

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

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(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 (submitted -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 understanding 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

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in West Africa) yielded distinct SiO_4^{4-} 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.

Specific comments

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

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?

page 13, line 9: is there any evidence (other than the SiO_4^{4-} concentration) of an Amazonian influence all the way to station M3 in the middle of the Atlantic?

page 13, lines 33 and 34 : I find interesting that the increase of PO_4^{3-} is only observed when dust was added in large amounts; wouldn't the relationship between dust and PO_4^{3-} be expected to be linear (assuming the dust samples were well homogenized)? what could possibly explain the existence of such an apparent threshold for PO_4^{3-} release?

page 14, line 6 : if the release of PO_4^{3-} 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 nutri-

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ents; 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?

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?

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

Technical corrections

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

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

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