

Interactive comment on “Compositional changes of present-day transatlantic Saharan dust deposition” by Laura F. Korte et al.

Anonymous Referee #1

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Review of “Compositional changes of present-day transatlantic Saharan dust deposition” by Korte et al.

The present manuscript deals with the changes in Aeolian dust and marine fluxes on transect from Western Africa to the Caribbean. Sediment traps were deployed in several depths in the ocean, as well as a horizontal flux sampler was on the continent. Seasonal cycles are discussed. A lot of data material is presented and set into context of previous literature. The paper was a pleasure to read for clarity. Some spelling errors remain, which should be taken care of by the technical editing. I wonder though, whether the title is appropriate, as much of the manuscript deals rather with total mass fluxes and changes in total mass flux composition (including biogenic, carbonate), but a minor part only is dedicated to Saharan dust composition.

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Being on the atmospheric side, I can't comment on the oceanic sediment-specific techniques. A few minor remarks are made below; in particular the XRF data handling should be re-considered.

P6L9: How was the dust removed from the MWAC bottles – liquid suspension or dry?

P9L17: Goossens and Offer (2000) give the efficiency for a size distribution with a mass median of 30 μm (geometric) (their Fig. 8). Van der Does et al. (2016) show mass median grain sizes between 4 and < 20 μm for the sediment traps. How was the size distribution in the MWAC samplers? If the mass median was considerably smaller than 30 μm , a significant overestimation of collection efficiency / underestimation of mass flux would need to be regarded; see Mendez et al. 2016, <http://dx.doi.org/10.1016/j.aeolia.2016.02.003>

P7L33-35: As the intensity readings are used and it is calculated with, I would suggest not calling this qualitatively. It does not become clear from the manuscript, whether the procedure of Weltje and Tjallingii (2008) was performed (i.e. log ratios were calculated, relative detection efficiency and specific matrix calibration for the particular setup were obtained), or whether the raw intensity readings were used.

P7L34-35: “these two elements”: Weltje and Tjallingii suggest a method based on two elements. If you normalize to the total counts here, you do right that, what Weltje and Tjallingii try to avoid by using two different elements. Please consider revising the data processing or write more clearly, if the method of Weltje and Tjallingii is used.

P8L25: If there were wind speed measurements available at the MWAC station, it should be considered giving the information together with the horizontal mass flux, as averages or as times, where the wind speed was higher than an appropriate deflation threshold. That way, it could be estimated, whether emission was locally dominated.

P9L3: This is actually an import.

P9L15 and P9L38: If there are significant differences between the upper and lower

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traps recorded: where does the additional mass come from, where does the lost mass go to? The differences are not small.

Fig. 4 is on the small side. In particular the relative mass percent are difficult to compare. I suggest having an additional column of cumulative/stacked plots in white/gray/green/orange instead of the small separate plots. I would also suggest dividing the x axes by seasons instead of 2-month intervals, as seasons are discussed in the text. Moreover, vertical grid lines at the season boundaries would surely help to read particularly the upper plots.

Fig. 5: What are the diamonds in the modal grain size?

P13L14: “Saharan dust is characterized by exclusively lithogenic elements”: I suggest “We identify Saharan dust by elements”, as in particular K in the atmosphere might have different sources (biomass burning), which might be not relevant in your case.

P13L19: If the procedure of Weltje and Tjallingii was implemented, the following comment can be neglected, otherwise: Correlating relative readings (‘normalized intensities’) can produce quickly spurious correlations, in particular, if one of the major elements shows independent variations (e.g., Ca, Si). I highly suggest not doing that, but investigating elemental ratios instead; e.g., Ti/Al vs. Ca/Si.

Fig. 9: What are the units on the y-axis?

P15L13: What does PSA mean?

P15L18: How can the horizontal flux from the MWAC sampler can be compared to the vertical flux of the sediment traps? At least, information on wind direction is necessary here. Also, it seems to me rather improbable that horizontal surface flux on the Northafrican continent should be closely linked to ocean deposition near the Caribbean, as commonly a lot of dust is transported aloft in the Saharan air lay for longer distances (as detailed below in the manuscript).

P15L20,25: By the notion ‘content’ in relation with XRF, reference is made to a linear

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contribution (mass, volume, moles). This seems in contradiction to the method section, where normalized intensities are referred to, which do not reflect directly a mass without proper calibration.

P16L7: Particle size would play the primary role (over shape) in settling speed, so it should be termed like that instead of the ambiguous “lighter“. Moreover, the silicium-rich quartz and feldspar particles are usually found at larger particle sizes, which can be extracted from the referenced literature, e.g. the Kandler et al. (2009) paper. As result, this compositional change is consistent with the downwind fining.

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