Review of “Interrelations between surface, boundary layer, and columnar aerosol properties over a continental urban site”

The focus of this paper is a description of multiwavelength Raman lidar measurements of aerosols within the boundary layer in Warsaw, Poland. Additional measurements also discussed include MFRSR and AERONET measurements of column AOD, and surface measurements of PM. The paper gives a general description of the lidar measurements of boundary layer aerosol properties such as extinction, extinction/backscatter ratio, particulate depolarization, as well as boundary layer height and aerosol optical depth within the boundary layer. The paper attempts to make general statements about the aerosol properties over Warsaw with this combination of measurements. However, for a Raman lidar system that performs “quasi-continuous 24/7 observations”, the paper describes analyses that make use of a relatively small number of these profiles acquired during 2013, 2015, and 2016. Moreover, there were only a few periods during this time when measurements from all sensors were available and coincident so very limited combined information is available.

A major problem with the paper is the attempt to draw general conclusions about aerosol properties, and interrelationships among these properties, from this limited dataset where the correlations are weak at best. Figures 5, 6, 7, 9, 10, 11 display scatterplots which typically show small correlation coefficients (in absolute value) between the aerosol properties, yet the authors attempt to imply that there are some relationships between variables. Similarly, the paper also attempts to indicate that the aerosol properties during some periods were different from the properties observed during other periods; however, looking at the average and standard deviations of these values, it is not at all clear that these differences are statistically significant. In many instances, the paper uses previous measurements to indicate such a relationship exists, when the data presented in the current paper do not clearly support such a relationship. In short, the paper attempts to draw conclusions which the data, as presented, do not support.

1. Page 1, line 27 (associated) would be a better word than attributed.
2. Page 2, line 3. Why no significant correlation between AOD_{ABL} and PM2.5?
3. Page 4. The paper states that the Polly XT lidar performs quasi-continuous 24/7 observations. If so, why are only those measurements collected during the EARLINET observational periods used in the analyses? If this lidar operates nearly 24/7, why not take advantage of this capability and use more data in these analyses?
4. The paper states that there were only 49 profiles available during daytime conditions. Why so few?
5. The description of the periods is confusing. Dusk till sunrise (05-07 UTC) does not sound correct. Dusk starts around 20 UT not 05 UTC. What is “Sunset to down”? Does this mean “sunset to dawn? If so, what time is this? What is the difference between “sunset to dawn” and nocturnal time? These periods need to be clearly described and defined.
6. It sounds like the only periods for which there were coincident lidar and MFRSR data were the daytime profile? Correct?
7. What period of time corresponds to a profile? That is, are these profiles 1 min, 10 minute, 1 hour?
8. Page 6. Line 6. It sounds like some contrast between the summer and winter boundary layer is being made. What is this contrast?

9. The paper does not include any significant discussion or evaluation of the impact of the different in locations among the various measurements. Given the extensive discussion later in the paper regarding correlations (or lack thereof) among these various measurements, there must be more extensive discussion about the impact of the different locations on these correlations. This is particularly true for measurements made at or near the surface.
10. Page 6, line 14. For those readers who are not familiar with the "classical Raman retrieval approach", it would be good to provide a brief (few lines) summary of what is referred to.
11. Page 7, line 3. How many profiles were excluded because of this AOD threshold?
12. Page 7, line 8 What is the basis of the lidar ratio values used for aerosol type determination? Iy may be better to move this sentence to later on this page when discussing other studies of aerosol types.
13. Page 7. Line 13 4-8 should be 0.04 -0.08.
14. Page 8, line 2. Looking at the average values and st. deviations, are the ABL values of aerosol extinction and AOD really statistically significantly different for the sunrise/sunset period than the other two periods? It doesn't look that way. (Likewise for the Angstrom exponent…is it really statistically different for the nocturnal period?)
15. Table 1. It would be helpful to include the ABL height for each period.
16. Page 9, line 17. This sentence "twice larger wavelength" is confusing.
17. Page 9, lines 28-30. These lines show percentages of different aerosol types. How were these percentages determined? What is the uncertainty in these percentages?
18. Figure 2 shows no color (no blue or red symbols); only black and white.
19. Page 10, line 12. The fraction of AOD within the ABL varies widely with location and period, so no blanket statements should be made.
20. Page 10, line 21. Table 2 shows AOD values of 0.15 (355) and 0.07 (532 nm) for EARLINET during cases of no long-range transport. Why does line 21 say 0.2 (355) and 0.1 (532 nm)?
21. Page 11, line 4. There doesn't appear to be much of a anti-correlation between the ABL and column Angstrom exponents in Fig. 2.
22. Page 11. There is no satisfactory explanation given why there is an apparent decrease in AOD (column) with increasing ABLH. The total column amount of AOD should not depend on the ABLH, unless the aerosols that get mixed into the ABL are somehow removed. However, this is not necessarily true nor demonstrated.
23. Page 11, line 28. It seems unlikely that, in these cases where the AOD in the free troposphere is less than 0.3 or 0.4, these elevated aerosol layers reduce solar radiation sufficiently to significantly lower ABLH. If so, the authors should present a more convincing case.
24. Page 12, line 21. Where do the particle densities come from? Also, given these large standard deviations, the difference in these densities among these periods does not look to be statistically significant. Are these significant?
25. Page 12, line 22. There is very little trend of FCMR with ABLH.
26. Page 14. Fig. 7 shows essentially no correlation of AOD within the ABL and surface PM2.5. This should be stated more clearly in the text. This is not what one would typically expect; some more focused discussion about why this was observed would be appreciated,
27. Page 14. Was there any relationship between RH and AOD in the ABL? Or relationship between mean extinction and RH? Also any relationship between RH and the optical properties (LR, Angstrom exponent)?