

***Interactive comment on “Ceilometer-lidar  
inter-comparison: backscatter coefficient retrieval  
and signal-to-noise ratio determination” by  
B. Heese et al.***

**Anonymous Referee #2**

Received and published: 25 September 2010

General comments

The AMTD paper of Heese et al., 2010 address an issue of optical properties retrieval from atmospheric signals obtained using a new generation ceilometer type, a relevant scientific topic for the lidar community. Authors reach conclusions using valid scientific methods and assumptions, however, do not always clearly outline them. Most of the issues are covered by Referee 1# and I gave only a few additional points below in Specific Comments. Presented results are traceable and in most cases authors give proper credit to related work, however, do not always clearly indicate their new/original

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contribution (e.g. p. 3914 lines 22-27 Is the described calibration in Cirrus heard of or invention of the authors?). The title and abstract reflect contents of this paper and the overall presentation is well structured. The mathematical formulae are used correctly. The figures are fine, except a few typing errors in captions, which I gave in Figures Comments. Similarly, I pointed out a few typing errors in Reference Comments.

The main problem of this paper is a question whether the presented results are sufficient to support the interpretations and conclusions. Two authors of this paper are from the EARLINET community. I would like them to address an issue on how this study, which is based on only two observations, one for nighttime and one for daytime, can 'determine whether the ceilometers are capable to deliver quality assured particle backscatter coefficient profiles' and 'characterize the ceilometers signal performance', as it is claimed in the abstract. The EARLINET community has a very strong recommendation on performing a long-term feasibility studies of such as addressed in this paper scientific questions. Authors should clearly explain why they do not discuss a long-term study (from the text I can guess that you have only a few days of observations during the CLIC campaign) or, if they did analyzed a long-term observations, what is the statistical significance of the presented here 'representative cases'. Authors must clearly state to what extend are their conclusions valid.

If the issues highlighted by both Referees are addressed I would strongly recommend this interesting manuscript for publication in the AMT journal.

## Specific Comments

## Abstract

p. 3908 ln. 3 Inter- Comparison => Inter-Comparison (and use this one throughout manuscript)

## 1 Introduction

p.3909 ln. 13 Unfortunately, along with this manuscript there are not many studies

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done on a retrieval of optical properties using the CHM15k(-X) ceilometers. Hence, it is worth a citation here the conference paper which discusses one of such approaches: Stachlewska, I. S. and Markowicz, K. M.: On forward Klett's inversion of ceilometer signals, 25thILRC International Laser Radar Conference, 5-9 July 2010, St. Petersburg, Russia, 2010

p. 3910 In. 6 More details on the CLIC campaign should be given. How many observations were taken? What was duration of the campaign? Where was it performed? I guess it was a few days during EARLI 09, or is there more to it?

## 2 Instruments

p. 3910 I. 16-17 Does the Polly XT fulfill 'the requirements of the EARLINET lidar'? Please check this statement. To my knowledge, accordingly to Dr. G. Pappalardo (priv. com. 2009), there is no such thing as requirement for EARLINET lidar.

p. 3911 In. 1 84 micro J ?

## 3 Data evaluation

p. 3911 In. 21 Was any overlap correction on the ceilometr and lidar signals performed and if so, add information on how was it done.

p. 3912 In. 1 - 4 As Referee 3# pointed out please add more details on Raman retrieval. How where the Raman profiles averaged in time and space? What method was used for smoothing?

p.3913 In.1 1 km of incomplete overllep of lidar or ceilometer?

p.3913 In.2 As Referee #1 mentioned more on error estimation is needed. What about error of using AOD of photometer at 1020 nm and AOD of ceilometer / lidar at 1064 nm in the daytime measurement? What about a difference in daytime – nighttime errors due to the use of a different optical path of the ceilometer/ lidar and photometer AOD which is a constrain on retrieval?

#### 4 Ceilometer-lidar inter-comparison

p. 3913 In. 7 Ceilometer lidar => Ceilometer-lidar (and use this one throughout manuscript)

p. 3913 In. 16 Please state here once again were the lidar the sun-photometer and the ceilometer were based. Where they apart (and what instruments) in as distance of 2km for the daytime case? Where they together at one site for the nighttime case? This must be clear here.

p. 3913 In. 16 Please comment on how much / little representative for the atmospheric variability are presented here profiles which were obtained with such a long averaging (over 3 h).

p. 3914 In. 5 Reference for FLEXPART is missing, e.g. Stohl, A., Hittenberger, M., and Wottowa, G.: Validation of the Lagrangian particle dispersion model FLEXPART against large scale tracer experiments, Appl. Optics, 32, 24, 4245–4263, doi:10.16/S1352-2310(98)00184-8,1998.

p. 3914 In. 10 More information on the obtained AOD photometer value is necessary. Is this value calculated also over the 3h period ? How did you eliminated the passing or sub-visible clouds, if they where on the photometer's sight of view?

p. 3914 In. 22- 27 Is the described calibration in cirrus heard of or invention of the authors?. Please clearly indicate you contribution or give a reference.

p. 3914 In. 27 (and also in Figs. 2 and 3) what is this unit  $Mm^{-1}$  ?

p. 3914 In. 10 (and also Fig. 3) An information on the obtained AOD photometer value is missing. Did you use this value as a constrain for ceilometer retrieval? Were the two instruments placed nearby? Was the ceilometer and lidar tilted to measure into the same direction as photometer? Did you filter the cirrus optical depth out of photometer data? Was the photometer AOD measurement take during the whole 2h period corresponding to the vertical profiles?

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p. 3915 ln. 17 please add also here a reference to the AMTD paper of Flentje et al. 2010 with a commentary that another example of a nighttime case is discussed in detail therein.

p. 3915 ln. 25 It is not trivial to obtain the background for the 1064 nm signal as the ceilometer signals have a short-range SNR. Please give a hint how you did it.

#### References

p.3918 l. 16 correct => Markowicz Remiszewska Stelmaszczyk

p.3919 l. 1 add references

=> Stachlewska, I. S. and Markowicz, K. M.: On forward Klett's inversion of ceilometer signals, 25thILRC International Laser Radar Conference, 5-9 July 2010, St. Petersburg, Russia, 2010

=> Stohl, A., Hittenberger, M., and Wottowa, G.: Validation of the Lagrangian particle dispersion model FLEXPART against large scale tracer experiments, Appl. Opt., 32, 24, 4245–4263, doi:10.1661/S1352-2310(98)00184-8,1998.

p. 3919 l. 1 and l. 4 Reference to Papalardo et al. 2010 needs to be placed alphabetically.

#### Figures

##### Commentary to Fig.2

There is significant difference in ceilometer retrieval below 5 km with w change of the reference height of 0.8 km. An explanation of why this is happening is necessary (in the text). Also in the caption you write that the obtained AOD from the dashed profile is 'too low'. You mean 'too low' with respect to what, the photometers AOD? If so, you must state firmly to what extend you do expect those two values to match, as this is not trivial.

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### Commentary to Fig.3

It is not clear here nor in the text how you obtained the AOD from ceilometer / lidar, i.e. did you integrate AOD from profiles with or without the cirrus range? And how the AOD of photometer is obtained, i.e. is it 'contaminated' with this cirrus cloud? There are two things necessary to conclude for the AOD comparisons.

### Commentary to Fig.4 and 5

In caption: signal => signals

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Interactive comment on Atmos. Meas. Tech. Discuss., 3, 3907, 2010.

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