The authors present a study of cirrus morphology and optical properties (depolarization, optical thickness and LR), based on a short term measuring period. For the identified cirrus layers the cloud optical properties for the 532-nm wavelength were derived and an empirical multiple-scattering correction was applied, based on the optical depth values. Authors also present a trajectory analysis giving some possible links between the optical characteristics of clouds with the thermal and convective history of the air mass. The main results of the study are of interest. However, the methodology part needs further work and improvements, as even the cloud boundary detection is not clearly described. The paper is suitable for publication in ACP. However, I recommend a major revision of the manuscript, after considering some general and specific issues detailed below in my review.

General comment

According to authors, the detection of the cirrus clouds is made after applying the following threshold: Threshold values of 1.15 for BR. With only this threshold, are authors confident that they exclude from the analysis any transported elevated layer? Moreover, have you checked the SNR, before applying the cirrus detection?Using a standard BR value, can be applicable to cases that no systematic errors occurred (e.g wrong background subtraction). However, the SNR (signal to noise ratio) should also checked.

How can authors explain the detection of cirrus clouds with depolarization values less than 10%? Figures 7, 8, 10, 12, 13 are reporting cirrus depol values starting from 0%? Have you checked the SNR of these causes? The depolarization values are really surprising for cirrus clouds. Moreover, the integration time of 5-min average, could have restrict the accuracy of the depolarization ratio.

Concerning the multiple scattering correction, I would suggest the authors to make clear that the derived optical depth, may contain significant biases due to the multiple scattering, corrected by Chen et al., (2002), with eta depending on the optical depth of the cloud layer. The authors, should justify better, the reason for adopting this approach. Moreover, the authors claim that (Page 6 , Line 177) “It tends to produce observed extinctions and depolarization respectively smaller and greater than the real (effective) ones”. However, few lines latter, they state that “No corrections were made to the backscattering and depolarization coefficients.” How can you deal with that? Is this the reason for the very low depol values presented? The authors should provide more details and explanation.

Authors claim that “When this approach does not produce results, due to optical thickness too small or noise of the profile below and/or above the cloud, a fixed value of LR=29 sr was assumed (Chen et al., 2002)”. The authors should provide more details about the errors introducing in their statistics with this choice.

The authors claim that LR is indicative of small particles. However, they should be more careful, as this parameter depends also on the particle orientation relative to the laser beam.

The structure of section 3 separates the Clouds vertical distribution and morphology (#3.2) and Clouds optical properties (#3.3). However as it is written and structured the text introduces optical properties (optical depth values) in the first part (#3.2). I propose a reconstruction of these Sessions or a change on the title of the second session.

Terms should be clearly defined the first time they appear in the manuscript (e.g. Line 4 UTLS). Also, replace the abbreviations in the Conclusion part.

Please provide more references in the Discussion part.

Specific comments

You are referring to the measuring period either from 15 February to 25 March or from 16 February to 25 March. Please correct.

Page 5 line 129. “An absolute calibration of the channels gain ratio was also performed before the deployment”. Authors must provide details for the procedure.

Page 6, Line 179. The authors claim that “Different correction algorithms have been proposed although there is still no consensus on a univocal and rigorous correction method”. Please check and provide as references the model of Eloranta, 1998 and Hogan, 2006.

Eloranta, E.: Practical model for the calculation of multiply scattered lidar returns, Appl. Optics, 37, 2464–2472, https://doi.org/10.1364/ao.37.002464, 1998.
Hogan, R. J.: Fast approximate calculation of multiply scattered lidar returns, Appl. Optics, 45, 5984–5992, 2006.

Figures 2b and 6. The color scale used makes the figures hard to read and to distinct values, especially for values close to zero. The authors should consider choosing a different color map.

Page 12, Line 357. “Depolarization increases with height and generally decrease with temperature”. Please provide references to enhance this statement.

Figure 5. Why cirrus clouds with geometrical thickness less than 150m are plotted in the Figure? According to line 254, page 9, thinner clouds than 150m are excluded from the analysis.

Figure 2. The size of the axis labels should be improved to be readable.

Figure 3. Replace “Distribution of Backscatter Ratio observatons vs altitude. Data are 5 min averages of lidar vertical profiles, with 30 m vertical resolution. The colour codes the number of samples in each bin. Only data with BR>1.15 have bee reported” with “Distribution of Backscatter Ratio observations vs altitude. Data are 5 min averages of lidar vertical profiles, with 30 m vertical resolution. The colour codes the number of samples in each bin. Only data with BR>1.15 have been reported”.

Page 12, Line 360. Double “in” written.

Page 10, Line 288. Replace “obsservation” with observation

Page 3, Line 84. Replace airmasses with air masses

Page 7, Line 191. Replace “R” with “BR”

Page 7, Line 196. Replace “airmasses” with “air masses”

Page 7, Line 209. Replace “theair” with “the air”

Page 8, Line 236. Replace “,” with “.”

Page 9, Line 272. Replace “behavious” with “behaviour”

Page 10, Line 310. Replace “airmass” with “air mass”

Page 12, Line 376. double “of”

Page 12, Line 360.