**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Aerosol vertical distribution and optical properties over M'Bour (16.96° W; 14.39° N), Senegal from 2006 to 2008" *by* J.-F. Léon et al.

Anonymous Referee #2

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This papers present an extensive characterization of the climatology of tropospheric aerosol, based on ground based lidar and sun-photometer data acquired over a three years period from the site of M'Bour, in Senegal. The dataset is well presented and its consistency is thoroughly discussed. This is a comprehensive study that surely deserves publication in ACP: the observations and methods are well described and the overall scientific framework is well addressed and referenced, so I consider it as a valuable contribution, both in term of methodology and of results, to the studies of aerosol climatology in a region of the world where observations have been episodical. The paper is thus appropriate for ACP. Minor revisions are thus required, as detailed in the following.

Page 3, left column, lines 28-30: The authors take the altitude 6.5 – 7 km, considered above the main aerosol layer, as the range to calibrate the LIDAR signal. However even in the free troposphere aerosol can be detected at those altitudes, and this can represent a source of systematic error in the LIDAR retrieval. The authors should dwell more on this topic, and give an estimation of the systematic error they may expect.

page 3 right column line 11: more properly, the afterpulse effect (a.k.a. Signal Induced Noise) occurs whenever the light sensor has been exposed to a high luminous signal, irrespective from where it comes – so even form the atmospheric near range (there's no need to quote this, in the present context, but you may be interested in reading Cairo et al. :"A survey of the signal-induced-noise in photomultiplier detection of wide dynamics luminous signals", Rev. Sci. Instrum. ,67, 3274-3280, 1996.)

page 3 right column line 13: Here the authors states that, even after preprocessing the data to account for sky background, Signal Induced Noise, partial overlap, still the signal is not usable below 225 m. Then they should comment whether and how this impacts on the computation of the lidar AOD, in view of the iterative comparison with the sunphotometer AOD they implemented to assess the extinction. A quantitative assessment of the lidar AOD underestimation should be provided.

page 3 left column lines 6-9: Here it is not clear whether the authors implement the iterative procedure to derive the extinction profile from the lidar signal ONLY when the estimation of the lidar ratio from sunphotometric measurements is not available (see page 2 left column lines 38-43), or ALWAYS. In this latter case, I assume the lidar ratio is the one parameter to adjust in order to match the lidar and sunphotometer AOD. Then it would be of extreme interest to compare the lidar ratio obtained with the two differen approaches, and I would invite the authors to do so.

page 4 left column lines 8: Use "altitude" instead of "area".

page 4 left column lines 28: "doest"?

page 4 left column lines 32: How do you consider the first 220 metres where the LIDAR is blind(ed) and the contribution to the AOT can be relevant? Here uncertainties should be quantified.

page 5 left column lines 2: Can this be explained in terms of different transport regimes? The authors should comment more on this.

page 6, right column line 14: (See my comment on page 3 left column lines 6-9) Would it be possible to display in fig. 7 the iteratively derived lidar ratio?

Figure 9 and 11 have axis titles too small.