Comments to
Optical properties of different aerosol types: seven years of combined Raman- elastic backscatter lidar measurements in Thessaloniki, Greece
By E. Giannakaki

I am not quite sure if AMT is the right journal for this contribution since no new technique is presented. Why didn't you choose a journal that is more related to the presentation of measurement results and new findings?

This paper presents the results of the analysis of a seven-year time series of Raman lidar measurements in Thessaloniki, Greece, a heavily polluted city in the region of the eastern Mediterranean. Special emphasis is drawn to the extinction coefficient and lidar ratio at 355 nm as well as the backscatter-related Angström exponent for the wavelength range 355/532 nm. A categorization of several aerosol types depending on their region of origin is discussed.

Before accepting the paper I would recommend major revisions concerning the methodology applied in the study. It seems like no attention has been paid to a seasonal dependence of the observed parameters except for the optical depth. The assumption of a well-mixed boundary layer up to a height of 1.5 km in winter for the classification of the kind or origin of the present aerosol species seems airy. Furthermore, column mean values are used when the properties of certain kinds of aerosols are discussed. Thus, the height resolved lidar observations are sold under value since such a study could easily be performed with a sunphotometer. The advantages of lidar measurements are not fully utilized in this study.
I suggest that the authors focus on the analysis of free-tropospheric aerosol layers since anthropogenic aerosol is omnipresent in the boundary layer at such an urban site.

Regarding the style of the manuscript, I suggest a careful cleaning from redundant content. Papers are cited with little system and sometimes without relation just to fill the list of references. The phrasing needs to be simplified to form shorter and more precise sentences instead of constructions that expand over 3-4 lines.

General remarks:

Sentences are phrased too detailed and often too long, especially in the Abstract and the Introduction. Please use short and plain sentences. Redundant phrasing should be avoided to keep the paper clear and readable. Please screen the paper for needless text. Many sentences could be shortened.

The use of quotation marks is disturbing since it implies (at least to me) that something else is meant.

Citations are unstructured and sometimes inappropriate.

Some aspects of the study reduce the height resolved lidar measurements to column mean values not different from what is derived by applying a sunphotometer. Why is the additional information so carelessly wasted? A discussion of the general vertical extend of the aerosol layer over the measurement site and its dependence on the time of the year would have been a valueable information.

Maybe you should focus on free-tropospheric layers only to screen your profiles from local influences and achieve a more suitable aerosol type classification. Furthermore, you would avoid uncertainties introduced by an incomplete overlap. Your mean OD of 0.52 for clean conditions suggests a huge contribution from the planetary boundary layer that might influence all your profiles.

Specific remarks:

3029, line 8-10: This is hard to believe if you consider industrial regions in China and India, biomass burning in the Amazon or Central Africa, and the regions of strong influence of mineral dust like Western Africa and the Arabian Peninsula. Is there also newer literature to proof this statement?

3029, line 17: Rather use vertical than spatial to state the advantages of lidar measurements. The whole sentence needs to be simplified.

3029, line 24: Wrong citation: it is Ansmann 1992 as is correctly stated in the References section.

3029, line 24: What about particle shape?

3029, line 26: Skip Ansmann 1992 and Müller 2002, 2005 (the latter deal with microphysical properties!) and rather cite Müller 2007: Aerosol-type dependent lidar ratios!

3029, line 28 and later: Skip the vertical in front of profiles. The reader should be aware of this by now. The same holds for the repeated reference to your measurement site Thessaloniki, Greece.

3030, line 20: Is it possible to keep the description of the system as short as possible and refer to a published paper?

3031, line 12: Why is the Angstöm exponent estimated and not simply calculated?

3031, line 19: All profiles in Figure 2 go down to 500 m. How can that be if the overlap correction only allows for a retrieval down to 1000m?

3031, line 21: This paragraph is confusing! Do the errors described in this paragraph originate from the retrieval intercomparison performed in the framework of EARLINET (citation of Matthias 2004a is inappropriate!) or are these estimations of your real measurement errors? If these are estimations of errors of your system, why is the error for Raman backscatter larger than the one for the Klett solution and why is the error for backscatter larger than the one for extinction?
If you want to discuss the errors of your retrieved products, the reference to the EARLINET retrieval intercomparison seems to be redundant.

3032, line 10: How do you account for the change of PBL top height with season of year? Does the PBL top in Thessaloniki reach 1500m in winter (and in the evening)? A modulation of the arrival height of your trajectory with season of year seems more appropriate. Did you check individual measurement for this assumption? If so, mention it in the text.

3032, line 17: This whole paragraph is redundat and can be shortened or dropped. A simple reference to the DREAM model is sufficient.

3033, line 1: Just the facts! Keep this paragraph short and do not describe pictures that are not shown.

3033, line 13: What about the Monday noon measurement?

3033, line 22/ Figure 1: This seems a bit too general. How are these mean values calculated? Do you only consider regions of vertically constant aerosol burden or do you integrate the profile and divide it by the aerosol layer depth? How representative is this mean value if separate aerosol layers occur?

3034, line 8: an unknown backscatter coefficient

3035, line 13: If you calculate a vertical mean lidar ratio for each measurement (and thus simplify your temporally and vertically highly resolved measurements to a single value) you loose all the structures that might show up in the lidar profiles. That does not seem to be desirable. I am not sure if you really can extract any information of a height average of a mean profile representing a time range of seven years! This might include too many different weather conditions. Does the standard deviation of the lidar ratio of 22sr originate in a strong vertical (for each individual measurement) or temporal (different means of individual measurements) variation of your measured parameters? The lidar ratio should by highly variable for individual measurements at your site. How trustworthy are these mean values if the lowermost 1000m of the column above the measurement site are not covered?

3036, line 4: Parts of this paragraph were mentioned early and might not need to be repeated. See notes 3032, line 10. The part describing the different clusters could be shortened, especially since it is mentioned later that this method is not suitable for aerosol discrimination.

3036, line 26: Why do the mean values for the different clusters show such a large standard variation? I would expect more homogeneous conditions for properly selected clusters. Maybe the selection of distinct clusters not automatically implies a selection of different aerosol types. As you mention in the next paragraph, all this seems pretty vague. Do not waste to much time on something you later assess to be obviously insufficient.

3038, line 5: You observe higher wind speed under the influence of high pressure systems? And what is meant with katabatic vertical motion? Subsiding cold air under high pressure? Katabatic wind usually is associated with downslope motion of cold air at mountain ranges.

3038, line 6: Why is it clean continental and continental polluted? Is there also clean maritime or should it be continental clean?

3038, line 9: The data couls easily be screened for the influence of maritime aerosols if you neglect the lowermost 1.0-1.5 km of the aerosol column (being your range of incomplete overlap).

3038, line 16: In the remaining part of the paragraph you state that an observation of distinct aerosol types is hardly possible at your site since usually different types of aerosols are present. However, you aim at obtaining the lidar ratio of these key aerosol types that can not

be distinguished clearly. Focussing on specific events with clear aerosol conditions seems to be more promising. The announced analysis of individual aerosol layers would be more appropriate.

3039, line 5: How can the mean profiles in Figure 2 reach down to 500 m when your overlap effect only permits reliable values down to 1000 m (see instrumental section of your paper)?

3039, line 23: How can you separate the particle size of different aerosol types when you mention earlier that a clear classification of individual profiles to one distinct aerosol type is hardly possible? The Angström exponents of Saharan dust should be well below unity. The values you show in Figure 2 represent rather mixed conditions.

3040, line 7: OD 0.60-0.75 is moderate? This is heavily polluted in most places! Your statement might become clearer if you discuss it with respect to the annual mean OD observed in Thessaloniki.

3040, line 13: OD 0.52 is usually not clean. Local sources seem to influence your measurement. Did you compute trajectories for different height levels or just at 1500 m as stated earlier? I would suggest that you focus on free-tropospheric layers only (see general remarks).

3040, line 18: Is this the same data as presented in Figure 2 or do you now show vertical mean values of individual measurements? In the first case your data look wrong. However, your data contain the information on boundary layer aerosol. You should screen this influence by only considering free-tropospheric aerosols.

3041, line 6: Are such large Angström exponents for Saharan dust reported in the literature?

3041, line 8: These are bad comparisons! You basically compare your findings to the possible range of lidar ratios.

3041, line 25: That is a vague statement. Do your 21 biomass burning cases show vertical variability of your parameters or is it always spread across the whole column?

3042, line 4: Isn’t gas-to-particle conversion associated with new particle formation?

3042, line 5: What is meant with “condensation of large organic particles from their gas phase…”?

3042, line 8 to the end of the paragraph is confusingly written.

3042, line 10: Are backscatter-related Angström exponents compared?

3042, line 24: It is hard to draw any conclusion if you always consider the whole column and drop the advantages of lidar measurements. A statement like this can also be drawn from combined sunphotometer/backscatter lidar measurements.

3043, line 3: What is meant by significantly modified? Somehow pollution (OD=0.5!) must be injected into these air masses. It is hard to see a difference in the extinction values (Figure 2) of continental clean and continental polluted.

3043, line 8: Please shorten your conclusion section.

3044, line 12: That is no finding. It is known that the lidar ratio is size dependent.

3044, line 17: You say smoke particles are larger than dust particles? The lidar ratio increases with decreasing particle size. What about the influence of absorption?

3044, line 26: CALIPSO operates at 532 nm, your system at 355 nm!

3044, line27: You mention that a clear separation of aerosol species is hardly possible at your site. So how can you generalize your findings for distinct aerosol types?

3045, line 1: In this paragraph you state that you cannot discern between different aerosol types for the whole column since you always observe anthropogenic aerosol in the boundary layer. The announced detailed analysis sounds much more promising than this paper.

References:
Please shorten all author lists of more then 10 contributors to XXXXX et al.
Carefully screen your list for unused references.

3047, line 19: Is this paper cited in the paper?

Figure 1, upper left picture: adjust the unit of the mean extinction coefficient.

Figure 2
State the number of cases in the graphs.
Why do the profiles reach down to 500 m?
It is sufficient if you show profiles up to a height of 5 km.
I would also adjust the axes of the backscatter coefficient plots to 20 and 6 Mm^-1 sr^-1, respectively. The lidar ratio plot should only reach to 100 sr. AE from 0 to 4.
The Angström exponent for Saharan dust seems too large.

Figure 3
State the number of cases in the graphs.
Why don’t you use height-resolved correlation plots? The vertical mean includes the boundary layer that might be purely anthropogenic at your site.