

Interactive comment on “Observations of Saharan dust microphysical and optical properties from the Eastern Atlantic during NAMMA airborne field campaign” by G. Chen et al.

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Comment: The paper by Chen et al. gives optical and physical properties of Saharan dust observed during the NAMMA campaign of 2006. The results add depth and new substance to the existing literature on the very important topic of dust optical properties and they warrant publication. The paper is well written and the subject matter is appropriate for ACP.

The major issue with the paper stems from the aircraft inlet having a size cut of 4 microns (50 percent transmission). The size cut of the inlet is not an issue for the “closure” analysis because all of the data involved in this analysis was obtained through the

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same inlet. The problem occurs when the authors attempt to say that their measured size distributions and optical properties are representative of the ambient air. Figure 5 clearly shows that the inlet is having a large impact on the volume distribution and the authors themselves point this out. The fact that they are able to derive effective radii that are similar to those from MODIS and that the derived mass extinction efficiency is reasonable is intriguing, but does not necessarily mean that the measured size distribution is correct. The solution to this issue is simple. The authors need to reword key parts of the manuscript mentioned below and mentioned by Referee 1 to explicitly state that the results are for particles less than ~ 3 microns. If this is done, I see no issue with the conclusions they have drawn and I do not see a need to majorly revise the manuscript. It would perhaps be interesting, if the authors have a good curve of inlet transmission, to attempt to correct the data for particle losses above 4 microns and see what the effect is, but this correction is by no means necessary.

The authors successfully demonstrate that the observed optical data can be explained with Mie theory and their measured size distributions. While there is significant uncertainty in the scattering and absorption closures, it stems from uncertainties in nephelometer truncation factors and APS sizing that the authors address, quantify, and identify as areas for further study. A more clear and thorough discussion of composition and electron microscopy results would enhance the paper.

Response: We agree with the reviewer’s comments. We will systematically go through the manuscript and add text to clearly state that the major conclusions are mostly drawn from sampling of the dust particles smaller than $5 \mu\text{m}$ (The correct inlet size cut should be $5 \mu\text{m}$, not $4 \mu\text{m}$) in aerodynamic diameter. This size cut corresponds to dust particle size of $\sim 4 \mu\text{m}$. Table 1 and portions of the text have been modified to expand upon the electron microscopy results. The revision to manuscript will state that due to the incomplete sampling of the dust particles larger than $\sim 4 \mu\text{m}$ the assessed SSA value should be considered as the upper limit while the value of n_i should be used as the lower limit.

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Specific Comments Comment 1): The authors should state in the abstract what the measured size range was. Without this statement, the cited statistics about volume density and VMD could be misleading.

Response: We agree with the reviewer and will add text to clearly define the measurement size range as defined by the sampling inlet size cut. We will also emphasize that this study is based on observations of the dust particles mostly smaller than $\sim 4 \mu\text{m}$.

Comment 2): Line 6, pg. 13452. The authors mention that the heated tube of the “hot” cpc may cause 30 percent particle loss. Is there a size dependence to this loss? Could they be more specific on why this loss occurs? It is not obvious to this reviewer.

Response: This 30% particle loss is primarily due to the thermophoretic effect, particularly when the heated sample flowing into the unheated portion of the sampling line. The post-mission tests showed this loss is nearly constant for particle size larger than 50 nm. Based on the dust particle size distribution displayed in the manuscript, this loss would have minimum impact on the sampling the non-volatile fraction of the dust particles. We would like to note here that the thermophoretic loss does not affect our size distribution and optical measurements as the latter were taken from upstream of the sample line.

Comment 3): Line 27, pg. 13453. Integrated particle volume range should be stated to be 0.7 microns to ~ 3 microns where the inlet cutoff was (far below 20 microns).

Response: Changes will be made according to the reviewer’s comment.

Comment 4): Line 25, pg. 13456. More detailed and quantitative information on the dust composition (especially iron oxide content) would be good if available. Also, more information on the methodology of obtaining this particle type breakdown is needed.

Response: This line (line 25, page 13456) will be removed. This reflects the fact that our analysis did not discriminate between iron inherent in the dust structure and iron present as grains on the surface. Therefore, we cannot positively determine effects on

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the absorption coefficient. We will add more text to describe the methodology.

Comment 5): It would be very informative to show the data for the V10 and V90 curves in Figure 5. While the fit seems good for V50, showing more than one fit on the graph would be much more convincing.

Response: The curve fit parameters are given in Table 3. We did find the labeling in the table is somewhat inconsistent with Figure 5. Changes will be made to clearly link the contents in the table and Figure 5.

Comment 6): Line 26, pg. 13463. It seems clear from the volume data that the impact of the 4 micron cut is not limited.

Response: We will alter the sentence to reflect the reviewer’s comment. Particularly, we will state that the size distributions shown in Figure 5 may be representative of the Saharan dust particle with size smaller than $\sim 4 \mu\text{m}$.

Comment 7): Table 1 shows that 20 percent of the particles in the second SAL layer and even higher percentages of the particles in the MBL were “mixed”. It would be very informative if this “mixed” category could be separated into “coated/mixed dust” (mentioned in the footnotes) and “mixed particles without dust”

Response: We will modify the text and the footnote to clearly indicate that the “mixed” category denotes particles containing dust and other components.

Minor Corrections 1.) Table 3 in the text below the table there is a typo in the explanation for $N > 1$ micron. The text says particles larger than 10 microns.

Response: Correction will be made.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 13445, 2010.

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