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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

Anonymous Referee #1

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General comment

The paper "Correction of water vapor absorption for aerosol remote sensing with ceilometers" by M. Wiegner and J. Gasteiger describes a new technique for correcting near-infrared automatic LIDAR profiles for the absorption due to water vapor. The proposed methodology is based on two database of automatic LIDAR and radiosounding during the years 2012-2013 at a Tropical and mid-latitude sites. The study draws the conclusions that water vapor corrected profiles of backscatter carry much smaller uncertainty than uncorrected ones also including the inherent uncertainty of the used correction method. 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 βp around 910 nm by using directly the CL51 instead of a CHM15Kx that needs assuming an Angström 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).

Technical comments:

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

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

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"

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

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

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

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

Pg 3, ln195-196: replace with “This approximation is legitimate for the term Z1(z), i.e. at wavelengths. . .”

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.

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

Pg 5, ln 366, Eq.18: please define the relative humidity term f_rel

Pg 5, ln 386-387: can the authors justify/support the assumption of a Gaussian emitted spectrum around a $\lambda 0$ wavelength? Is the Vaisala laser temperature-stabilized? If not, what would be the typical operational temperature range?

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

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?

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 βp 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 CeilLinEx 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.

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.

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.”

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.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C2805/2015/amtd-8-C2805-2015-supplement.pdf

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