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 #2
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General comments

Mineral dust transported in the atmosphere from arid continental landmasses to the oceanic realm represents a potential supply of bio-limiting nutrients for marine ecosystems. Mineral dust is therefore thought to play a key role in the open ocean biological productivity, and could also enhance carbon export down through the water column due to its contribution to the ballasting of marine particulate matter. The impact of dust on primary production is expected to be particularly significant in HNLC areas where iron is the main limiting nutrient for phytoplankton growth. In oligotrophic regions where phytoplankton development is controlled by phosphorus and nitrogen availability such as the Tropical Atlantic ocean, mineral dust could also boost productivity by stimulating nitrogen fixation. Yet, the impact of Saharan dust inputs across the Tropical Atlantic (by far the largest mineral dust delivery to the ocean) on surface waters productivity is insufficiently documented, and it is still unclear how significant the biogeochemical impact of Saharan dust is. In this manuscript, Korte et al. report on incubation experiments conducted along a trans-Atlantic transect at about 12◦N and designed to further our understanding of the effect of dust delivery on nutrients release, phytoplankton response and particulate organic matter production. As earlier studies, in the Mediterranean in particular, have suggested that the deposition mode (wet vs dry) could have an influence on the nutrient release from the dust, the authors tested the biogeochemical impact of both dry and wet deposition of mineral dust on various Atlantic waters from 23◦to 49◦W sampled at various water depth. Different quantities of dust (sub-mitted -or not- to acidified artificial rain mimicking atmospheric conditions), from two distinct West African sources, were added to seawater to determine whether these factors may influence the response of the ocean biogeochemistry to the dust delivery. Many parameters were analyzed including nutrients (PO3-, NO3-, SiO44-, dissolved iron), particulate organic carbon, and picoplankton abundances. Incubation experimental studies are tricky to set up and so such an extensive effort must therefore be commended. This experiment therefore yielded some important advance for our under-standing of the potential impact of Saharan dust on the Atlantic surface water biological productivity. Among other findings, this study confirms the fundamental role of the atmospheric pre-conditioning of the dust (through acidic cocktails) to allow for nutrients release (PO3-, SiO44-, dissolved iron) and potential impact on ocean biogeochemistry; also, this study highlights the importance of the dust atmospheric

cycle (and its contact with HNO3) for nitrogen release (in these incubation experiments, dust inputs did not result in nitrogen release as the dust introduced in the incubation bottles had not been subjected to atmospheric pre-conditioning). Furthermore, according to the authors, the amount of wet-deposited dust to the Atlantic might be sufficient for biological stimulation via nutrient release, even in the western part of the ocean where dust inputs is much lower than on the eastern side of the basin. Another interesting outcome is that the two different types of dust used in the experiments (from two different dust sources) yielded distinct SiO44- and dissolved iron (while releasing similar phosphate amounts), suggesting dust from different sources may have dissimilar impacts on the ocean biogeochemistry. These outcome should be particularly useful for the set up of seeding experiments in the Atlantic ocean. Also, I find the manuscript well organized, clearly written and appropriately illustrated. I would therefore recommend publication in BG nearly as is. Still, I have listed a few comments/questions below that I hope will be of some use to the authors while working on the final version of the manuscript.

Thank you for the overview into the fertilization topic and your overall positive review and validation of our incubation experiment. As Reviewer #1 had comments on the aggregate formation and the first experiment we conducted in the western Atlantic with the suggestion to remove these results, we decided to do so and shortened the manuscript to gain more focus on the nutrient release findings.

But of course, we are happy to reply to your specific and technical comments as stated below.

Specific comments

page 3, line 20: why would clay material be expected to contain more bioavailable nutrients than coarser (supposedly less weathered?) material?

We clarified our line of thinking by adding an extra sentence on the mineralogy of the two dust types.

'The lake dust was expected to contain more bioavailable nutrients, e.g. silicate and dissolved iron, since lake deposits are often associated with freshwater diatoms and fine-grained iron-containing clay sediments (Scheuvens et al., 2013; Bristow et al., 2010). The dune dust consisted of coarser-grained sediments associated with more refractory minerals like quartz and feldspar.'

pages 4-5, bridging sentence: it is unclear to me what is the reasoning for the addition of 40mL of artificial rainwater (in about 6 liters?) and how this translates into a precipitation rate of 0,04 mm d-1; could you please clarify?

Yes, we agree that the calculation is unclear.

We used the satellite-derived precipitation data at the specific locations of the transect. While it rains most in the western Atlantic and least in the eastern Atlantic, we chose the average amount of rain in the centre of the transect (M3, 0.05 mm d^{-1}). Over 1m^2 of ocean, this amount would translate to 0.05 L (1m*1m*0.00005m=0.00005m^3=0.05L). As the eastern Atlantic receives less rain, we decided to stay below this amount to not overestimate the precipitation rate.

We added this information in the paragraph 2.3 as follows:

'The amount of rainwater was chosen based on satellite-derived precipitation data. During spring, the time of the year in which the incubation experiments were conducted, it rains most in the western Atlantic, while it rains least in the eastern Atlantic (Fig. S2). Given the average precipitation rate of 0.049 mm per day at M3 in the centre of the transect, it would translate to 50 mL of rain per 1 m^2 of water. Therefore, we stayed below this amount to not overestimate the rain. According to Van der Does et al. (2016), a small amount of precipitation is already sufficient to wash out suspended dust from the atmosphere by wet deposition.'

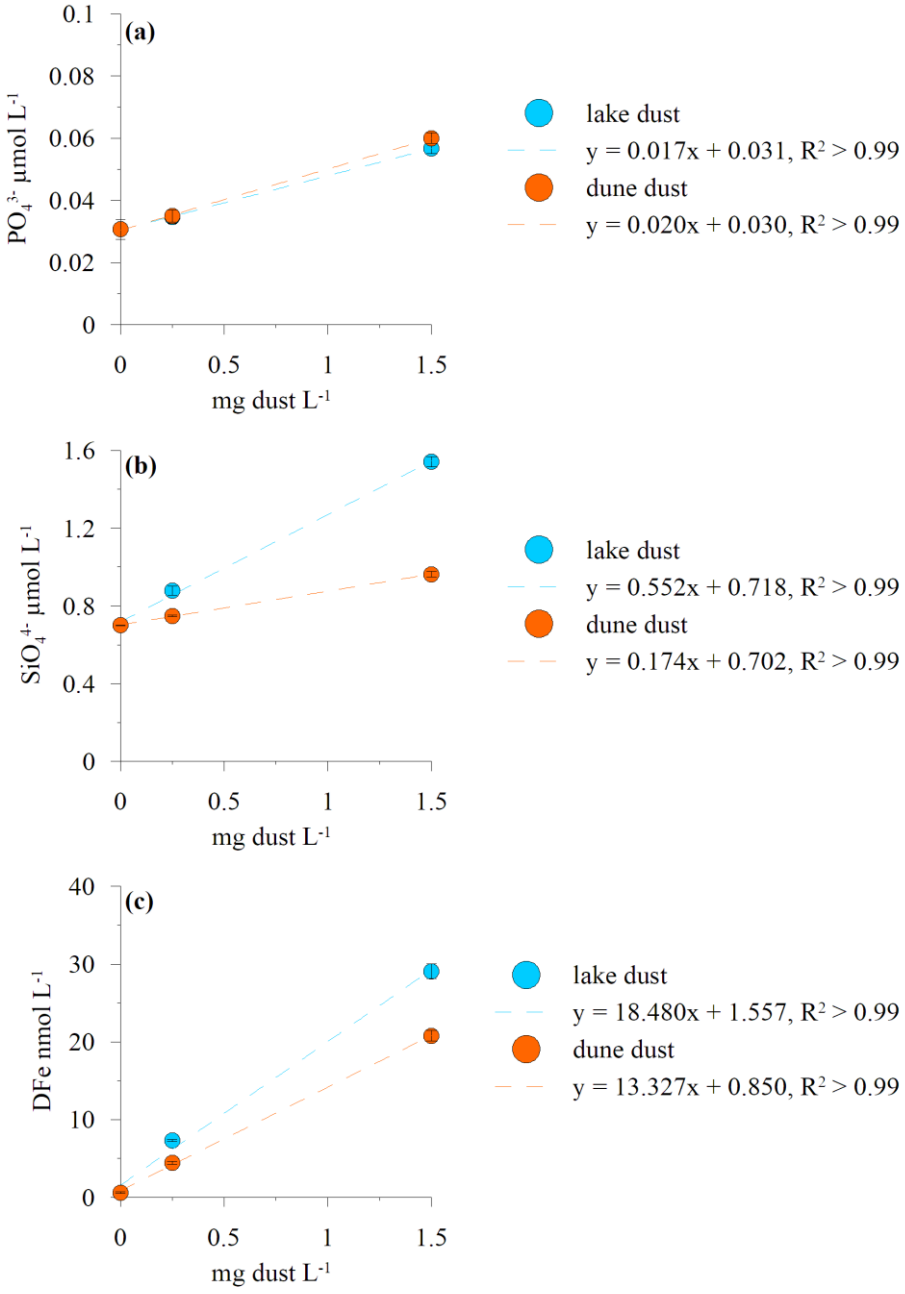
page 13, line 9: is there any evidence (other than the SiO44- concentration) of an Amazonian influence all the way to station M3 in the middle of the Atlantic?

Next to high silicate concentrations, the Amazon River affected water is also low in salinity. During the times when the Amazon River discharge is retroflected into the North Equatorial Counter Current between June and January, the surface water salinities decrease eastward in the open ocean. With salinity observations, it would be possible to trace the Amazonian influence all the way to station M4 in the west, and possibly even to station M3 in the central Atlantic. However, during the time of the incubation experiment in March, the Amazon River discharge was transported with the North Brazil Current in north-western direction along the coast of Brazil. Therefore, we do not mention the possibility of lower salinities in the surface waters by the Amazon River in the manuscript.

page 13, lines 33 and 34: I find interesting that the increase of PO43- is only observed when dust was added in large amounts; wouldn't the relationship between dust andPO43- be expected to be linear (assuming the dust samples were well homogenized)? what could possibly explain the existence of such an apparent threshold for PO43-release?

The phosphorous concentrations do slightly increase with the low dust amounts as well, however the increase is insignificant from the control sample. When looking at the nutrient release after wet dust addition for phosphate, silicate and dissolved iron, there is a linear relationship for both dust types (Fig. below).
We added this figure in the supplement (Fig. S3) and added the information in the paragraph 4.2 in the discussions as follows:

‘Although the nutrient release of dust was linear to the dust amounts added (Fig. S3), a significant increase of PO_4^{3-} concentration was only observed in the experiment when dust was added in high amounts (Fig. 3b and c), while SiO_4^{4-} and DFe showed significant elevated concentrations already after the addition of a low amount of dust (Fig. 4b and c).



page 14, line 6: if the release of PO43- and dissolved iron may promote nitrogen fixation by diazotrophic cyanobacteria, why there was no such response by diazotrophic species in the incubation bottles?

page 15, line 4: the “abiotic” hypothesis for the decrease in nutrient concentration through the experiment raises the question of the bioavailability of the released nutrients; if, as indicated in the text (quoting earlier studies), the elevated pH of

seawater leads to iron precipitation for instance, is the precipitation kinetic known and will iron be available long enough to be used by the phytoplankton?

We could like to reply to the comments on the diazotrophic response and iron kinetic together since they are coupled.

It might be that we do not see a response by diazotrophic species since they demand a high iron concentration in the seawater to thrive. Although the iron concentrations increase extremely in the beginning of the experiment with wet dust deposition, they also decrease throughout the experiment due to complex binding ligands and precipitation in seawater pH (~8). There are also scavenging processes of phosphate back to the dust particles decreasing its bioavailability. As a detailed assessment of the iron kinetics is beyond our expertise as well as the scope of this paper, we can only speculate why the increase in dissolved iron did not lead to an increase in diazotrophic respond. We merely want to demonstrate that several metals and nutrients are leached off dust particles during droplet formation, thus potentially increasing their bioavailability with wet deposition.

We added the following sentences in paragraph 4.4 in the discussions.

'This contrasting observation of nutrient decrease without cell abundance increase, suggests an abiotic decrease of nutrient concentration rather than biological uptake. Diazotrophs are in need of high iron concentrations to thrive (Berman-Frank et al., 2001). Although the DFe concentrations were extremely high at the beginning of the experiment, the iron will be complexed by organic ligands in the seawater (Rue and Bruland, 1995) and the higher pH of the water (~ 8) leads to iron precipitation (Spokes and Jickells, 1996), reducing its bioavailability. In addition, the observed decrease of dissolved inorganic phosphate might be due to scavenging processes back to the (iron-containing) dust particles in the incubation bottles (Louis et al., 2015), inhibiting biological uptake.'

And added the information of abiotic processes in the conclusions.

'Clear evidences for phytoplankton growth in our experiments is however lacking, possibly due to a missing source of new nitrogen-nutrients and abiotic nutrient precipitation processes.'

page 15, lines 33-34: the fact that there is no difference between the incubation bottles and the control bottle does not favor a major role of the dust in the formation of marine snow aggregate, does it?

To determine the role of dust in aggregation formation in our incubation experiments is only speculative. Within our incubation experiment it was not possible to look at the aggregates individually, e.g. if they contained dust. We only judged from the final POC concentrations at M1 that dust might be responsible for aggregate formation since final POC concentrations increase with added dust amounts. However, at M3 this simple relationship is not valid anymore as there is, as you noticed, no difference between the control and dust addition treatment. As

Reviewer #1 also mentions that we cannot discriminate and quantify between newly formed aggregates and increase in micro-phytoplankton cells, we decided to remove the entire paragraph 4.5 as a more detailed analysis on aggregate formation is needed to identify the role of dust.

page 16, line 23-24: again, this seems to me a bit of an overstatement as a significant POC increase in only observed at station M3, and that, in all cases, the incubation bottles do not show significant differences from the control bottles

We agree and we removed the speculations on POC formation as mentioned in the comment above.

Technical corrections

page 5, table 1: shouldn't dust addition unit be mg L-1

Yes, that is true and fixed.

page 6, section 3.1.1: there seems to be a bit of redundancy between the first and second paragraphs

The paragraph might read a bit monotonous, but we think that the comparison between nutrient release of the lake and dune dust gets easier to follow this way.