reviewer #1.

P6643L2 In what way the surface seawater was collected? Were appropriate trace metal-clean technique were applied?

The surface seawater was collected by an acid cleaned bucket. As our initial intention of this study is to look at the N input, but not trace metal. So we did not measure the trace metal concentrations in the microcosm during the experiment, but only measured trace metal composition of the aerosol for reference. We tried our best to avoid any contamination during experiment. Because our aerosol leachate has high trace metal concentration, thousands times higher than in natural seawater as the reviewer points out, the contamination issue that may have occurred during sampling and experimental setup is likely not a big concern. Besides, because we are aware that our approach is not trace metal free, we do not focus our research on trace metal. If all our bottles or sampling procedures are somewhat contaminated, despite our best effort, then the difference between aerosol enrichment and N and P enrichment observed in Exp. 3 may still be attributed to the unknown substance (although we speculate metals) in the aerosol.

P6643L2 From which depth the seawater was collected?

The seawater was collected at 0 m.

P6646L21 Although the authors describe “Concentrations of inorganic N were below 1  $\mu\text{mol l}^{-1}$ ”, Table 1 tells us that at PM7, nitrate and nitrite concentration was over 1  $\mu\text{mol l}^{-1}$ . Which is correct?

The table is correct. We have changed the sentence in the revised version.

P6648L7 The authors say “The Chl a concentration in the Low treatment also showed a slight increase”, but figure 3d show that chlorophyll a concentration in the Low treatment dramatically decreased. Do the authors mean “The Chl a concentration in the

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Low treatment was slightly higher than in the control”?

Yes. We mean the Chl a concentration showed a slight increase compared with control. We have corrected it in the revised paper.

P6650L9 “... abundances of dinoflagellates species showed a decreasing trend after aerosol addition.” But Fig. 4 demonstrated that peridinin, which is a marker pigment of dinoflagellates, increased after aerosol addition. Does this mean that most dinoflagellates were heterotrophic species, that cell-specific pigment contents increased or both of the two?

Both of the two reasons were possible. Also, as we mentioned in the response to reviewer #1, counting by microscope using only 10-30 ml sample may also resulted in large variations since dinoflagellates abundance was very low. Due to the fact that dinoflagellate abundance was two orders of magnitude lower than diatoms, we may not need to include the rather highly variable data in Fig. 6.

P6651L15 What do the authors mean by “most” trace metals? On nutritional terms, trace metal elements include V, Cr, Mn, Fe, Co, Ni, Cu, Zn and Mo (and possibly As, Sr, Cd, Sn, Ba and W), while on biogeochemical terms, they include most metal elements other than Na, Mg, K and Ca. Anyway, the data provided in Fig. 2 do not cover all the elements included in “trace metal”.

We mean most of the trace metals listed in Fig. 2. We have changed this inaccurate term.

P6651L17 “EA samples ... contain notably higher contents of trace metal elements ... than those in African and European aerosols” Aerosol composition data provided by the authors in Fig. 2 are shown as “trace metal concentration in aerosol leachate”, which was affected by solubility of aerosols. Is it to possible them with other aerosol composition data?

The trace metals and macro-nutrient concentrations of African and European aerosols

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we compared to were also the soluble fraction. In that study (Paytan et al. 2009), incubation received the aerosol filter, and the amounts of nutrients and trace metals were measured after they released to the incubation bottles from the addition of 6 mg of African aerosols or European aerosols. In order to make those data comparable, we converted the unit of our trace metal data from nmol/L to  $\mu\text{g}/\text{mg}$  aerosol using the in situ PM 2.5 concentration data obtained from the Environmental Central Facility (ENVF/ IENV) Atmospheric & Environmental Database of HKUST (<http://envf.ust.hk/dataview/gts/current/>). But the size fraction of aerosol we used may be different, so we have added more information when make the comparison in the paper.

P6651L26 In this study, aerosols were added after leaching in seawater for some time and following filtration. Is it possible to compare the results with others?

Some studies we compared to using the same approach as ours. For other studies in which the dust was added directly and then incubation was conducted for several days, the leaching procedure also existed. The difference is that we did the leaching procedure before the incubation. Furthermore, we have tried our best to stimulate the natural condition. The filtration was to exclude bacteria which may consume the nutrients in the leachate. Therefore, our method of making leachate should not prevent our results to be compared with those of other studies. However, since we used PM2.5 to conduct our study but others used mineral dust or PM10, it may not be appropriate to compare the results directly because PM2.5 contains high nutrient content relative to its volume. So we have made some changes in this part.

P6652L11 Given than N/P ratios were larger than 16, I suspect that the possible limiting element was phosphorus.

The N/P ratios were less than 16 at most stations especially in the oceanic stations. Sorry for the typos.

P6652L14 In this discussion, the authors must be cautious not only about elemental

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ratios, but also about absolute amount (availability) of each element. If the ambient (initial) amount of each nutrient was scarce, the impact of aerosols on elemental stoichiometry became relatively great.

Agree. We will incorporate this suggestion into revised manuscript.

P6652L19 Actually the surface Fe concentration of 0.2-0.3 nM is very low, but whether it is at a limiting level is not clear. Check the stoichiometry (Fe/N and Fe/P ratios) of phytoplankton.

Thank you for your suggestion. According to previous studies, the C: Fe ratio ranged from 10,000 (Fe-replete condition, Morel and Hudson, 1985) to 100,000 (Fe-deplete condition, Anderson and Morel, 1982). Assuming the Redfield ratio of 6.6 C: 1N, the N: Fe ratio ranges from 1.52 to 15.15  $\mu\text{M}$ : 1 nM. If we use Fe concentration of 0.25 nM in the South China Sea, the amount of N for supporting phytoplankton growth should be ranged 0.38 to 3.79  $\mu\text{M}$ . Sunda et al. (1991) have showed that the C: Fe may be as high as 500,000: 1 in the open ocean, which suggested a higher value of N: Fe ratio. As we observed the N concentrations of <0.3  $\mu\text{M}$  in the oceanic stations SEATS and A1, it is more possible that the N was relatively limiting. We will consider incorporating this evaluation into the revised paper.

P6654L23 As described here, it has been reported that some strains of *Prochlorococcus* and *Synechococcus* lack in proteins essential for utilization of nitrite or nitrate. However, probably all marine phytoplankton including *Prochlorococcus* and *Synechococcus* can uptake and utilize regenerated inorganic nitrogen (ammonium); therefore they may have benefited from aerosol enrichment. Responses in their cell size and chlorophyll fluorescence (Figs. 8 and 9) seem to rather support this explanation. The explanation that activated grazing exceeded the enhancement of pico-sized cyanobacteria by aerosol enrichment sounds more natural than the description here. P6654L13 The explanation in the section 4.4. sounds more reasonable to me. Rather the discussion in the section 4.3 seems to contradict with the discussion here.

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Agree. We also realized that it was not appropriate to say that *Prochlorococcus* and *Synechococcus* do not benefit from high concentrations of aerosol leachate. We have changed this part of discussion accordingly.

P6657L12 How did the authors define the terms “ultraoligotrophic” and “oligotrophic”?

The word “ultraoligotrophic” was from the paper we cited (Marañón et al. 2010). In that paper, the word was used to describe the very oligotrophic site in the Mediterranean Sea. Many other studies also used this word to show the extremely oligotrophic condition in some parts of the Mediterranean (e.g., Thingstad et al. 2005). Although we could not find a clear criterion for defining the term “ultraoligotrophic”, some studies indicated that the ultraoligotrophy is a very severe degree of oligotrophy, with strong stratification, great nitracline depth (e.g.,  $>\sim 120$  m, Marañón et al. 2010), very low N or P concentration (at the magnitude of 0-10 nM), and very low primary production (e.g., 29  $\mu\text{mol}$  of C  $\text{m}^{-3}$  Day $^{-1}$ , Thingstad et al. 2005) and Chl a concentration (at the magnitude of  $<0.1$   $\mu\text{g/L}$ ).

P6657L12 In this study, experiments with different designs are conducted at different ocean stations with different chemical fields. Here the authors must take into account confounding of experimental design (mode of aerosol enrichment) and environmental factors (initial nutrient concentration and composition of autochthonous plankton communities). I believe only after the authors discuss this point deeply, their conclusions can be extrapolated into the whole South China Sea and the North Pacific.

That is an important point. We will incorporate this suggestions into revised manuscript.

Fig. 3 Were these figures obtained by fluorometry or by HPLC?

These figures were obtained by fluorometry.

Fig. 7 Was the precision sufficient for ciliate concentrations? In Methods, the measured volume was 10-30 ml. which means that counts of ciliates were at most 90 or so. This may not be insufficient.

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We agree that the volume may not be enough for counting ciliates precisely. It may be also the reason of the large error bars here. Due to the large variation and inaccuracy of the data, we have decided to delete this figure in the paper.

Fig. 11 How was the biomass (or abundance) of Haptophyta and green algae measured or calculated (estimated)? Show the detailed methodology in Methods.

We used the concentration of the marker pigments neoxanthin and 19'-hexanoyloxyfucoxanthin measured by HPLC to indicate the biomass of haptophyta and green algae respectively. We have added this information to the revised paper.

Technical comments

P6647L5 Check significant digits.

We have made changes as suggested.

P6652L12 Using "limiting" seems more appropriate than "limited".

We have made changes as suggested.

P6654L8-9 Give an appropriate citation for this description.

We have made changes as suggested.

Table 2 Check significant digits (particularly for chlorophyll and nitrate concentrations).

We have made changes as suggested.

Fig. 2 Show the standard deviations or standard errors.

We did not collect replicated samples of trace metal measurement.

Fig. 7 For the unit of ciliate cell concentration, cells L-1 would be more appropriate.

We have made changes as suggested.

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