

Interactive comment on “Optical properties and mineralogical composition of different Saharan mineral dust samples: a laboratory study” by C. Linke et al.

Anonymous Referee #2

Received and published: 21 April 2006

Review of manuscript: Optical properties of mineralogical composition of different Saharan mineral dust samples: a laboratory study, C. Linke et al.

General Comments: The paper describes measurements of the scattering/absorption properties of polydisperse but size-range limited Saharan dust samples to facilitate modeling of their radiative effects. In addition to bulk measurements of composition, the aerosol properties of the dust were studied with a 3-wavelength photo-acoustic instrument, an aerodynamic particle sizer, and a UV-VIS extinction spectrometer, with additional information about particle morphology from SEM analysis of filter samples.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

The experimental hurdles of these measurements were carefully negotiated by the authors, who clearly present their results in tables and graphs. Their results are also compared to other measurements, adding to the body of knowledge about the optical effects of these types of dust. Most of my questions concern possible biases in results due to the fairly sharp 1 μ m size cutoff, and experimental details.

I found the manuscript quite readable, but think that an improvement in its organization would make the whole more easily digestible. Also, I think that by adding some extra supporting information, the paper will be more easily appreciated by the wider community.

Specific Comments: The presentation of the aerosol generation steps, and the reduction by impactors of the maximum aerosol size was very clear, but would be well supported by a clarification of the effect of cutting off the large size aerosols. I found myself wondering: what are the expected lifetimes for the fraction studied compared to the larger modes? The point that volume is dominated by the “large diameter tail” focuses attention on this issue, especially since most of the actual tail has been removed with the impactors (I assume). I suggest that the authors add some explanation of the significance of the stripped tail, and perhaps a quantified statement of how well the APS size distribution constrains the log-normal fits that are used to model the entire distributions. (I mean: do you cover the peaks in the volume distributions?) A plot of the four sample size/volume distributions would make this clear.

I recommend that the experimental procedures (2.2) section of the paper be filled out. I noticed references to additional equipment/measurements several times in the paper after this section, and found it hard to piece together exactly what had been done with each sample. For example, the Difference Method [Schnaiter et al] was referenced to check the PAS, but without explicit mention the use of nephelometer elsewhere in the experimental section. This also raised the following question: Since nephelometer information is available to get scattering/absorption measurements from your extinction LOPES measurement, why it was not presented in addition to the PAS results?

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Adding this could only make your conclusions stronger if the results are as consistent as those for the CAST soot. Also, I suggest adding a line describing the DM in this paper. Another example of the way this section might benefit from an expansion, is in some additional discussion of Figs. 4-6. These figures are used to support the use of the 1.5 value for the dynamic shape factor for the three samples shown. However I found myself wondering whether the conglomerates might be an artifact of the aeration process - is there evidence supporting the idea that the tested samples are good proxies for naturally mixed dust aerosol? Also, these figures contradict, at some level, the assessment of the “compact” shape of the dust particles and the resulting analysis. Is this significant?

One point of the paper deals with hematite and goethite. In Sec. 4 (mineralogical composition & analysis). An explicit explanation of the importance of these phases might improve the impact of your finding, and support the impact of the comparisons to other measurements. I suggest, too, that you add discussion about the measurement of hematite extinction. From the figure, it appears that the hematite aerosol was monodisperse. In this case the structure in the extinction spectrum is probably a Mie resonance, and the results aren't readily comparable to those of the polydisperse dust aerosols.

Pg 205 line 19: Table 1 clearly indicates that the specific extinction cross section of the dusts are very similar, and that Agadez and Cairo 3 are virtually identical in distribution, with only < 10% difference in #/cc. However, I have trouble understanding how Figure 9 supports this idea. For these two dust samples, specifically, the extinction coefficients in Fig 9 differ dramatically. Do you think this is due to real density differences, mismodeling of size distributions, or some other difference? Or do you refer to figure only to support the idea that the wavelength dependence is similar between the dusts? If so I would recommend normalizing the extinctions by #/cc at least.

Details:

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Abstract: The abstract suggests that all absorption cross sections were measured with the PAS. I think that it follows naturally that SSA would be measured by the difference of the extinction measurement from the PAS measurement. Since this is not how SSAs were measured perhaps you should weaken the language in the abstract.

Pg 2901 lines 15-25: Was the soot polydisperse? If not, was overall absorption changed by diluting flow?

Pg 2902 line 15: I assume that the PAS was only used in the second set of measurements because that's the only time it was available. By stating this openly, questions about why the experiment was split into two parts will be avoided. If this isn't the case, why were two sets of measurements made?

Pg 2902 line 15: It's not clear whether number concentrations and size distributions were measured in both halves of the experiment, or just the first half (w/o PAS).

Pg 2902 lines 20-25. Do you have any concerns about contamination of the Cairo 3 sample by pollution from Cairo?

Pg 2903 line 3: I suggest that discussion of sieved fractions that aren't used be removed.

Pg 2904, line 12: Is the tempering process commonly known? I wonder if the samples were tempered in an inert atmosphere.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 2897, 2006.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)