Answers to interactive comments from Referee #2

General
The paper is well written and contains original material (long-term observations of aerosol optical properties with aerosol Raman lidar). The paper is appropriate to be published in ACP.
The observational part, especially the 1064nm lidar observations and the Raman lidar observations, needs revision.
Minor revisions are required.

Page 14053, Title: Because Barcelona is an EARLINET site, I would prefer to see 'EARLINET' in the title.

The title has been replaced by "Seasonal variability of aerosol optical properties observed by means of an Raman lidar at an EARLINET site over Northeastern Spain".

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

Page 14054, line 3: Elastic-Raman lidar: What does 'elastic Raman (scattering)' mean? In section 2.2 you state that elastically backscattered radiation is separated from Raman-shifted backscatter,: : :. So your lidar obviously is an elastic-backscatter Raman lidar or a classical aerosol Raman lidar (with 1064nm channel).

"Elastic Raman lidar" has been replace by "Raman lidar" throughout the text.

Page 14054, lines 8, 10: please use 'are' analyzed, etc. instead of 'have been' analyzed.

From this sentence on, the abstract has been changed to present.

Page 14054, line 16: Raman inversion? Elastic-inversion? Be specific and clear: Raman lidar method, elastic-backscatter lidar data analysis, retrieval of particle optical properties from Raman lidar measurements, retrieval of optical properties from elastic backscatter signal profiles: : :..

The sentence has been replaced by "A detail study of a special event including a combined intrusion of Saharan dust and biomass-burning particles proves the suitability of combining the retrieval of aerosol optical properties from Raman and pure elastic lidar measurements to discriminate spatially different types of aerosols and to follow their spatial and temporal evolution.".

Page 14055, line 6: You state that Western Med. Basin is one of the most polluted areas in the world, later in section 3.1 you state that the average optical depth is just 0.14 (500nm). So, the Barcelona optical depth is rather low. Therefore the above statement should be removed.

This sentence has been deleted. The authors also realized that it was a little bit out of the context of this paragraph of the introduction.

Page 14060, line 18: Please add the reference: Pappalardo et al., JGR, 2009, on the ESA-EARLINET-CALIPSO activities: : :

The reference “EARLINET correlative measurements for CALIPSO: First intercomparison results“ by Pappalardo et al. and published in JGR in 2010 has been added.

Page 14062, line 2: The agreement of the lidar and photometer statistics in Figure 1 is not that good. Please explain the large discrepancies for the low 1064nm AOT range.

The low AOTs at 1064 nm (and also at 532 nm) occur during winter months. The large discrepancies between both datasets (the lidar and the sun-photometer dataset) are mainly due to the explanation given in p.14063, line 16-20: “They are due to a lesser number of lidar measurements (which causes the errorbars not to be representative of the statistical sample) due to either bad weather or a lack of sun-photometer measurements, sunset measurements being performed at nighttime during these months of the year.”
The text has been slightly modified in order to make the explanations given for the discrepancies observed in Fig. 2 also valid for Fig. 1b.

Page 14064: A similar study by comparing total AOT versus PBL, FT (free troposphere) AOT was done by Mattis et al. (JGR, 2004). Please compare and discuss the differences!

The authors thank the referee for this valuable comment: the paper by Mattis et al., (JGR, 2004) is indeed very interesting. The paper has been referenced and a short comparison has been added at the end of Section 3.1.

Page 14065: A general point: The 1064nm backscatter coefficient retrieval is highly sensitive to uncertainties in the reference value (signal calibration) and may be to uncertainties in background subtraction. So, the uncertainties of the 1064nm backscatter coefficients are large (may be 20%-100%). In view of these uncertainties, what does an error of 1sr mean here? You state that the lidar ratios at 1064nm are about 20sr lower than at 532nm. I have never seen such a difference in the literature. There must be something wrong with the analysis? According to Ackermann (lidar ratio paper, 1998) the only aerosol type that may produce a large difference seems to be maritime aerosols (20-25 sr at 532nm, 40-45 sr at 1064nm). No other aerosol component can do that. But you observe even the opposite, i.e., larger 532nm lidar ratios than 1064nm lidar ratios. A potential systematic error source is always the reference value (in the reference height in the free troposphere). May be the reference value for the 1064nm particle backscatter

coefficient is systematically too large? Or the background subtraction is not ok? Please re-check the 1064nm results. The lidar data for 532nm seem to be ok.

The reference value in terms of aerosol backscatter coefficient is always taken in the free troposphere assumed "aerosol free" and is fixed to 10^{-5} $Mm^{-1}.sr^{-1}$. We assume that such a small value generates a negligible error on the final profile of aerosol backscatter coefficient.
The background signal is calculated averaging the lidar returned power between the ranges 20 – 24 km where the signal has reached noise level.
The errorbar of 1 sr represents the error due to the method. It does not reflect any other source of error. This has been clarified in the text.
Given the close deadline of the discussion paper closure (3 August), the complete check-out of all inversions at 1064 nm is not possible before the discussion closes. We would like to propose to the referee two options listed here in order of preference of the authors:

1. The discussion about the lidar ratio at 1064 nm in Section 3.2 is simply deleted. The paper can be modified immediately once the referee gives his/her ok.
2. All elastic inversions at 1064 nm are re-checked. The modifications to the text and the figures could take another two months from now and an extension of the revision process would be asked to the editor.

There are further papers on desert dust lidar ratios, please check Amiridis et al. (JGR, 2005?), de Tomasi et al. (Appl. Opt., 2003?), and especially Tesche et al., (Tellus, 2009). Tesche et al., provides desert dust lidar ratios for 532 and 1064nm and Angstrom values for the 532/1064nm range for pure dust in Morocco.

By looking at those papers it appears that a typical value of lidar ratio for mineral dust ranges between 50 – 60 sr. The text has been changed accordingly and the above references have been added.

Page 14068: A general point: The Raman signal profiles obviously need to be smoothed with much larger vertical smoothing lengths than done in the paper. The fact that there are rather strong lidar ratio variations points to the direction that the Raman signals were still too noisy for a proper extinction coefficient retrieval.

This point has been partially answered in the answers to the first referee's comments.

1. 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). Typical smoothing lengths are 30 – 40 samples (225 – 300 m) in the lowermost layer and 90 samples (675 m) in the uppermost layer. In our opinion those lengths are not small. By taking larger smoothing lengths the inversion, especially in the lowest layers, could be biased because each smoothed sample could be the result of different aerosol types.
2. 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.

  Because of those conditions, the mean extinction and backscatter profiles are the average of discontinuous profiles, hence the jumps and variations in the mean extinction profile in Fig. 7a and 7b. We think that the strong lidar ratio variations come more from the variety of the individual profiles than from too small smoothing lengths.

3. We also recall in the paper (following the first referee's comments) that "Let's recall that those mean profiles are the average of a certain number of individual profiles uncorrelated one with another: some profiles may contain aerosols only in the PBL and others may contain several aerosol layers above the PBL. Because of the Raman algorithm procedure itself the individual profiles do not start and do not end at the same heights, neither the interval slicing nor the inversion spatial resolutions are the same.".
4. The profiles of Fig. 7a have been cut above 2953 m because very few values were available in that range which in winter is free of aerosols (as said later in the text).

Page 14069, line 5: You mean:: : :mixture of PBL and free tropopsheric aerosols: : :. If so, please use free tropospheric aerosols to denote the lofted aerosol.

"Free tropospheric aerosol" have been used now in this section to denote lofted aerosols.

Page 14071-14073: Special events: There are several papers with lidar ratios for smoke and dust, please check, e.g., Muller et al. (JGR, 2007?), aerosol-type dependent lidar ratios: : :, and again, please use Tesche et al. for dust comparisons.

The paper of Tesche et al. (Tellus, 2009) has been used in the discussion about lidar ratios of dust. The paper of Müller et al. (JGR, 2007), although interesting, is a bit general and is already cited by Tesche et al. (Tellus, 2009).

Page 14074: The conclusion section is much too long, should be of the order of 0.5-1 page, not more. Please summarize just the most essential findings.

The conclusion has been summarized and fits in 1 page now.

Figures:

Figure 4: I would like to know how many cases (observation days) are considered for each defined class, these numbers could be given at the top of each plot, just above each class. That would help to find out how trustworthy the statistics are.

Those numbers have been added above each bar in Fig. 4a and 4b.

Figures 5 and 6: The same, please provide numbers of observations per interval.

Those numbers have also been added above each bar in Fig. 5b and 6b.

Figure 7: The grey areas should be mentioned in the figure caption. However, these error areas are strange. I would always expect an increasing width of the grey area with increasing height (caused by increasing signal noise). As long as the shape of the extinction profile is not similar to the respective backscatter profile, I would not trust the extinction profile. Figure 7b is already in a much better shape than Figure 7a.

The grey areas have been mentioned in the figure caption in the revised manuscript.
Please don't forget that Fig. 7 represents mean profiles! The spatial resolution increases with range (see the answer to the comment "Page 14068: A general 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.
Another possible explanation comes probably from the reason given in the same answer (see the answer to the comment "Page 14068: A general point …"): 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.
As far as Fig. 7a is concerned, the profiles there have been cut above 2953 m. Below 1 km the "false" increase of the extinction coefficient is due to the error contribution of the derivative of the logarithm of the overlap factor. In response to one of the comments from the first referee, an analytical formulation that enables to estimate the contribution of this systematic error source is now given in a new Appendix, Appendix B, and a simple estimation of this error is made in the text.

I would recommend to re-check the entire Raman lidar data set and to use much longer smoothing lengths. The same smoothing length (used in the least squares retrieval, extinction) must then be used in the backscatter retrieval. Afterwards the respective extinction-to-backscatter ratios can be calculated. Afterwards, you can average the lidar ratio profiles to obtain the seasonal mean lidar ratio profiles.

We ask the referee to see the answer to the comment "Page 14068: A general point …".
In our opinion the smoothing lengths used in the Raman retrievals are not small. By taking larger smoothing lengths the inversion, especially in the lowest layers, could be biased because each smoothed sample could be the result of different aerosol types.
The method to calculate the lidar ratio profiles described by the referee is the method the authors used.
In the revised manuscript we have realized a significant cut in Fig. 7a. In Fig. 7a and 7b the error bars of the extinction coefficient profiles (representing both retrieval uncertainties and atmospheric variability) are relatively large which demonstrates that the profiles obtained of extinction are not very reliable. This is an information and we think that even in those conditions our results in terms of backscatter and lidar ratio profiles are interesting enough to be worth publishing. Now, if the referee still believes

that all our Raman inversions should be re-checked, then the authors will ask the editor for an extension of the revision process.

The average extinction profile in Figure 7a (below 1 km) is not trustworthy for heights below 1 km. Are overlap effects properly corrected?

The referee is totally true. This question has been taken care of in the answer to the first referee.
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 $R_1 = 590\,m$ and 100% full overlap at $R_2 = 915 \ m$ the error contribution to the extinction is as high as $\varepsilon_1 = -87 \ Mm^{-1}$ at R_1 and $\varepsilon_2 = -82 \ Mm^{-1}$ 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.

The error bars show what? Uncertainties caused by retrieval uncertainties or just atmospheric variability or both? Please, state that clearly!

The way the error bars were calculated is described at the beginning of all the paragraphs that deal with the discussion of a figure. In all those paragraphs a brief statement about what the error bars show has been added in parenthesis. In general they show the atmospheric variability. In the case of Fig. 7a and 7b they show both retrieval uncertainties and atmospheric variability.

Plate 1: The lidar profiles (extinction, lidar ratio) in Plate 1b are again not trustworthy (unacceptable). Much longer smoothing lengths (or least squares fit intervals) are required.

In that figure, 3 regions were considered: [316 – 1839 m], [1839 – 3999 m] and [3999 – 6044 m] where 51 (382 m), 61 (457 m) and 91 (673 m) samples were smoothed, respectively. Also see the answer to the comment “I would recommend to re-check the entire Raman lidar data set…”!
If the referee still believes that a new Raman inversion of the measurement presented in Plate 1 should be performed, then the authors will ask the editor for an extension of the revision process.