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Interactive comment on "Coarse mode mineral dust size distributions, composition and optical properties from AER-D aircraft measurements over the Tropical Eastern Atlantic" *by* Claire L. Ryder et al.

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Review of "Coarse mode mineral dust size distributions, composition and optical properties from AER-D aircraft measurements over the Tropical Eastern Atlantic" by Ryder et al.

The manuscript describes airborne measurements of mineral dust size distributions close to the source. Representation of aerosol size distributions inside atmospheric transport models is still based on many assumptions, and new data for evaluations and

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input is highly welcome. The manuscript adds new data for the region close to the source at the beginning of the trans-oceanic transport. It also adds additional evidence to the importance of studying particles larger than 10 μm, which are frequently omitted in aerosol research, and which are difficult to study. Furthermore, optical properties for the dust aerosol are reported, which can also serve the community, e.g., as model input or for remote sensing interpretation. The authors show that variation in physical as well as compositional parameters can have a considerable impact on the optical properties, and that in their samples, the compositional variation is dominating the variation in single scattering albedo.

The topic is suitable for ACP. The paper is well-written and clearly structured. References are mostly made where appropriate, some additional comparison with previous work would enhance the general significance. Some issues - particular on the error handling - should be addressed and questions answered before publication.

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Remarks and questions

P3 L32: Are really any inlet size restriction removed? Given the high speeds at which particles are collected, a minimal deviation from perfect alignment might easily introduce boundary layers and re-circulations.

P6 L32: Should this diameter then considered as optical equivalent diameter? Please state.

P7 L10: Have experimental or theoretical approaches been used to characterize the losses?

P8 L6 and Figure 2: It looks like the sizing uncertainties from the number size distribution were not propagated into the volume size distribution (e.g., the CDP point at 20 μm seems to have a factor of 3 uncertainty in size, which should propagate to around a factor of 10 in volume, but a much smaller error bar is shown in the volume plot). Instead it looks like the same vertical error bars are shown in both plots. Please explain or modify.

Figure 2 is hard to understand also from another aspect: Either the measurement points are all at the lower boundary of the error, or the error always includes zero. If they are at the lower boundary, please explain why. In particular with a non-zero counting error (which is necessarily two-sided), this seems impossible. If the error always includes zero, then the statement in P9 L27, “agree within the error bars” is meaningless, as all data could be zero.

P8 L31 and P9 L4: if a particle was not counted for the given reasons – how was this underestimation of the concentration then accounted for? Decreasing proportionally the sample volume?

P9 L6: For what reason the projected area equivalent diameter was not used, which appears to be a common standard in shadowing techniques (i.e. light microscopy)?

P9 L11-21: Please be more explicit on the error handling. Was is done by analytical error propagation, by Monte Carlo simulation, ...? What were the distribution assumptions (Poisson?)? How was the counting error treated, if measurement points with only 4 particles were regarded? If error bars are given: do they show a certain confidence interval, one (or more) standard deviations, or maximum errors?

P9 L30-35: Are there any arguments beyond consistency with previous measurements for disposing (if I understood correctly) the CIP15 CC data? It is stated that the 2DS XY metric is better – better in terms of what? Please explain.

P10 L1: This type of effective diameter seems usually (Hinds 1999) to be called the Sauter mean diameter (apparently defined by Sauter 1926) or mean volume-surface diameter (Hinds 1999). As different effective diameters exist, I suggest referring to one of the mentioned denominations and referring in addition to a more fundamental paper (McFarquhar et al. 1998).

P10 L4-7: Given the high counting uncertainty associated with a single count of the maximum-sized particle, wouldn't there be a better option, e.g., fitting a log-normal distribution or power law into the data? At least, both mentioned approaches should be compared to allow for consistent comparison.

P11 L12 (and below): As magnifications are relative to the used screen, a measure like nm per pixel or pixels per smallest particle would be more meaningful.

P11 L14-19: Wouldn't it make more sense using the same metrics as described for the shadowing instruments above?

P11 L25-29: Please state where you draw the lines between thenardite/sulfate and gypsum/sulfate.

P16 L18-20: This behavior was described and a similar explanation was given for the same geographical region and season a while ago (Jaenicke et al. 1978), and also some model approached seem to predict that (Garrett et al. 2003), so maybe a short comparison with previous finding would make sense here and enhance the general significance of the findings.

P16 L28-30: This finding is surprisingly similar to the volume distributions shown for Cape Verde in winter time, comparing dusty and marine situations (Kandler et al. 2011), so a comparison could show a broader relevance.

P17 L14-18: This would mean that the flow through the filter would have been ten times as high as the one used for calculating the size distributions – is this still in a physical probable range? Can there exist any aerosol concentration effects due to high velocity gradients during sampling?

P17 L22: Smoothness of the curves could be related to different size intervals. The wing probes seem to have more size intervals (with higher counting uncertainties) in Figure 7.

P18 L23-32: Probably a median aspect ratio, instead of a modal value, would make interpretation less dependent on a single interval. In particular, as in the following the values are compared with median values from other sources, which otherwise can't be compared.

P20 L24-25: How about sea-salt reacted with sulfuric acid?

P21 L7-12: How would black carbon have been identified in this work? Were all particle images manually inspected for fractal-like structures (doesn't come clear from the method section).

P22 L20: In particular for the internal mixing, there is plenty of room with respect to complexity for calculating an effective value (Nousiainen 2009; Lindqvist et al. 2014).

P24 L6: In addition, also measured refractive indices show the dependency on iron (Moosmüller et al. 2012; Caponi et al. 2017), so it appears to be consistent in general.

Figure S2: Where does the 'step function' of the imaginary part at 700 nm wavelength derived from?

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Corrections

General: The order of the figures does not correspond with the order of their references in the text.

General: The table numbers in the text don't fit with the table numbers at the end.

P3 L8-11: including into calculation/model ?

P4 L22: Why time-of-flight? Wasn't it an SP2 instrument?

P4 23-25: While the link to the optical property measurement is clear, the reference to the ice nucleation measurements and to the modeling work doesn't seem to add something useful here.

P4 26-30: I suggest removing the paragraph, as the structure is standard.

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P5 L15-22: If results from SAVEX and CATS are not discussed here, these explanations should be removed.

P8 L2: “ambiguities” instead of “singularities”?

P9 L9: What does “Instead, ... sizing metric” refer to? Aren’t the previous sentences the section about the sizing metric?

P10 L8-9: “do not have a minimum detection concentration level, but at low particle concentrations the sampling statistics simply become poor”: In fact, this is the detection limit. With poor sampling statistics the (counting) error becomes high in comparison with the signal. If it is decided to omit data below a certain signal/error ratio, the detection limit is introduced (which is a common procedure).

P10 L14: “PSD’ size distribution” – doubling “size distribution”

P12 L10: “low SEM signal” low image contrast?

P15 L5: increased?

P16 L24: There seems to be no (b) in Figure 6.

P18 L5: In Figure 7, in most cases the volume maximum is below 10 μm, in one above (filter size distribution). “Dominating” therefore doesn’t seem to be appropriate for the largest particles with respect to the mass.

P18 L12: Sentence ends with a comma.

P24 L28: Most of section 4 is rather a summary than conclusions, so the section should be termed accordingly.

Table 1: Different reference formats. Maybe explain instrument abbreviations, e.g., as addition in Table 3?

Table 3: Check caption

Table 4: different time formats between table 2 and 4 (?)

Table 5: 'IT' number format (E+01 etc.)

Figure 2: Both y axes should have the same number format.

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