

Interactive comment on “Vertical profiles of aerosol mass concentrations observed during dust events by unmanned airborne in-situ and remote sensing instruments” by Dimitra Mamali et al.

Anonymous Referee #2

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General comments

In general the presented material is very interesting and should be published, however I have some comments. Authors stated that mass concentration was derived from in-situ and remote measurements. I would like to see word “derived” in paper title. Article structure seems to be unsuitable for this journal, in my opinion the part related to description of individual cases is a little too long. The paper should rather be about technique then case studies. Authors show AOT and Angstrom Exponent time-series for two locations. Is it crucial for the presented methods or is it placed just to prove that

they compare the same air masses?

More specific comments regarding OPC measurements.

The used OPC has a measurement range much smaller than spectra obtained from CIMEL retrievals. It may be crucial in case of coarse mode. Authors show that 20% of coarse mode and most of fine mode is not covered by OPC spectrum. I can understand that applied correction (page 14, line 14) should mitigate this problem. I suggest fitting bimodal distribution to OPC data with boundary condition applied to CIMEL size distribution with zeros at the ends. Maybe it could help. Authors discussed uncertainties caused by refraction index of material used for calibration of OPC and index of real aerosol. Could they discuss uncertainties caused by aerosol shape? I can suppose that non-spherical aerosol may give different signals than spherical one used for calibration. Another question, in the manuscript is stated that authors used constant density (page 4, line 7) whilst in Appendix, section S1 they mentioned dust particle density and non-dust particle density. How was it really calculated, one density for the whole spectrum or different densities for different modes?

More specific comments regarding POLIPHON.

I understand that retrieval of mass concentration from POLIPHON method bases on assumption that coarse mode is dominated by large depolarizing particles. What happens when coarse mode is a mixture of polarizing and non-polarizing particles. In example mixture of dust and maritime aerosols? This is the case in second episode (page 13, line 4). Maybe, in case of second episode, authors should not apply correction to OPC measurements but to POLIPHON retrievals because assumptions of POLIPHON methods seems to be not fulfilled.

More specific comments regarding case studies.

In my opinion this section is a little too long compared to discussion and Mass Concentration Profiles section. In general Figures containing LIDAR signals, LIDAR quicklooks

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and figures containing mass concentrations have different vertical scales. Unification of scales will help in quick comparison of results. I would also like to see time window of both LIDAR measurement, for LIDAR ratio and mass concentration estimation, as well as for UAV flight. What are colors in Figure 6. I can suppose that are altitudes of trajectory endpoints, it is not defined. Authors estimated LIDAR ratio (LR) around midnight. It is OK for Raman measurements. However during LR measurements and UAV flight different depolarization ratios are observed? Is it really the same aerosol? Increase of depolarization ratio (Figure 5) may suggest some changes. Could you please comment that. A few comments regarding trajectory analysis. I suppose that Figure 6 should prove that air mass originates from northern Africa. However, it passes over southern Italy, Greece and Turkey. Could you please comment possible influence of anthropogenic aerosol on your results. Height of trajectory may be large enough not to capture anthropogenic aerosols. However, it is not clear from the figure and text. The same for Figure 9, trajectories proving that it is dust start at the ground level. What about anthropogenic aerosol from Cairo or Alexandria?

More specific comments regarding mass concentration profiles

I would like to see discussion of uncertainties induced by shape of particles (OPC) and contribution of only polarizing particles to coarse mode (POLIPHON). It is mentioned but not discussed. Second thing. Corrections of OPC mass concentration by POLIPHON mass concentration and integrated volume size distributions makes sense when authors are sure that POLIPHON works well. In my opinion in second case study POLIPHON assumption is not fulfilled. That's why OPC correction is so large. I would rather extrapolate somehow (for example by fitting bimodal function) OPC size distribution and then compare OPC results with POLIPHON one. Regarding comparison of OPC and POLIPHON results. Statements that values are within error bars and provision of pretty large correlation coefficients looks great but could you give more sophisticated statistical analysis? In my opinion it is necessary especially in case of measurements taken in different locations. Could you provide tests for mean values

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or for distribution of mass concentration. It may be done for whole population or for different altitude ranges.

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