

In the spirit of stimulating the discussion, I post the following comments. I will post other comments, or correct some of the following, after another reading of the manuscript.

I refer in the following to the discussion paper page numbering, x-y) mean x-th page, y-th row from the top.

The major points are the following:

Section 3.1)

14061-15) I read "...comparing the probability density function (PDF) and annual cycle of the AOT retrieved from both the daytime lidar measurements and the daily sun-photometer data". But, as far as I can understand from section 2.2, the lidar AOT has been determined varying the lidar ratio to match the sun photometer AOT. So, it seems fairly obvious that the comparison of both quantities and related function (probability density function, averaged annual cycle) is good.

After some thinking, I realised that, since the Cimel is an automated instrument, it is like that the number of photometer measurements is much larger than the number of lidar measurements. Thus, what is called "AOT lidar" is actually the subset of the photometer measurements when lidar measurements are available too. Figure 1 and 2 show that this subset has more or less the same characteristics of the larger set, so that we can conclude that the lidar measurements are representative of a larger sample. Maybe there is some statistical test to state this unambiguously, but I must confess that statistics is not my preferred matter. I suggest to avoid terms that can be misleading, like "PDFs from both instruments" (14062-11), "agreement between both instruments" (14062-11, 14063-3, etc). This is not a comparison between instruments, but a comparison between datasets, and this should be stressed in the text.

We totally agree with the referee that the objective of this section is not to compare instruments but datasets. The text has been modified in that sense. In this section we refer now to "datasets" and no more to "instruments". For the referee's information, the number of sun-photometer measurements is approximately 8.5 times higher than the number of lidar measurements, hence the necessity to check as the referee says that the small subset of lidar measurements "has more or less the same characteristics of the larger set" of sun-photometer measurements.

Note: all modifications in the text have been added in bold.

Section 3.2)

What is the physical meaning of the daytime lidar ratio ? I understand that it is the lidar ratio that makes the lidar-derived AOT matching with the sun photometer AOT, at a certain wavelength. But it is not clear how this parameter is related to the "real" lidar ratio. Intuitively, I would expect that it should be a proxy to an average lidar ratio (defining properly the average), but this should be demonstrated.

The daytime lidar ratio shown in this paper is indeed a proxy of the average lidar ratio in the whole atmospheric column. This sentence has been added in the paper. We

would like to make reference to a paper from Pelon et al. (J. Pelon, C. Flamant, P. Chazette, J.-F. Léon, D. Tanré, M. Sicard, S. K. Satheesh, “Characterization of aerosol spatial distribution and optical properties over the Indian Ocean from airborne lidar and radiometry during INDOEX'99”, J. Geophys. Res., 107, 8029-8041 (2002)) who used the same method than we use and stated that the lidar ratio “retrieved with the single column approach is really an “equivalent” [lidar ratio] representative of properties in the” sounded layers.

Ideally, this lidar ratio should be compared to the lidar ratio obtained from Raman measurements, but I know that it is difficult to perform such measurements in daytime. Maybe, it could be interesting to compare this this lidar ratio with the corresponding quantity that could be derived from the sun-photometer single scattering albedo.

With the UPC lidar system it is indeed not possible to invert daytime Raman measurements. Therefore the comparison of daytime elastic-derived lidar ratio can only be made with nighttime Raman-derived lidar ratios, but given the local atmospheric dynamics in Barcelona, the aerosol stratification is quite different between daytime and nighttime. Such a comparison is not appropriate (see also the answer to the next comment).

A comparison of a sunset measurement with both elastic and Raman algorithms has been made in a recent paper by Reba et al. (M. N. M. Reba, F. Rocadenbosch, M. Sicard, D. Kumar, S. Tomás, “On the lidar ratio estimation from the synergy between AERONET sun-photometer data and elastic lidar inversion”, Proc. of the 25th International Laser Radar Conference, vol. 2, ISBN 978-5-94458-109-9, Saint-Petersburg (Russia), 5 – 9 July 2010, pp 1102-1105) which presents an analytical method to constraint elastic lidar inversions with sun-photometer AOT. It shows that the elastic derived lidar ratio (54.4 sr) and the Raman derived lidar ratio (50 sr) are similar.

At this stage of the paper it is understood and hopefully accepted that the set of lidar ratio values obtained with the method and in the conditions described in the paper is a proxy of the average lidar ratio in the whole atmospheric column (see the answer to the previous comment). Since the paper is not focused on neither the validation of the method nor the comparison between instruments we do not believe that the comparison between the daytime lidar ratio and the sun-photometer-derived lidar ratio needs to be included in this paper. Nevertheless to our knowledge no or very few literature exists about the comparison of those two lidar ratios. The point raised by the referee could probably lead to a publishable work.

Section 3.3)

Could it make sense to compare the Raman derived lidar ratio PDF and cycle with the corresponding quantities at daytime? This could indirectly answer the previous question about the physical meaning of the daytime lidar ratio.

The authors do not think that the comparison between Raman- and elastic-derived lidar ratios would bring new information for the following reasons:

1. Given the local atmospheric dynamics in Barcelona, the aerosol stratification is quite different between daytime and nighttime.

2. Less than half nighttime measurements are available (compared to daytime measurements). Since the lidar dataset is made of measurements randomly distributed in the period 2007-2009 a previous step would be to check if the small subset of nighttime measurements is statistically representative of the monthly variations, which we doubt (this is the reason why we plotted nighttime measurements by season, Fig. 7). The comparison suggested by the referee would make sense if successive daytime and nighttime measurements were systematically compared. This is not the objective of our paper.

About the last point, see also the answer to the previous comment (the conclusions of the work from Reba et al., 2010).

I find interesting that average profiles show a stratification. Similar measurements performed in other cities (ex. Leipzig, Thessaloniki, Lecce) show an exponential decay of the extinction coefficient. This difference is probably due the particular Barcelona orography. This aspect should be emphasized in the text, and possibly explained, in the beginning of the section.

The particular orography of Barcelona mentioned by the referee is one of the reasons but there might be another reason prevailing. Let's recall that the mean profiles shown in Fig. 7 are the average of a certain number of individual profiles uncorrelated one with another: some profiles may contain aerosols only in the PBL up to 1 – 1.5 km and others may contain several aerosol layers up to 4 – 4.5 km. We have recalled with a sentence at the beginning of section 3.3 how the mean profiles were obtained.

However there is an important technical point that should be discussed. We can see from figure 7a that, in winter, the backscattering coefficient decreases going from 0.59 km and 1 km, while the extinction coefficient correspondingly increases. The two measurements are however affected differently by possible systematic errors. In principle, the backscattering measurement is independent of the overlap function of the system, while the extinction is dependent on the derivative of the logarithm of the overlap function. Even if this is almost 1, it could happen that the derivative could affect the value, decreasing the observed extinction. This effect should at least be estimated.

The referee is completely right. The “false” increase of the extinction coefficient in the 590 – 1000 m range interval in Fig. 7a is due to the error contribution of the derivative of the logarithm of the overlap factor. In Appendix B (a new appendix added to the text) we reproduce an analytical formulation that enables to estimate the contribution of this systematic error source.

As an example, if we assume a typical overlap function which reaches 95% full overlap at $R_1 = 590 \text{ m}$ and 100% full overlap at $R_2 = 915 \text{ m}$ the error contribution to the extinction is as high as $\varepsilon_1 = -87 \text{ Mm}^{-1}$ at R_1 and $\varepsilon_2 = -82 \text{ Mm}^{-1}$ at R_2 . It results that the overlap factor error negatively biases the inverted extinction making it lower than its true value. Since the optical alignment of the system is performed several times each year the overlap factor can slightly vary from one alignment to another which prevents to make a systematic correction of it. This explanation has been included in the paper along with Appendix B.

Concerning the backscatter profile the inversion of the backscatter coefficient with the Raman algorithm is independent of the systematic error caused by the overlap function provided both the elastic and the Raman channel have the same overlap function, which is the case for the UPC lidar.

Finally, it is worth mentioning that the UPC method used to invert the extinction coefficient is based on partitioning the full inversion interval into different range intervals (typ. 2-3) where the extinction inversion is performed with different spatial resolutions, usually poorer (i.e., spatially larger) with increasing range, so as to counteract a progressively deteriorated Raman signal-to-noise ratio (SNR). In each of these range-partitioned intervals and prior to compute the derivative of the S-curve (i.e., the logarithm of the ratio between the nitrogen molecular number density profile and the range-corrected Raman signal, see Appendix B) the Raman signal is spatially smoothed to enhance the SNR. Besides, in order to avoid smoothing filter start-up transients affecting the first points of the range 0.59 – 0.915 km, the raw lidar data is cut well below the desired starting inversion range (0.59 km). The procedure is described in Chaps. 6-7 of Reba (Reba, M. N. M.: Data processing and inversion interfacing the UPC elastic-Raman lidar system. PhD Thesis. Universitat Politècnica de Catalunya, Spain (in preparation), 2010), and Section 2 of Rocadenbosch et al. (Rocadenbosch, F., Comerón, A., Sicard, M., Reba, M.N. M.: Statistical considerations on the extinction error variance for the Raman lidar inversion, in Proceedings of IEEE “International Geoscience and Remote Sensing Symposium (IGARSS)”, pp. 2771 – 2774, Barcelona (Spain), July 25, 2007. ISBN: 1-4244-1212-9, 2007.).

Furthermore I see some problems in the presentation of data. What was the criterium to consider data good? The scales of x - axis should be expanded to show also the negative part of the fluctuations. There are some strange height interval. What happens in fig. 7a) between 3.5 and 4 km ? It seems that for some reasons extinction cannot be defined.

The criteria used to consider data good were applied on the extinction and the backscatter retrievals and are the following:

- the coefficient must be positive,
- the associated errorbar must not be higher than 100 %,
- punctually extremely high values (compared to the rest of the profile) were excluded from the profile.

Those clarifications have been added in the text. Because of those conditions, the mean extinction and backscatter profiles are the average of discontinuous profiles, hence the gap in the mean extinction profile in Fig. 7a between 3.5 and 4 km. Fig. 7a has been modified to answer the referee’s comment “An average lidar ratio 42 ± 99 (Fig. 7a) is meaningless ...” and the highest range 2953 – 4326 m has now been removed from the figure (see 2 comments down from here).

The minimum limit of the x-axis has been extended towards negative values in order to show more clearly the negative part of the errorbar.

In fig 7b), in the same height interval, I see a decrease of the error bars. I suppose that it could be related to changes in spatial resolution of the measurements, but this should be explained. I find also strange to find a backscattering close to zero around 3 km and an extinction significantly different from zero. Maybe it is just a

problem of scale (backscattering reduces much more than the extinction), but this should be discussed.

The spatial resolution increases with range (see the answer to the comment “However there is an important technical point ...”) and is different for each individual profile, so that theoretically in certain circumstances the errorbar of the mean profile could decrease with increasing range. This is one possible explanation.

However the main explanation comes probably from the reason given in the answer to the previous comment: in some intervals the number of averaged points is smaller than in others (because data points were excluded) resulting in a misleading decrease of the errorbar.

In Fig. 7b at 3 km the backscatter coefficient is smaller than at 1.5 km and the extinction coefficient as well. It is true that the backscatter reduction looks stronger than the extinction but not significantly much stronger. The average lidar ratio confirms this fact since it is higher in the range 3 – 4.3 km than in the layer centered around 1.5 km.

In order to clarify how Fig. 7 was obtained (along the last 3 comments from the referee), a thorough description of the average computation and the criteria used has been added at the beginning of Section 3.3.

An average lidar ratio 42 ± 99 (Fig. 7a) is meaningless. It could make sense to say that the average is somewhere between 0 and 150, but negative lidar ratio should not be considered in the average. If they have not be considered but the estimated variation leads to negative values, I would say that the statistical hypothesis at the basis of the ML method is not verified. Maybe the simplest is to remove this point.

The referee is totally true. As said in the text “In winter virtually no aerosols are present above 2500 m”. The errorbar of 99 sr comes from the simple fact that very few profiles are available in the range 2953 – 4326 m and that they are highly variable. We decided to simply remove this range and to truncate the winter profiles at 2953 m. The text has also been slightly modified accordingly.

Minor points

14058-16) It is well known that, for analog signals, there could be a problem of timevariable background. Has this effect been taken into account?

The background power signal is subtracted out of each individual signal profile, i.e. every minute, by using the least-square Rayleigh fitting method (for more details see Reba, M. N. M.: Data processing and inversion interfacing the UPC elastic-Raman lidar system. PhD Thesis. Universitat Politècnica de Catalunya, Spain (in preparation), 2010). The background-subtracted profiles are then averaged (over 30 and 150 minutes for daytime and nighttime measurements, respectively) before the inversion. This information has been added in Section 2.2.

14059-7) It is not clear wether the extinction is that derived from the Raman signal or from the elastic backscattering multiplied by the lidar ratio (or both)

The lidar-derived AOT has been calculated only for daytime lidar measurements in the purpose of comparison it with daily values of the sun-photometer. Therefore the extinction has been obtained from elastic backscattering multiplied by the lidar ratio. It has been clarified in the text that the AOT was calculated for daytime measurements only.

14059-19) I know that Earlinet measurement are scheduled at 13:00 UT +/- 1 hour, and at a first glance it seems that Barcelona is more or less at the same longitude than Greenwich, so I should say that 13:00 UT is not the same than 14:00 LST. Unfortunately I cannot find an official document stating what is the scheduled Earlinet day time, the authors should check.

Spain has 1 hour difference in summer (2 hours in winter) compared to UT, just like France or Germany.

Anyway regular measurements within EARLINET are stated in an internal webpage of the EARLINET-ASOS project (EARLINET internals/Data archive and use/Schedule). Here is a copy and paste of that webpage:

“Schedule of measurements

The consortium agreed on a common schedule of three regular measurements a week. They will be done on

Monday at 14:00 LST (local solar time) $\pm$ 1 hour and at sunset -2h +3h and on Thursday at sunset -2h +3h.

For climatological measurements the typical averaging time is 30 minutes, shorter periods are accepted if the height range is increased significantly (measurements with broken clouds).”

14067-25)

" We believe that the increase of the PBL height in June and July is partially due to two factors: 1) the constant increase of incoming solar radiation between June and August" To my knowledge, the solar radiation (I suppose it is meant the solar radiation flux) increases, in the northern emisphere, up to the summer solstice, then it decreases in July and August.

Intrinsically the referee is right: the solar radiation flux start to decrease from June on. But if we take into account the cloud coverage, the solar radiation hitting the surface in Barcelona is stronger in the months of July and August than in any other month of the year. The sentence has been replaced by: “the strong solar radiation reaching the surface between June and August”.

14067-27) " ..the main holiday exodus from the Barcelona area during July and especially August. The first point has two opposite effects: the growing of the convective PBL and the amplification of the Iberian thermal low which prevents the vertical development of the PBL. " The growing of the convective PBL could be a reason of the observed increase of the PBL height, but I suppose that it is due more to the increase of the turbulent flux at the surface, grossly proportional to the soil-air difference, than to the increase of the solar radiation.

We thank the referee for this comment. This is actually an important point that has been added to the explanation of the convective PBL growth in June-July. The soil-air temperature difference which depends on the solar radiation reaching the surface is now stated as the principal contributor to the convective PBL growth.

I do not understand the second part: if the Iberian thermal low, which prevents the vertical development of the PBL is amplified, why the effect of this should be an higher average PBL ?

Sorry for not being clear. By saying at the beginning of the sentence “The first point has two opposite effects” we mean that it simultaneously contributes to increase and decrease the PBL height. Both opposite effects have been clearly written in the text as “the growing of the convective PBL” and “the decrease of the convective PBL”.

14068-1) "The second point induces a drastic decrease of the PM levels emitted at ground level (Pey et al., 2010) and thus a decrease of the number of aerosols to be mixed in the convective PBL. " OK, but what is the relation with the PBL height ?

This second point has been removed from the text.