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Atmospheric Chemistry and Physics Discussions

Interactive comment on "STUDY OF AFRICAN DUST WITH MULTI-WAVELENGTH RAMAN LIDAR DURING THE "SHADOW" CAMPAIGN IN SENEGAL" *by* I. Veselovskii et al.

Anonymous Referee #2

Received and published: 5 April 2016

General Comments:

Careful measurements have been performed in a region (Senegal), which is highly interesting for dust measurements. The SHADOW campaign seems very promising for further dust and dust mixture studies. The 3 case studies and the time series presented are a helpful contribution to the global community of remote measuring groups as measurements close to the Sahara are rare. A good approach to the particle microphysics is shown. So I recommend the manuscript for publication with 4 major concerns and some minor comments.

Major Comments: 1. The title: You should mention the "inversion" in the title, so it becomes more specific. Take care with the title, especially when you are planning a

second part of the "SHADOW" campaign.

2. Chapter 3 "Troposphere Stratification and Dynamics": It is not linked with rest of the manuscript and it is not mentioned in the conclusion. Maybe you should prepare a separate publication dealing with wind information; there you may use the information of the micropulse lidar, too. It was not used for this publication, although it provides depolarization information down to 300m. Or you should present one of your three case studies in this detailed manner. But in total it is still a very nice measurement case to introduce the possibilities of your instruments.

3. Your LILAS lidar: Is there a reference describing the lidar system in more detail? A well characterized lidar system is crucial for the data quality. Why you use 47°? I assume that your entire lidar system is inclined by 47°. Have you measured the transmission ratios (transmittance of cross and parallel polarized light) of your detection unit as they may affect the total signal in presence of heavy dust plumes with high depolarization ratios?

4. Comparing your results to the AERONET retrieval (p16l25) is not the final proof as AERONET is using a particle shape model, too. So AERONET is affected by non-spherical particles with high depolarization ratio, too. It would be better to compare your values to in situ measurements. So I suppose using the depolarization information from a lidar system would improve the inversion.

Minor Comments:

p4l7 "demands the use of assumptions"

p5l21 add the year to the date (2015)

p7l19 add the year to the date (2015)

p8l20-21 "to at" something is missing

p12 chap "10 April" You have not considered the lidar ratio. Please comment on it while classifying the aerosol layers.

p13l15 “in a first guess” (missing space)

p13l18 15μm as a maximum particle radius seems small so close to the desert.

fig 1: add the year to the date (2015); add a description to the color bar;

fig 2: mention "horizontal" wind to not get confused with vertical winds

fig 4: Wouldn’t you expect higher depolarization values for the more continental air (CT)? Lidar ratios for marine particles in Layer A are very high. Could you please comment on this?

Fig 6: "Typically EAE varies in 0-0.3 range, but during dust episodes the values of EAE became negative, decreasing to $\sim$-0.15." It is not seen in the insert of fig 6, as nearly all cases of EAE are in the Angström box "0". You may choose a different scaling for the insert diagram.

Fig 7: To see the variability in the particle depolarization a separate diagram for the depolarization values would be nice. There is enough space to put error bars to every depolarization value without confusing the reader, so it would be a good idea to add the error bars.

Fig 9a: beta 1064 should be shown in red as in the other plots.

Fig 14: Are you sure with the peak of beta532 at 3 km? See description: there are no open symbols (this belongs to fig 15, I suppose).

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