

Interactive comment on “Synergetic measurements of aerosols over São Paulo, Brazil using LIDAR, sunphotometer and satellite data during the dry season” by E. Landulfo et al.

Anonymous Referee #2

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General remarks:

The paper presents aerosol measurements over an area that is affected both from the mega-city and from the Amazon basin, where few it is known on the vertical distribution of the aerosols. In that sense the paper presents new information that is of interest for the study of the optical properties of aerosols in that region. However before final acceptance there a number of issues that should be improved concerning the way the data are presented and some of the conclusions are too strong relative to the evidence presented.

General comments:

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1. I think that sections 2 and 3 should be joined to make the text flow more consistent. For the same reason in paragraph 3 I suggest to put the description of the trajectory model after the MODIS description and add a paragraph on the methodology the authors use for comparing lidar and photometer data in order to estimate the lidar ratio in order to avoid repetitions later on in the text. For the latter reference should be given to earlier studies with similar approaches (e.g. Marengo et al., 1997, Chazette et al., 1995) as well details on the time and space differences between photometer and lidar measurements.

2. Section 3.4 provides too many technical details for the sensor, which could eventually be shortened, but there is no description of what are the measured parameters used later in the study and what is their quality. This is more important for the discussion.

3. The concept of categorizing the profiles is interesting. However the way this categorization is presented is rather weak. I think that in section 4 the authors should add a paragraph where they should describe the overall concept of each category, relative to the profile shape, to meteorology and to emission sources. As it is written now the reader cannot clearly distinguish the difference between cat2 and cat3.

4. In figure 20 the authors give the distribution of the profiles per category. However, table 2 and figures 1 to 19 show case studies and there is no indication if these cases are indeed representative of the respective category. Summary statistics for the AOT, lidar ratio and Angstrom exponent should be provided either as figures or tables in order to support the category concept. Otherwise the reader cannot judge the numbers presented which in many cases (see pages 845 and 2846) are inconsistent especially for the lidar ratio values shown.

5. Section 5 should be merged in the discussion of 4.1 to 4.3. The authors show trajectories only for cat3 and not for the other two (See also comment no3). However trajectories are very useful for the interpretation of LR and Angstrom exponent values

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6. The last paragraph of the conclusions is not really supported by the results presented, since there is not shown any quantification of each source (biogenic or anthropogenic) in the AOT values observed.

Specific comments:

p2837, line28: It would be helpful to provide some meteorological characteristics characteristics of the dry season

p 2838, line 27: It should be given here an estimate for what is the expected difference between level-2 AOT data and the only cloud screened data. This is essential because both type of data are compared with the lidar ones.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2835, 2003.

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