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.

Changed for:

Retrieval of optical and physical properties of African dust from multi-wavelength Raman lidar measurements during the "SHADOW" campaign in Senegal

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.

We understand the reviewer's point of view. Nevertheless, we prefer to keep this section as it is for illustrating the capability of the joint use of both instruments. A separate publication on synergy of wind and Raman lidars is ongoing using extended data sets.

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 deg 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?

No, we didn't publish anything about LILAS yet. A description of the system is under review in a more "technical" journal (AMT). Since the paper is not accepted yet (and could be rejected), we decided to provide a limited amount of informations.

Measurements were performed from inside the IRD building through a window. 47 deg to horizon was the maximal angle possible

Reviewer is right, it is important for dust plumes., We compared transmission at the two polarizations. We have opportunity to use polarization cubes for all three elastic channels in the receiver and after calibration to combine co- and cross-polarized components.

We have added some information about LILAS in the text.

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 nonspherical 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.

AERONET is not the final proof for sure but it fits the whole phase function while difference between spheres and non spheres is significant for large scattering angles. But we agree with reviewer, that problem of validation is the most critical for lidar inversion.

Minor Comments:

p4l7 "demands the use of assumptions"

Corrected

p5l21 add the year to the date (2015)

Added

p7l19 add the year to the date (2015)

Added

p8l20-21 "to at" something is missing

Corrected

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

The sentence is added

p13l15 "in a first guess" (missing space)

Corrected

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

Yes, it would be good to consider larger radii, but maximal wavelength available for us is 1.064 µm and for intervals above 15 µm the retrieval becomes unstable since the sensitivity of the measurements to large particles is very limited.

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

Added

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

Added

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?

Similar comment we had from Reviewer 1. The air masses which call CMT still contain a lot of dust, so it is not pure maritime aerosol and depolarization is quite high through the whole layer A. We have added the comment to the manuscript.

Fig 6: "Typically EAE varies in 0-0.3 range, but during dust episodes the values of EAE became negative, decreasing to _-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.

The scale of figure is chosen to show that EAE is mainly inside [-0.2, 0.2] interval. Uncertainty of EAE calculation is about 0.2, so we think there is no reason to plot statistics with higher resolution. Nevertheless, we slightly modified the text, removing "Typically EAE varies in 0-0.3 range…" because negative EAE is also typical for dust.

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.*

Variation of depolarization is in 20%-35% range, so for such small interval, a separate figure won't bring additional informations.

Although there is enough space to add uncertainties for depolarization, we would need to add also uncertainties for lidar ratios also , which would result in a confusing plot

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

Corrected

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).

To be exact the peak is at 3039 m, so added "approximately".

Yes, it was from fig.15. Removed.