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Atmospheric
Measurement
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Discussions

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.

Anonymous Referee #2

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The authors presented a case study based on lidar, sun photometer, aircraft measurements and they tried to estimate the chemical composition using the ISORROPIA-II model. There are not so many publications dealing with such a variety of instrumentation and model calculations. Hence the manuscript is of interest for AMT.

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. Comments about the AOD errors, AE errors and erros on effective radius (fig. 7) and aerosol size distribution retrievals (fig. 8) were omitted. 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. 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). Later in the manuscript the effective radius derived from OPC measurements was compared without providing an estimation of its uncertainty. Also the refractive index of the particles was determined from aircraft measurements without providing information on the method.

More specific comments 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.

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

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.

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

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

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. 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.

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.

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.

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

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

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

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

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

Figure 8: I suggest removing the averaged size distribution

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 589, 2012.