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Comment

Interactive comment on “Multi-wavelength Raman lidar, sunphotometric and aircraft measurements in combination with inversion models for the estimation of the aerosol optical and physico-chemical properties over Athens, Greece” by R. E. Mamouri et al.

R. E. Mamouri et al.

apdlidar@central.ntua.gr

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The authors would like to thank the reviewers for the helpful comments and technical corrections:

General Comments

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1. The main critical comments are related to the descriptions of measurements, corrections, retrievals, and errors. While the description of lidar retrievals and errors seem sufficient (section 2.1), it is unclear which data have been used for the sun photometer. I guess the authors used level 2.0, but this info is not provided.

Reply: Section 2.2 has been completely rephrased according to the reviewer's comments.

2. Comments about the AOD errors, AE errors and errors on effective radius (fig. 7) and aerosol size distribution retrievals (fig. 8) were omitted.

Reply: The AOD, AE, effective radius and size distribution uncertainties on the CIMEL measurements have been added in revised section 2.2.

3. In section 2.3 the aircraft instrumentation was mentioned but it remained unclear if the measurements were corrected or not, except for the PSAP. For instance the OPC measurements should be corrected for the refractive index and shape; the nephelometer for the truncation and Lambert error. It is unclear if the particles were measured at dry or ambient conditions.

Reply: Explanations and details concerning the in situ measurement are added in the section 2.3 of the new version of the manuscript.

4. The authors provided a reference "Rapsomanikis et al., 2011"; (Int. J. Remote Sens.). However this publication does not exist at the website of the International Journal of Remote Sensing (<http://www.tandf.co.uk/journals/tres>).

Reply: This paper has been omitted and replaced by Kelektoglou et al., 2012; Kelektoglou and Rapsomanikis, 2011.

5a. Later in the manuscript the effective radius derived from OPC measurements was compared without providing an estimation of its uncertainty.

Reply: This has been provided in the revised version of the manuscript.

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5b. Also the refractive index of the particles was determined from aircraft measurements without providing information on the method.

Reply: Information on the method has been added in the revised version of the manuscript.

More specific comments:

6. Page 599, description of figure 1: in several cases the lidar derived AOD does not correspond to the sun photometer derived AOD. For instance on day 24 July, AOD (lidar) > AOD(spm); for days 21,22,23 July, the AOD(lidar) is almost constant, while the AOD(spm) decreases with time. A few explanations would help.

Reply: In the revised version of the manuscript the figures are revised. The reviewer's point was correct. Now the figure is referring only in daytime sunphotometer and lidar data. The procedure for the direct comparison of the retrieval of the two instruments is given in the section 3.

7. Page 601, lines 12ff: description of figure 4: maybe the extinction coefficient derived from aircraft measurements could be included in figure 4 and included in the discussion

Reply: No extinction coefficient data from the aircraft are available.

8. Page 602, lines 1-10: the authors describe how they separate the vertical profile into 4 layers. However they omitted mentioning the vertical smoothing length. This information is needed in order to judge whether the optical data of the 4 layers are independent of each other.

Reply: Information about the vertical smoothing length is now given in the revised version of the manuscript.

9. Page 602, line 18: "10nm to 5mm particle radius" I think, it is 5um (micro meter).

Reply: This part has been corrected in the revised version of the manuscript.

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Page 603, lines 11 and 13: as already mentioned above, error estimation for the in situ derived values is missing. This would help the comparison.

Reply: The error estimation for in situ derived values is given in the revised version of the manuscript.

10. Page 603, lines 16-19: "However, we have to take into account that no change of the value of m with humidity growth was considered in our inversion algorithm which could explain the difference found between the two m values (in situ and lidar-derived)." I don't understand this statement. As far as I understood the inversion algorithm by Müller et al., and Veselovskij et al., the refractive index and effective radius of the size distribution are both an outcome of the inversion. I don't see how the authors want to consider changes in aerosol size and m for the inversion algorithm.

Reply: This part has been completely rephrased in the revised version of the manuscript.

11. Furthermore, radiosonde data (fig. 6) shows that the relative humidity was higher than 90%. It is therefore not surprising that the in situ derived m is clearly lower than 1.4.

Reply: Yes, that is right.

12. Page 605, lines 5-7: "To obtain an equivalent "columnar" reff value from ground up to 3 km height, we averaged the retrieved reff values at the five layers." How was the averaging done? The average should be calculated as a weighted average where the weighting factors correspond to the particle concentration per layer or to the layer mean extinction coefficients.

Reply: Reviewers comment has a point. More explanations are given in the new version of the manuscript.

13. Page 605, line 15: it is not clear why a mean size distribution was calculated. Maybe in the revised manuscript this mean can be erased.

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Reply: The mean size distributions have been removed from the manuscript.

14. Page 614, lines 5ff: Check the reference Rapsomanikis et al. 2011. I couldn't find it at the website of the International Journal of Remote Sensing.

Reply: This has been answered in point 4.

15. Figure 1: error bars for the lidar derived AOD are missing

Reply: Error bars have been added in lidar derived AOD in Figure 1.

16. Figure 3: please check x-axis. It seems that the time-spacing changes at 18:32

Reply: Revised Figure 3 has been added in the new version of the manuscript.

17. Figure 4: I suggest adding the extinction coefficient derived from aircraft measurements. and make some comments in the text

Reply: No aerosol extinction coefficient data are available.

18. Figure 6: For comparison reasons, I suggest to calculate the mixing ratio from radiosonde data and plot them in the right panel together with the lidar derived H₂O mixing ratio and make some comments on the comparison in the text

Reply: The water vapor mixing ratio from radiosonde data was added in Figure 6. Comments were added additionally in the new version of the manuscript.

19. Figure 8: I suggest removing the averaged size distribution

Reply: The averaged size distribution has now been removed from Figure 8 (now named, Figure 9).

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/5/C1364/2012/amtd-5-C1364-2012-supplement.pdf>

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