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

Thank you for taking your time to review and comment on our manuscript about the nutrient release of Saharan dust in the Atlantic Ocean.

Our experiment was designed to determine biogeochemical effects of dry and wet Saharan dust deposition in the oligotrophic waters of the Atlantic Ocean without adding nitrogen from another source other than Saharan dust. Therefore, we omitted e.g. nitric acid as a N-nutrient source in the wet deposition treatments to assess the potential response of the pico-phytoplankton community from only nutrients of Saharan dust. Such a respond was not observed. However, we found contrasting nutrient releases of phosphate, silicate and dissolved iron of the two dust types we have used, which is what we concentrated our manuscript on.

Still, we are very grateful for your comments and suggestion that we adopted as listed below.

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.

Yes, we agree with the comment given by the reviewer. It is right that we conducted three incubation experiments from which we could only discuss two experiments since the first from the western Atlantic was not successful. Still, we considered it beneficial to the scientific community to show the results from all the experiments. As the data are now already published in the discussion paper, and to increase the readability of the paper, we decided to remove the results from the manuscript.

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.

Indeed, the cell abundances, POC and nutrients are the only parameters we followed to constrain biological responses. During our experiments we tried to filter water for biogenic silica analysis, which was unfortunately unsuccessful. The data we have for silicate is therefore the dissolved nutrient measurements, which indeed decrease in the middle of the Atlantic.

To address the reviewer's point, we added one sentence in paragraph 4.3 of the discussions on the possible response of the diatom community.

'In our experiments in the DL at M3 (Fig. 8), there was a nutrient decrease of SiO_4^{4-} (Fig. 8b), in tandem with a POC increase (Fig. 8g), suggesting uptake by the diatom community, although the dust addition did not result in an obvious increase of the plankton cells (Fig. 8e, f).'

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?

According to the reviewer's suggestion, we removed the paragraph about the aggregation process. The incubated 6 litres of water allowed us to only analyse the final concentrations of POC. Since these concentrations increased with increasing dust amounts added, we speculated about aggregation processes. In the bottles the aggregates were in suspension, or at least brought into suspension once a day for nutrient and cell count sampling. However, we agree that we cannot make a solid argument about dust aggregation processes and have therefore removed the section from the manuscript.

Additional comments

The title should be modified to be more precise.

We changed the title to

‘Nutrient release from dry and wet Saharan dust deposition in the tropical North Atlantic Ocean’

Abstract - L29-30 – not necessary to specify M1 and M3 as there are no additional information for these sites in the abstract.

We removed the station names in the abstract.

‘After an initial increase in cell abundance, a subsequent decrease of these was observed for all experiments, independently of dry- and wet-dust deposition.’

The increase in Synechococcus was probably not related to the additions of dust since the same increase was observed in the control treatments.

Yes, we made it more precise in the same sentence as above.

‘After an initial increase in cell abundance, a subsequent decrease of these was observed for all experiments, independently of dry- and wet-dust deposition.’

Table 1 – I suggest to replace “mg” by “mg/L”

We made the suggested change in the revised manuscript.

P8-L24 – Replace “0 uM” by “below the detection limit”.

As suggested, we replaced 0 µmol L^{-1} with below the detection limit.

‘Concentrations in all treatments were below the detection limit and up to 1 µmol L^{-1} with large error bars pointing to inhomogeneity in the three replicates.’

‘During the 8 days of the experiment, the concentrations were also below the detection limit and up to 1.2 µmol L^{-1} with irregular peaks and large error bars (Fig. 5c).’

P11-L14-16-24 – I suggest to replace “original” by “initial”.

We used ‘original’ for all the CTD baseline. Therefore, we like to be consistent and keep it like this throughout the manuscript. Otherwise it might get confusing with the ‘initial’ nutrient concentrations measured at M1 directly after dust addition at day 0.

P14-L33 –I suggest to replace “nutrient development showed a similar temporal progression” by“nutrient development showed a similar temporal evolution”.

We replaced the phrase as supposed.

‘In contrast, when dust was leached in pH 4.5 rain, all nutrient concentrations remained as low as when dry dust was added, and the nutrient development showed a similar temporal evolution as observed in the control samples (Fig. 8).’

Figures 7 and 8 – Which incubation experiment?

Figures 7 and 8 are the incubation experiments at M3. It is now stated in the figure captions.

Finally, some parameters presented in this manuscript are not discussed, e.g., DIC

Since the DIC values do not show any significant results, but were still analysed, we initially showed them in the manuscript. However, we agree with the reviewer that they are not discussed and therefore, we removed them from the manuscript.