Atmos. Chem. Phys. Discuss., 10, C5664–C5667, 2010
www.atmos-chem-phys-discuss.net/10/C5664/2010/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal variability of aerosol optical properties observed by means of an elastic-Raman lidar over Northeastern Spain" *by* M. Sicard et al.

Anonymous Referee #2

Received and published: 23 July 2010

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.

Details

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

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).

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

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....

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.

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

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.

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!

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

ACPD

10, C5664–C5667, 2010

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.

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.

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.

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.

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.

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.

ACPD

10, C5664–C5667, 2010

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.

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

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.

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.

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

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14053, 2010.