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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optical and geometrical characteristics of cirrus clouds over amid-latitude lidar station" *by* E. Giannakaki et al.

Anonymous Referee #1

Received and published: 20 August 2007

Title: Optical and geometrical characteristics of cirrus clouds over a mid-latitude lidar station

Authors: Giannakaki et al.

General:

The authors present statistics about high-altitude clouds at mid-cloud temperatures below -25 °C that were observed between 2000 and 2006 with lidar over Thessaloniki, Greece. The central topic of the paper is the comparison of different retrieval schemes (optical depth, lidar ratio). This is a strong point of the paper. Two different ways of multiple scattering correction are presented as well. The weak point of the paper is that obvioulsy only 65 cirrus cases were observed from 2000-2006. The cirrus climatology presented in this paper is based on this small set of data. Furthermore, a questionable point in this paper lies in the definition of cirrus clouds. Mixed-phase and liquid-water clouds can occur down to temperatures of -38°C. Hence, accounting all clouds with mid-cloud temperatures below -25 °C to cirrus clouds is rather problematic. Especially when the lidar does not detect a depolarization signal which would allow the authors to distinguish water and mixed-phase clouds from pure ice clouds (cirrus). The Thessaloniki lidar obviously does detects a depolarization signal. Thus the authors have to present the method how they distinguish cirrus clouds from water and mixed-phase clouds at temperatures between -25 and -38 °C. Alternatively, they may just include the clouds above a certain height, e.g., above 9 km height, as often done in cirrus papers. They must clearly state that even then some cloud systems may still be mixed-phase clouds.

Major revisions are necessary.

Details:

P9284, Title: ....over a southern European lidar station ... would be appropriate. Almost all lidars around the globe are 'Mid-latitude' lidar stations.

P9284, Abstract: must be improved after all revisions.

P9284, Introduction:

line 23: There is a CIRRUS book (edited by Lynch, Sassen .....) with several chapters on lidar, but also on the role of cirrus regarding climate... that book should be referenced.

P9285, line 2-5 should be omitted, this is speculation.

line 10: chemical...? what do you mean?

line 13-15: list of references... please include a HSRL reference, may be Grund and Eloranta, Month. Wea. Rev. 1990? (Fire experiment 1986 results).

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line 16: why is the backscatter coefficient of importance? why not mention the depolarization ratio (water/ice discrimination), why not mention the lidar ratio (for spaceborne lidars like CALIPSO).

line 27: I personally found Goldfarb et al. (2001) rather questionable. There is another European cirrus long term data set from Reichardt, (Physics and Chemistry of the Earth, Part B, 1999).

P9286: line 10: mention Reichardt (1999) too

line 15: Eloragini and Flamant published a paper concerning cirrus retrieval in Appl. Opt. years ago (1995-2000), please check.

P9287 - Line 1: The underlying dataset of 65 cloud cases observed during 6 years of regular measurements does not seem to provide enough material to call the resulting statistic a climatology.

line 2: According to JGR web page, Seifert et al. is now 'in press'. Provide JGR number of this article, move it to the reference list.

Here is a copy from the JGR page:

Seifert, M. P., A. Ansmann, D. Muller, U. Wandinger, D. Althausen, A. J. Heymsfield, S. T. Massie, and C. G. Schmitt (2007), Cirrus optical properties observed with lidar, radiosonde, and satellite over the tropical Indian Ocean during the aerosol-polluted northeast and clean maritime southwest monsoon, J. Geophys. Res., doi:10.1029/2006JD008352, in press.(accepted 31 May 2007)

P9287 - Instrumentation and data.

line 5-18: The actual vertical resolution of the lidar should be mentioned. Please state clearly whether the lidar is pointing to the zenith or not. If the lidar is pointing vertically, specular reflection is an important issue (bias) to be considered in the interpretation of cirrus observations. The related effects must be discussed in this section and the resulting bias in the cirrus statistics must be discussed in the result section.

line 26: Improve the numbers of measurements after introducing the threshold height of 9 km.

P9288 - Methodology

line 11: It is mentioned that the signal was smoothed by a sliding average smoothing routine but the authors did not point out which smoothing length was usually applied during data analysis (for the three different methods).

line 18: If there were different cirrus layers (in one observation session), did you separatly count these layers? In case that you distinguish different layers, what criteria for separation did you use? Anyway, please state what you did in the case of several layers with cirrus.

P9289, line 4, check also Eloragini and Flamant, Appl. Opt....

line 10, the mean lidar ratio.... please clearly state that this quantity is the effective lidar ratio and not the vertically mean lidar ratio that would be obtained in the case of averaging of the lidar ratio profile obtained with the Raman lidar. Give the definition for the effective lidar ratio: the optical depth divided by the column backscatter. This is then a backscatter-weighted lidar ratio (layers with strongest cirrus backscattering have the largest impact on the the effective lidar ratio). This in turn is then a serious problem in the case of a vertically pointing lidar where you may often observe very strong backscattering by layers with falling horizontally aligned crystals (large specular reflection). These layers strongly influence the cirrus effective lidar ratio and introduce a strong bias towards small lidar ratios.

line 24: the effective lidar ratio is not defined yet...

P9290, line 8: I guess the Chen et al. method is the same as the Eloragini and Flamant method...? Please check!

line 12: Standard Atmosphere...Is no actual radiosonde available? no Numerical Weather Prediction model output for grid point Thessaloniki? Did you also use Standard Atmosphere Profiles in the Klett aproach before?

Later (result section) the authors use actual radiosonde data (temperature profile) in the lidar data analysis. It should therefore be justified why there was a standard atmospheric model used in the case study that illustrates the transmittance method.

P9291, line 8: How did you obtain the lidar ratio of 26 sr? Did you vertically average the Raman solutions? As mentioned, the Klett lidar ratio is the effective lidar ratio, mainly influenced by the strongly backscattering parts of the cirrus (so, the lidar ratio is typically low because of specular reflection). The Raman lidar ratio does not suffer from this effect (there is no influence of the backscatter coefficient strenght). All cirrus parts contribute equally to the lidar ratio. That seems to be the reason that the Raman mean lidar ratio is much larger than the Klett effective radius. Please comment on that. This effect is also described in that CIRRUS book of Lynch et al., if I remember right.

P9291 - Section 3.2: Multiple scattering is an important issue when dealing with cirrus data, or more common, when dealing with lidar signals returned from large scatterers with diameters greater than approx. 10 μm. However, it should be taken care on an accurate multiple scattering correction. In comparison to the MS-corrections calculated by the models of Eloranta and Hogan the parametrization of Chen 2002 produces ms factors that stand in contradiction to these models. Whereas Chen accounts almost no multiple scattering effect to optically thin and thus geometrically thin cirrus (see Sassen and Comstock 2001 for the almost linear relation) the models calculate the strongest ms effect (eta around 0.5 because of forward-scattering peak of ice crystals) for geometrically thin clouds which decreases with penetration depth. The authors should mention that the parameterization of Chen is in contradiction to the model results and why this is the case.

P9293/9294, lines 20 to lines 8, The discussion on multiple scattering consequences

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is not satisfactory. The approaches done for the Klett and Raman solutions appear reasonable. Factors of 0.67 and 0.64 are ok. In contrast, the factor of 0.93 is not just trustworthy. At least, explanations are required, why this ms factor is so different from the two other factors, although one and the same lidar with fixed laser beam divergence and RFOV is used.

Result section:

P9295, lines 9-10: As mentioned above, the authors have to present the method how they distinguish cirrus clouds from water and mixed-phase clouds at temperatures between -25 and -38 °C. Alternatively, they may just include the clouds above a certain height, e.g., above 9 km height, in their statistics. This is often done in cirrus papers.

P9295, line 15: Cirrus cloud base is always rather variable, thus mean values of 7.4 and 7.8 km are almost the same when keeping the standard deviation (not given) into account.

P9295, line 19: As long as lidar-radiosonde observations are not done simultaneously and at the same place, the statement concerning cirrus top above the tropopause is speculation. Speculations should be avoided.

Section 4.2: Care should especially be taken in this section (and afterwards) where the results of the different retrieval methods are compared. It should be mentioned in the previous sections, what an 'effective' lidar ratio, optical depth, and extinction-coefficient is.

P9297: Please compare your findings with Reichardt (1999)

Further comments are not possible as long as a new set of trustworthy cirrus clouds (clouds above 9 km or so) and respective results are no avaialable.

Table 1: Tropopause heights (radiosonde observations) should not be compared with lidar data (taken at different time, different site).

Absolute numbers of observations per season are required to get an idea how trustworthy the statistical results are. The numbers seem to be rather low after Fig.4, thus Table 1 is generally questionable.

Figure 5 is trivial and should be left out.

Figure 6 is questionable because of the point mentioned above that radiosonde time and site is different from lidar time and site.

Figure 8a: Again absolute numbers per month are needed.

Figure 9: Message is: No Message, no dependence? How many cases per temperature interval?

Figure 10: How many cases per thickness interval, per mid cloud temp interval?

Remaining question: If you have the optical depth and the geometrical depth, what's about a mean cirrus extinction coefficient? May be you plot that as a function of temperature. This relationship is most interesting for modelers.

More generally: May be you leave out all the questionable figures, because the number of cases per class is too low, and just provide some histograms considering all 65 cases (or may just 50 cases after introducing a threshold height).

May be you separate winter (Oct to March) and summer data (April to September) only. The numbers per half year may be high enough.

All parts of the paper: Spelling needs improvement

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9283, 2007.