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Atmospheric Measurement Techniques
Discussions

Interactive comment on "What is the benefit of ceilometers for aerosol remote sensing? An answer from EARLINET" *by* M. Wiegner et al.

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Introduction

We thank reviewer #4 for his/her useful comments and suggestions for improvements of the paper. We repeat the points raised by the reviewer and add our comments in italics.

Point by point replies

[...] Although few aspects, mostly related to the water vapor absorption in the 905-910 nm spectral range and the possibility to calculate the LIDAR constant using a forward approach remain partially unsettled, the gross picture is fully resolved and one have the

feeling of having gone one step further in the understanding of the real benefit of using a ceilometer after reading this paper. A part from technical remarks that I summarize in the section below, I strongly support the publication of this article in AMT. To be accepted with minor revisions.

→ *Thank you for the positive feedback. We want to emphasize that the paper is already long (see review #1). We feel that it is not possible to cover all aspects in one paper, e.g. it is certainly worthwhile to discuss the water vapor issue and the combination with transport models in separate papers. We – and certainly other groups as well – are working on this, so that papers focussing on these specific topics can be expected (see also review #3).*

Specific Comments:

There are only few points that remains partially unanswered in the text or that may lead to confusion, I list them hereafter:

- Pg 2499, ln 4-5: "in the near infrared...", as it is, this statement is not fully correct. The transmission term is close to unity in the free troposphere not necessarily in the lower troposphere (boundary layer). Little-absorbing particles are slowly attenuating the LIDAR signal, but as soon as the absorption coefficient grows (soot, BC,...etc) the extinction is not negligible anymore.

  → *The lidar signal depends (among others) on the transmission of the atmosphere and thus is related to the extinction coefficient. The extinction of aerosols is quite low in the infrared, not only in the free troposphere (as the reviewer states) but also in the boundary layer (not as small as above but also small). Even if absorption is growing (decrease of single scattering albedo) the extinction remains small. Only in exceptional cases the trans-*

mission might be significantly reduced (at our site we have never experienced this), however, even then the inspection of the "shape" of the β^*-curve does not pretend aerosol layers. This effect has been shown when we discuss the water vapor absorption which is a quite similar situation (see Fig. 4).

- Pg 2499, ln 5-6: I am not sure that can be generally true. In fact, when comparing backscatter coefficient and att. backscatter in the boundary layer the two hardly give a match. Imagine having a height-dependent LIDAR ratio and a highly changing α-to-β relation, then the relation between att. Backscatter and backscatter is far from being obvious.

  → *This comment is similar to the previous. In principle the reviewer is right: due to a changing lidar ratio the relation between backscatter coefficient on the one hand and extinction coefficient or transmission on the other hand is not constant with height. However, as the optical depth in the near infrared is small, the transmission is hardly affected by a changing lidar ratio. As a consequence, layer detection by means of β^* is possible even under such conditions.*

- Pg 2503, ln 2-3: you should add here also the estimate of the error when the full overlap is at 1.5 km, which is typical for the CHM15K

  → *We have now covered this aspect: see our comment below referring to Pg 2503, ln 6-7.*

- Pg 2510, ln25-26. The hydrophobic assumption for aerosols, depending on the site, can be a very wrong assumption.

  → *We are aware of this fact and agree with the reviewer that assuming hydrophobic aerosols can often be wrong. The only reason for this assumption was that we wanted to investigate the effect of water vapor absorption on ceilometer signals in the 905–910 nm spectral range alone, i.e., without consideration of induced effects (growth of particles, change of S_p, etc.). Investigations of combined effects of water vapor and aerosols would not lead to clear and traceable conclusions. For similar reasons we also selected the idealized humidity profiles with jumps between 0% and 99%. As mentioned above it is certainly worthwhile to publish a detailed paper on the relevance of water vapor absorption for aerosol retrievals using ceilometer data.*

- Pg 2511, ln 20-22: One might argue this statement. By looking at Fig. 4, (which is excellent and very much informative!) it is clear that at higher ranges, and in any case where the water vapor concentration is negligible, the difference between the dry atmosphere curve and the other curves is constant. That suggests that when retrieving the LIDAR constant C one could just rescale the LIDAR signal to the molecular profile. That would result in smaller C values in case of water vapor absorption. One should always consider this when physically interpreting C, but it would not prevent to calculate C.

  → *Our statement in Pg 2511, ln 20-22, refers to the forward algorithm as described in Sect. 4.2.1: Eqs. 10 and 11 show that C_L requires the knowledge of T_{ovl}. The aerosol constribution can be estimated or set to unity as discussed in the manuscript, however, in case of water vapor absorption additional uncertainties are introduced. Thus, the inherent accuracy of C_L is reduced, only due to the fact, that an overlap region exists. Though in principle only one z is required to solve Eq. 10 for C_L any choice requires the knowledge of the water vapor distribution (for α or α_w, respectively).*
  We think that rescaling of the signal only works in a mathematical sense. If we multiply the lidar equation with T_w^{-2} we get

$$P(z)\,T_w^{-2}(z) = P^*(z) = C_L \frac{\beta(z)}{z^2}\,T_m^2(z)\,T_p^2(z)$$

i.e. a "modified lidar equation (for P^*)" that formally looks like the conventional lidar equation (see also Markowicz et al., 2008) and can be solved as usual with (e.g.) the backward approach. Then, $P^*(z)$ can be scaled according to a well known Rayleigh-calibration in an aerosol-free region. However, the transformation of $P(z)$ to $P^*(z)$ remains an issue as the vertical profile of the water vapor transmission is unknown. Thus, rescaling only will work in a purely mathematical sense, but will fail in practise. An example of the deviation of the retrieved β_p-profile due to an under- or overestimate of α_w (and consequently of T_w^2) is shown in the manuscript (Fig. 6).
Similar conclusions are found if in the formalism α_m and α_w are substituted by α_t with

$$\alpha_t = \alpha_m + \alpha_w$$

Then again the solution can be found straightforwardly, but the inherent problem of the unknown water vapor distribution remains.

Technical corrections:

Generally through all text, the term LIDAR should be in capital as it is an acronym.

→ *LIDAR is indeed an acronym, but it is used since 3 decades or more and can now be treated as a "normal word". I guess, more than 90% of all scientific papers use it this way. Note, that "laser" is also used as a word, though it is an acronym. For "radar" the situation is similar. Thus, (and as none of the other reviewers criticized this) we would like to stay with "lidar".*

- Pg 2492, ln 16: should be "with a relative error".

  → *Corrected.*

- Pg 2492, ln 23: replace "changing" with "modification of".

  → *Changed.*

- Pg 2492, ln 24: please add the correct reference to the indirect effect based on IPCC R5. It should be added in brackets "(ERFaci, Cloud lifetime effect and glaciation indirect effect, IPCC AR5, 2013)".

  → *We have added this as proposed.*

- Pg 2493, ln 15: should be "as they can provide", drop "only".

  → *Done.*

- Pg 2494, ln 2: define here the meaning of subscript "p".

  → *We have added a general comment that the subscript "p" stands for particle; thus, we have change "aerosols" to "particles", when the subscript first appears (normally "aerosols" and "particles" are synonymously).*

- Pg 2494, ln 7-8: replace by "Based on this set of parameters, and under favorable conditions, it is possible...".

  → *We followed this suggestion.*

- Pg 2494, ln 17: should be "increase in".

→ *Corrected.*

- Pg 2494, ln 21: should be "the poor spatial resolution remains an issue".

→ *Changed as suggested.*

- Pg 2495, ln 1: "investigate to which extent".

→ *Typo corrected.*

- Pg 2503, ln 6-7: The statement "we conclude that the accuracy..." Is too qualitative. The authors should consider replacing the statement with a maximum range of error to be taken into account when applying Eq. 12 to higher overlaps, e.g. 1.5 km.

→ *We have added a third case with z_{ovl} =1.2 km to Tab. 1 of the original manuscript, modified Fig. 1 accordingly, and extended the discussion in Sect. 4.2.1 as suggested by the reviewer. With such a high z_{ovl}, the underestimate of C_L indeed is larger than 7% (now 13%), and the proposed correction is less efficient (2% underestimate instead of 1% underestimate). However, for such ceilometers normally a correction function is provided that allows to correct for incomplete overlap down to approximately 0.5-0.6 km. This is e.g. true for the Jenoptik CHM15k, when we checked the provided correction function with own inter-comparisons with a Jenoptik CHM15kx and a Vaisala CL51 (and found good agreement). This was the reason why we had only included z_{ovl} =0.6 km in the original version. In conclusion we want to emphasize, that overlap correction functions are very important and should be cross-checked, and that the risk of a wrong extrapolation of α_p to the ground increases with increasing z_{ovl}.*

- Pg 2508, ln 10: replace straight forward with "straightforwardly".

→ *Done.*

- Pg 2508, ln 14: Should be "solid lines".

→ *Done.*

- Pg 2510, ln13: should be "spatio-temporal".

→ *Changed throughout the paper.*

- Pg 2510, ln 20: not "evaluation", replace with "evaluating".

→ *Typo corrected.*

- Pg 2510, ln 25: "are used".

→ *Corrected.*

- Pg 2511, ln 6: Please add, "A water vapor distribution between 0 and 2 km".

→ *Clarification added.*

- Figure 5: Authors should add a legend to the figure describing the different colors.

→ *This information was included in the caption. We have moved it to Fig. 5b to facilitate the reading of the figure and adapted the caption.*

- Figure 13: the caption of Fig. 13 is misleading. I think it should be like that: percentage of elevated layers detected by the CHM15Kx in daytime and nighttime conditions. The EARLINET-LIDAR MUSA is used as reference for the total number of aerosol layers.

→ *The suggested caption is indeed clearer, so we use this (but CHM15k instead of CHM15kx. Furthermore, we mention the measurement site).*

- Pg 2521, ln 7: There is no need of the first paragraph, conclusions should start from here.

  → *The first paragraph was written to outline the motivation of this paper, maybe it is too long. Following mainly the suggestion of the reviewer we have drastically shortened this paragraph: It now reads: "Significant progress in range resolved aerosol characterization is accomplished by means of the lidar technology. However, costs for investment and maintenance of advanced lidar systems are prohibitive for establishing dense networks. As a consequence, it is worthwhile to investigate to which extent the recently established ceilometer networks can contribute to aerosol remote sensing".*

- Pg 2521, ln 19: should be "particularly during night".

  → *Changed.*

- Pg 2522, ln 8: not "underway", replace by "on the way".

  → *Changed.*

Please also note the supplement to this comment: http://www.atmos-meas-tech-discuss.net/7/C545/2014/amtd-7-C545-2014- supplement.pdf

→ *The content of the supplement is identical to the list of comments above. So, our replies from above can be used.*
