

## ***Interactive comment on “Effects of dry and wet Saharan dust deposition in the tropical North Atlantic Ocean” by Laura F. Korte et al.***

**Anonymous Referee #1**

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This manuscript presents results obtained during incubation experiments performed with seawater collected at different sites in the tropical North Atlantic Ocean, and submitted to different types (concentrations, wet vs. dry, dust source regions) of dust additions. The release of nutrients and the subsequent response of pico-phytoplankton were followed for 4-8 days. The initial design of this experiment is interesting and robust with different treatments (different dust concentrations and mode of deposition), systematic controls and three replicate incubation bottles per treatment. However, while this topic is timely, some important information are missing and I think that the obtained dataset does not allow the authors to tackle the main problematic of this study, i.e., the potential of Saharan dust as a fertilizer for phytoplankton growth. I think that this manuscript should be rewritten in order to focus on the main findings, i.e., the

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absence of N release and the non-response of the pico-phytoplankton community.

Main comments - Three incubation experiments have been performed. Only two are discussed and the third one can be found in the supplementary info. Since results from the third experiment are not discussed at all, I would suggest to remove it from the manuscript, or at least from the abstract. - Cell abundance measured by flow cytometry is the only parameter used to follow the biological response. Information about chlorophyll a, micro-phytoplankton, etc. are missing. For example, the decrease in Si and increase of POC (Fig. 8) seem to indicate a response of the diatom community rather than the formation of aggregates. - The biological response is not mentioned in the discussion section while it represents the main problematic of this study. I understand that this is probably due to the lack of evidence of a fertilization effect. However, this experiment is neither designed to investigate/quantify the release of nutrients, nor the aggregation process. - I suggest to remove the section 4.5 about the aggregation process. Only final POC concentrations are used to discuss this process. How the authors discriminate (and quantify) newly formed aggregates from the increase in micro-phytoplankton cells for example? How did the incubation conditions influence the formation of aggregates? Were the particles maintained in suspension or did they sit on the bottom of the bottles?

Additional comments The title should be modified to be more precise. Abstract - L29-30 – not necessary to specify M1 and M3 as there are no additional information for these sites in the abstract. The increase in *Synechococcus* was probably not related to the additions of dust since the same increase was observed in the control treatments. Table 1 – I suggest to replace “mg” by “mg/L”. P8-L24 – Replace “0 uM” by “below the detection limit”. P11-L14-16-24 – I suggest to replace “original” by “initial”. P14-L33 – I suggest to replace “nutrient development showed a similar temporal progression” by “nutrient development showed a similar temporal evolution”. Figures 7 and 8 – Which incubation experiment? Finally, some parameters presented in this manuscript are not discussed, e.g., DIC.

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