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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 #3

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Title: Phytoplankton growth responses to Asian dust additions in the Northwest Pacific Ocean versus Yellow Sea

Authors: Chao Zhang, Huiwang Gao, Xiaohong Yao, Zongbo Shi, Jinhui Shi, Yang Yu, Ling Meng, and Xinyu Guo

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General Comments:

Biological productivity of the open ocean regions, especially oligotrophic parts, have attracted the attention of several researchers. Our present understanding of the role of

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mineral dust in enhancing the primary productivity of the oligotrophic ocean by supplying bio-available nutrients is still in its infancy. The present manuscript aims at advancing such an understanding of enhancement of phytoplankton growth and the phytoplankton community structure by the nutrient supply from the Asian in the subtropical gyre, Kuroshio Extension of Northwest Pacifica Ocean and Yellow Sea region. The authors attempt this by incubation experiment onboard R/V Dongfanghong II during spring. For incubation experiment, the mineral dust collected from Gobi Desert region was artificially modified and phytoplankton assemblages from subtropical gyre, Northwest Pacific ocean and Yellow Sea region were used. Each of the five microcosm experiment lasted for 9 to 10 days. Using a net conversion efficiency index, proposed by the authors, of nitrogen conversion to chlorophyll a the authors explore the role of bio-available nutrients from mineral dust in the primary production at the above mentioned three regions.

The subject matter of the manuscript addresses an important aspect of phytoplankton growth by "bio-available nutrients" from "treated soil from Gobi desert" which is "expected to" simulate the natural mineral dust. Though there are several concerns that have been listed under specific comments, in my opinion, the results are publishable; but only after adequately addressing the concerns.

Major concerns:

1. The major concern is that the authors have not succeeded in unambiguously resolving the issue of quantification of bioavailability of nutrients form artificially modified mineral dust, especially phosphorous, for phytoplankton growth, which is the central theme of the manuscript. See for example, lines 334-341. The ambiguity regarding the "missing N and P".

2. How would the authors differentiate the phytoplankton growth-response due to N/P/Fe and that due to Mn and Zn (see for example, Saito et al., 2008; Sunda 2012).

3. The authors do not observe any community shift in the phytoplankton in their study.

There is no discussion on this aspect and authors need to address this.

4. Based on the information provided, it is hard to see how closely the artificially modified soil collected from Gobi desert mimics the nature. Some more robust information on the atmospheric (chemical) processing of the mineral dust during the long-range transport from source to the proposed study site during spring is needed along with the upper air wind vectors. What are the chemical constituents of such atmospheric processed mineral dust in presence of anthropogenic aerosol is not clear.

5. It is not clear from the manuscript, whether the ocean atmospheric conditions during the 3-month period (March-May 2014) at each of the sampling locations could be considered as a part of the same season where ocean and atmosphere represents similar conditions. The data from the Table 1 do not support this. For example, the average temperature (is it SST?) at S4 is quite different from the rest of the stations, which was sampled in May 2014. Similar, the MLD also is quite different. The authors need to address these issues.

6. The authors need to discuss the efficacy of the proposed "net conversion efficiency index" in varying Redfield ratio conditions.

7. Most of the results obtained from the incubation experiment, such as co-limitation of nutrients, response of phytoplankton biomass and structure, are largely known as could be seen from the literature cited in the manuscript. For example, co-limitation in the south China Sea and its response to aeolian input(Gao et al., 2012), Fu et al. (2009) on N:P ratios during spring, Fu et al (2009) study on the phytoplankton biomass and structure in South China Sea. Nishibe et al. (2015) work in the Kuroshio Extension during spring. See also under Minor comments (8).

Minor concerns:

8. Lines 310-311: Authors need to at least briefly state what are those "Complex hydrographic conditions"

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9. Lines 339-342: This is purely speculative and needs further substantiation.

10. Lines 307-308: So what is new/different from the work of Nishibe et al. (2015).

11. Line 143: Expand SPSS

12. Table 3 : 2ns foot note (b) is missing in the Table

13. Also explain “E-3, E-4”

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