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# *Interactive comment on* "Phytoplankton growth responses to Asian dust additions in the Northwest Pacific Ocean versus the Yellow Sea" *by* Chao Zhang et al.

**Anonymous Referee #1**

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Review of Zhang et al. Biogeosciences

General comments

This study describes the results of several experiments in which surface plankton communities from the Yellow Sea and the NW Pacific ocean were amended with atmospheric dust and different nutrients added alone and in various combinations. The responses studied included the numerical abundance of diatoms and dinoflagellates, size-fractionated chl a concentration, and nutrient concentration. The main strength of the study is that parallel incubations, in which inorganic nutrients were added in different combinations, allowed the authors to gain insight into the causative mecha-



nisms underlying the phytoplankton responses to dust. However, in some cases (as in the case of P availability, see below) the authors seem to over-interpret the available evidence. A limitation of the study is that only standing stocks were examined; no metabolic rate measurements were included, and therefore it is not possible to ascertain the dominant type of nutrient limitation (Blackman versus Liebig). The authors rely heavily on the use of chl a as a proxy for phytoplankton biomass. However, variability in C:Chla ratios should be taken into account. The extent to which the simulated process of atmospherical tranformation of dust yields materials that are realistic in terms of nutrient content and solubility should be discussed. Finally, some sections of the Discussion are speculative and based on tenuous assumptions. All these limitations should be addressed before publication is recommended in Biogeosciences. Some suggestions as to data presentation and analysis are also given below.

Specific comments

Simulation of atmospheric transformation of dust. How do these AM-dust materials compare, in terms of nutrient composition and solubility, with real dust samples collected in situ? This is critical to assess if the results observed are representative of real responses at sea. Table in supp. info. shows that N concentration is increased 4 orders of magnitude relative to N concentration in collected rain. Does this mean that the potential for nutrient supply is grossly overestimated in these artificially treated materials?

Section 3.2. This section should present first the changes in nutrient concentration, and then those of chla concentration. In both cases, the actual increases (absolute values) should be described (e.g. nutrient or chl a concentration increased by xx umol/L or ug/L), rather than just the relative increases (xx-fold). It is important to describe the chla responses in terms of absolute value of increase, so that they can be compared with the amount of nutrient released from the dust or provided by the nutrient amendments.

Conversion efficiency index. This index should be described in the Methods section.

It is unclear why the index is formulated in this way. Why not use just final minus initial chla concentration, as is done for N? It does not make sense to sum consecutive differences over time in chla concentration between treatments and control. In addition, the index has a potential flaw, because C:Chla ratios are bound to be different in the different sites (due, for instance, to differences in nutrient and/or light availability). So the same response in terms of % increase in biomass (carbon) will yield higher chla concentration (in absolute values), and thus higher conversion efficiency, in waters with low phytoplankton C:Chla values. The limitations of using Chla as a proxy for biomass should be acknowledged and discussed. Finally, when reporting the values of this index in the text, its units should be indicated.

Section 4.3. This section is speculative and difficult to follow. It is unclear how the 'increase in bioavailable P concentration following AM-dust addition' has been identified. The relationship between N:P ratios in supply vs demand is tentative at best, since actual supply N:P ratios were not measured. The paragraph on lines 395-406 starts with an untenable assumption, namely that 'C_N:P in AM-dust treatments is equal to that in N treatments'. To the extent that dust additions and N additions create distinct nutrient environments, it is most unlikely that consumption N:P ratios will be the same. In fact, the previous paragraph has argued that consumption N:P ratio is lower in dust treatments than in N treatments. Thus the subsequent calculations and conclusions have no use. This sub-section (l. 395-406) should be deleted. In the subsequent paragraph, the basis for the need for additional P supply is unclear.

Minor comments

lines 53-54: Rewrite sentence: 'The N:P ratio of dust deposition is much higher than the Redfield ratio (N:P=16)...'

line 110. Were attenuation filters used? PAR levels should be given.

line 139. The ultrasonic method should be described briefly. The use of ultrasounds maximises the extraction of nutrients but it probably overestimates the amount of nutri-



ents that is actually released in real conditions at sea.

line 141. Re-write sentence, '...and filtrates were stored...'

line 153: '...enumeration of...

line 171: delete 'evidently'.

line 174: 'trophic level' means something else. Replace by appropriate phrase.

line 185: Why is P gained during the treatment?

line 189 Here cite Fig. 3, otherwise the reader does not know where is that increase reported. Tables do not report the nutrient increases observed in the treatments.

line 194 and elsewhere (including Fig. legends). The phrase 'successive increase' should be omitted. Sentence should read simply: 'During the incubation...'

line 271. Remove 'certain amount of'

line 300. This sentence seems to assume that all N present in the dust becomes bioavailable, because the concentrations referred to are those given in Table 3 (which correspond to concentrations in the dust, not in seawater).

line 448-449. Here the authors are deriving biogeochemical conclusions on the functioning of the biological pump, but their data consider just phytoplankton. Without information on how the metabolic activity of heterotrophs, bacteria in particular, changes in response to nutrient/dust additions, the ultimate effect on the biological pump remains unknown.

Table 1. Silicate measurements are missing – they would have been helpful to constrain the stoichiometry of diatom blooms in response to nutrient/dust amendments

Table 3. The data labelled 'increased concentrations' are theoretical or expected concentrations, assuming 100% of the nutrients in the dust becomes dissolved. This should be explicitly acknowledged in the Table legend. Is there any evidence to sup-

port the tenet that, upon dust deposition onto the ocean's surface, all nutrients become dissolved and bioavailable?

Figure legends: The phrase 'successive increase' is awkward. Delete in all fig. legends. It should be simply: 'changes in xxxx during the incubation period at each station'.

Fig. 2. Y-axis intervals should be regular (e.g., 0.5 or 1.0 ug/L) and consistent in all plots. Minor ticks should be included, to help the reader ascertain the magnitude of responses.

Fig. 3. Symbol for control should be more visible (it is often masked by other symbols).

Fig. 5. Revise species names spelling (e.g. Skeletonema). Species names and genera should be written in italics (but not 'spp.').

Figs. 6 and 7. The index values shown here result from the subtraction and division of variables measured independently, each with its own error. Therefore the error bars shown should be computed using the error propagation formulae for addition and division.

Fig. 8. These N:P ratios should be defined in the Methods section. Strictly speaking, the N:P supply ratio is not known, since no solubility experiments were conducted.

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