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Atmospheric Measurement Techniques Discussions

Interactive comment on "Correction of water vapor absorption for aerosol remote sensing with ceilometers" *by* M. Wiegner and J. Gasteiger

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Introduction

We want to thank reviewer # 1 for his/her careful reading, the overall positive rating and the provision of a number of useful comments and suggestions – it helped us to improve the paper. We repeat the points raised by the reviewer and add our comments in italics.

Page and line number refer to the version submitted for quick review.

Point by point replies

General comment

[...]

Apart from a couple of points that I point out in the technical comments, the methodology is well described. The topic is of utter interest as it and deals with a major problem within the LIDAR, aerosol and NWP communities. Until now, the lack of correction for water vapor absorption in the 890-910 nm band has caused significant underestimation or overestimation of the calculated backscatter coefficient depending on the employed inversion scheme. The manuscript highlights also the need to have the manufacturer disclosing the emission spectrum and to become more user oriented if they really aim at becoming a reference for aerosol retrievals. My (major) remark aims at improving the calculations of β_p around 910 nm by using directly the CL51 instead of a CHM15Kx that needs assuming an Angstrom exponent. The overall readability of the manuscript is good, but the written English should be improved at many places where flaws are present.

I believe this manuscript should be published and that will make an important contribution to the state of the art of automatic LIDAR retrieval schemes. I recommend the publication of this article after minor revisions (detailed below).

→ *We have considered all reviewer's suggestions, details are given below. We especially address "My (major) remark aims at improving the calculations of β_p around 910 nm by using directly the CL51 instead of a CHM15Kx that needs assuming an Angstrom exponent." when commenting on "Pg 9-10, ln 659-708, Sect 5.1:".*

Moreover we have updated the references (e.g., AMTD → AMT) and reviewed the whole text as "...the written English should be improved at many places...". That led to a number of minor changes (replacement of words, clarification of *expressions, typos), that are however not explicitly mentioned below.*

Technical comments

- Pg 1, ln 11-12 : modify as following: "...the majority of ceilometers emits signals at wavelengths that are influenced..."

  → *We have changed the sentence in a way that is very close to the reviewer's suggestion: "...the majority of ceilometers provide signals at wavelengths that are influenced..."*

- Pg 1, ln 40: "Meanwhile, the total number of such systems is almost 2000." Where? Europe, US? Worldwide?

  → *The number is based on the survey of Werner Thomas mentioned in Wiegner et al. (2014) in AMT. Currently the database includes 1997 ceilometers and 160 lidars (Werner Thomas, pers. comm.). Though the database is worldwide, the focus is on Europe and North-America because it seems to be difficult to get reliable information for the rest of the world. Approximately two third of the ceilometers store the raw data profiles, but only a minority of instruments is currently online. Some weather services are currently replacing old system by state-of-the-art ceilometers. It is told that Vaisala has sold several thousands of ceilometers, so it can be assumed that the number of 2000 is underestimated. As long as (obviously most of) these ceilometers are not online it remains difficult to give a precise number.*

  Accordingly, we have clarified this statement. It now reads: "The number of such systems is rapidly growing and is assumed to be a few thousands, but only data of approximately 200 ceilometers is currently available online. Many ceilometers only retrieve cloud base heights according to their original design, however, in the last years more attention..."

- Pg 1, ln 41-42: "for aerosol related retrievals: studies were devoted", replace it with "as a tool to study aerosols intensive properties. Moreover, studies were devoted"

  → *We don't want to suggest that intensive properties of aerosols can be derived from ceilometers. This requires more complex lidar systems (multi-wavelength, depolarization, Raman-channels). This might change when a depolarization-ceilometer will be available: then the (intensive property) "linear depolarization ratio" can be used to characterize the particles.*

  So we would like to leave the sentence unchanged.

- Pg 1, ln 61: replace "could by" with "could be"

  → *We corrected this typo.*

- Pg 1, ln 66: replace "acceptable accuracy" with "accurately enough".

  → *Done as proposed*

- Pg 2, ln 70-73: "Nevertheless, backscatter ... sophisticated research lidars." This is too generic statement that should be supported by references.

  → *This sentence was meant to mention possible future applications of β_p as derived from ceilometers. Therefore, references cannot be provided at the moment. To our knowledge data assimilation is not yet successfully implemented (but several groups seem to work on it), validation of chemistry transport models is currently limited to uncalibrated ceilometer signals or case studies (see citations in line 44), and the joint exploitation of data from ceilometer networks and advanced lidar systems ("anchor stations") is also still missing. As mentioned in the Introduction most papers dealing with* *ceilometers and aerosols are still devoted to the determination of the mixing layer height.*

To make this clear we have re-phrazed this sentence as follows: "Nevertheless, it is expected that in future backscatter coefficients can be quite useful for data assimilation and the validation of chemistry transport models, and for some kind of interpolation between the limited number of sophisticated research lidars."

- Pg 3, ln 185, Eq.4: even though the forward/backward Klett solutions are well known the authors should explain the term S_m and S_p right after equation 4.

  $\rightarrow$ *Done*

- Pg 3, ln195-196: replace with "This approximation is legitimate for the term $Z_1(z)$, i.e. at wavelengths..."

  $\rightarrow$ *Done as suggested*

- Pg 3, ln 203, Eq. 6: please provide range values of the molecular and particle Beer-Lambert terms in the incomplete overlap region (at 905-910 nm) to justify the approximation.

  $\rightarrow$ *Using the values from Tab. 1 and assuming full overlap at 200 m one gets the following values:*
  $T_p^2 \approx$*0.980 for 910 nm and* $T_p^2 \approx$*0.985 for 1064 nm, and* $T_m^2 >$*0.999 for both wavelengths.*

- Pg 5, ln 351-352: the chosen spectral interval, the spectral resolution and the number of calculations are not consistent.

  $\rightarrow$ *The spectral range is λ=895–930 nm. This corresponds to a wavenumber interval from 11173 to 10753 cm^{-1}, i.e. an interval of approximately 420 cm^{-1}. With a resolution of 0.01 cm^{-1} this results in 42050 monochromatic calculations as indicated in the paper.*

  We assume that the remark "...though for ceilometer applications the range between 900 nm and 925 nm probably would be sufficient." has confused the reader. Thus we have changed the order of two sentences. It now reads: "... we use the atmospheric radiative transfer simulator (ARTS, Buehler et al. (2005)). Though for ceilometer applications the spectral range between 900 nm and 925 nm probably would be sufficient, we consider a slightly larger interval from 895 to 930 nm (wavenumbers $\tilde{\nu}$ between 11173 and 10753 cm^{-1}). The spectral resolution $\Delta\tilde{\nu}$ was set to 0.01 cm^{-1}, resulting in 42050 different calculations."

- Pg 5, ln 366, Eq.18: please define the relative humidity term f_{rel}

  $\rightarrow$ *Done (it was defined but only in line 464.)*

- Pg 5, ln 386-387: can the authors justify/support the assumption of a Gaussian emitted spectrum around a λ_0 wavelength? Is the Vaisala laser temperature-stabilized? If not, what would be the typical operational temperature range?

  $\rightarrow$ *We do not know the exact shape of the spectrum around λ_0, so we assumed a Gaussian shape. The Vaisala laser is not temperature-stabilized (see lines 387, 856). According to house-keeping data the temperature typically changes by 3–4 K in the course of a day, but there are also cases where the change exceeds 10 K. We also see "the need to have the manufacturer disclosing the emission spectrum and to become more user oriented" as the reviewer mentioned in his/her general remark (see our conclusions).*

- Pg 6, Fig.1 & 2: a legend would do.

  → *We are not sure if we understand the reviewer's intension correctly. As in Fig. 2 a lot of colors and symbols are used, we have added a legend to facilitate the reading. We feel that this is not necessary for Fig. 1 as it is much clearer and almost self-explanatory. The modified version of Fig. 2 (legend and shortened caption) is shown in the supplement to this file.*

  We have also slightly modified Figs. 3 and 5 and Figs. 8 and 10 in the reviewer's sense and included the wavelengths λ_0 in the panels (see supplement for two examples).

- Pg 8, ln 574: The statement about the variability being larger for the Tropical case is confusing. How can it be larger if for the mid latitude cases the variability accounted for 48% for both the 2012 and 2013 cases and only for the 24% for the tropical case?

  → *We compared the absolute values: 7.7 kg/m^2 vs. 11.4 kg/m^2, this was the reason for the confusion. Thus, we have modified the sentence: "...and the absolute values of the variability are larger as well."*

- Pg 9-10, ln 659-708, Sect 5.1: I appreciate the fact that generally the (analog?) detection mode and the inherent bias at higher altitude that appears in the CL51 profiles make more difficult to perform a Rayleigh calibration. However, it is possible to find cases when a molecular calibration is possible also with a CL51 and to use these measurements directly to retrieve β_p without through an external ceilometer signal at a different wavelength and thus introducing additional assumptions. In particular, I am not convinced about the "threefold advantage" described by the authors, I would rather see the proposed procedure as a "no-way" backup solution. I think that useable CL51 profiles could be found in the

framework of the current CeiLinEx field campaigns in Lindenberg. I invite the authors to use directly a CL51 profile and to compare the results with those obtained using a CHM15Kx.

  → *As this is the major concern of the reviewer (see also his/her general comment) we want to comment very detailed and cover several aspects.*

  (1) The purpose of this paper is to make the community aware of a problem, to describe a methodology to treat/solve the problem (correction of water vapor absorption) and to demonstrate what happens if the problem is ignored. For the last part we need realistic atmospheric profiles: then we can demonstrate how large the errors of β_p could be. This was done on the basis of one aerosol profile and a very large number of water vapor profiles. So we had the problem which aerosol profile should be selected for the investigations. We feel that a "typical" profile will do, it does not matter if β_p is smaller or larger by a few percent at certain heights. In other words: the "introduction of additional assumptions through an external ceilometer signal" might lead to an aerosol profile that might only slightly differ from the real one at some heights (if the assumptions are wrong). In our opinion, however, this approach is better suited than any profile from Lindenberg, because Lindenberg is not representative for Munich. Though a "real measurement profile" would have been the most "elegant" solution – a profile composed of a combination of a real measurement and a simulation meets the requirements. Note, that additionally "extreme cases" were introduced by scaling the typical profile ("clear" and "turbid" case) to get further insight.

  (2) We fully agree with the reviewer, that measurements from CeiLinEx should be exploited. In our view, however, this constitutes the next step in the series of studies dealing with aerosol retrievals by ceilometers: the validation. It is planned to apply WAPL to CL51-measurements and compare β_p to CHM15k/CHM15kx-measurements. As mentioned in the paper

(Section 6, lines 881 ff.) we hope to have auxiliary data sets to adequately treat the inherent problem of the wavelength dependence of aerosol optical properties (Angström exponent, also mentioned by the reviewer). This will be a complex effort and is beyond the scope of the present paper. Moreover, contributions of other colleagues will be required so that the list of co-authors will be different from this paper.

(3) The term "threefold advantage" was indeed a little bit euphemistic, but we don't believe that the idea behind is wrong; see (1). Nevertheless, to take into account the reviewer's suggestion we have re-phrased the sentences "The advantage of this approach is threefold: first we can consider profiles ... can be applied more generally." The new version is: "So we can consider profiles of aerosol properties that are realistic for the site (note the very good qualitative agreement of the two profiles in the boundary layer), we can test the forward and the backward solution (in particular as we can set β_p=0 at a reference height), and the water vapor effect is not masked by measurement artefacts."

- Pg 10, ln 706-708: why an overlap correction is not needed? The water vapor concentration is higher close to the surface and the absorption coefficient is particularly high in the region of incomplete overlap.

  → *This is a misunderstanding – we are sorry for this. In the AMT-paper Wiegner et al. (2014) we have shown that a specific overlap-correction in addition to the standard (i.e. automatically applied) overlap correction provided by the manufacturer is required for a few cases from 2012 and 2013. The reason was that we detected some artefacts that could be corrected by this function. The function was determined from horizontal measurements.*
  This does not apply to the data used in this paper. Here, only the standard correction function provided by Vaisala is used. We expect that Vaisala-users a familiar with this procedure.

  As these facts might provoke misunderstandings and are not really relevant in the context of this paper, we have skipped the corresponding sentence.

- Pg 11, Fig 10-11: While I have no problems with the results shown by the authors, I think that the difference between backward and forward over- and underestimations should be explained better, and not let to a simple "it is obvious that...". I'd add something like that: "The switching between underestimation and overestimation of β_p^* using, respectively, the forward and backward integrations is all based on the choice of the reference point. This last is in one case (forward) only little affected by water vapor and leads to underestimated β_p^* and in the other case (backward) is greatly affected by water vapor (absorption in the lower layer) and leads to overestimated β_p^*."

  → *We followed the suggestion of the reviewer and added a few sentences for explanation. They read starting at line 774: "It is obvious that ignoring the water vapor contribution (red curve) leads to a $\beta_p^*(z)$ that overestimates the true aerosol backscatter coefficient. The reason is that integration starts at a height that is strongly affected by the water vapor absorption of the lower layers and thus the reference value $\beta_p(z_{ref})$ is underestimated. In the previous case (forward integration) integration starts at a height that is only little affected by the reduced transmission due to water vapor. With increasing height the effect of the absorption gets stronger leading to an underestimated β_p^*."*
  As a consequence of this change we have also adapted the subsequent paragraph to get a smooth transition.
  "As a consequence the accuracy of the retrievals for the elevated layer and the mixing layer are quite different for the two integration schemes (compare to Fig. 8). In case of the backward integration (see Fig. 10) the overestimate is only 5 % in the elevated layer because the absorption between the reference height and the lower boundary of that layer is small due to the low

water vapor concentration (see Fig. 7, right panel). Thus the water vapor transmission is comparable at both levels. In the mixing layer the overestimate of β_p increases to around 10–15 % because of the larger water vapor concentration in this layer leading to a more pronounced change of the water vapor transmission compared to the reference height."

- Pg 13, ln 877-884: in this regard I refer to my previous comment: valid CL51 profiles should now be available for a more accurate validation of WAPL.

  → *There is no doubt that validation is the important next step to assess the potential of ceilometers operating around 910 nm for aerosol retrievals. As discussed above and mentioned in the paper, validation is planned in the framework of CeiLinEx (still ongoing until mid of September 2015).*

  We have now explicitly mentioned this campaign in the "Summary and conclusions".

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C2983/2015/amtd-8-C2983-2015-supplement.pdf

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 6395, 2015.