Comments on

Ceilometer aerosol profiling vs. Raman lidar in the frame of INTERACT campaign of ACTRIS

by

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The paper describes the outcome of the campaign INTERACT when three ceilometers from different manufacturers (Jenoptik, Campbell, Vaisala) run co-located and simultaneously with a calibrated high-performance Raman lidar MUSA. The comparisons covered 6 months, so a valuable set of data was gained to investigate the performance of ceilometers for aerosol profiling. This topic is of growing importance as recently a large number of ceilometers was installed, and are increasingly used for atmospheric research. Madonna and co-authors focus on investigations of the stability of the ceilometers, their overlap characteristics and their performance to measure aerosol properties under different atmospheric conditions.

The paper fits into the special issue as an EARLINET-lidar is used as reference for their assessment. However, the manuscript is in many parts not as precise as it should be and the reasoning is often neither clear nor convincing. Some parts are confusing. Thus, before being accepted for publications the authors must significantly improve the text. Furthermore most of the figures must be revised as the labels and the legends are far too small to be readable!

I encourage the authors to revise their manuscript as it could be a useful contribution to a recent branch of remote sensing applications.

Please find a list of comments – ordered by appearance, not by relevance – below (page/line is given). Note, that several comments are linked as the corresponding issue is covered at different sections/paragraphs of the manuscript.

- 12409/12: The information on the price in the present form is not very helpful. Either give a concrete list of prices, give the overall price range or explain the details for the 45 kEuro-class.

- 12409/25: The list of references can be extended.

- 12410/1: Give a citation of the recent EARLINET overview paper here (and an equivalent older paper, if it includes some relevant aspects).

- 12410/24: "aerosol content": what is this? Mass concentration, number density, optical depth - please be precise (throughout the paper).

- 12411/17: "and the idiosyncrasies...": this is never covered in the manuscript. Thus, it can be omitted here.

- 12411/23: Please add one or two sentences on the site, e.g., orography and aerosol burden (e.g. aerosol optical depth).

- 12411/25: Sometimes "VAISALA" and sometimes "Vaisala": This is an indication of carelessness ('Vaisala' is adequate).

- 12413/12: "Moreover, ceilometers hardware can be assessed...". It is not clear to me why this is a drawback as stated at the beginning of the paragraph. Maybe these sentences require some rephrasing.

- 12413/25: Comments on Table 1 are missing. What is the most important information? What shall we learn from the comparison? What can we anticipate in view of the performance assessment provided later in the manuscript?

- 12413/27-12414/24: When discussing the different ceilometers the same characteristics should be mentioned: overlap is not given for the Vaisala, detectability of clouds in the partial-overlap regime is not discussed for Jenoptik. Please give temporal and spatial resolutions for all ceilometers.

- 12414/2: To my knowledge the paper of Wiegner and Geiß deals with the CHM15k-x ceilometer (see also comment below). Is this true for the ceilometer used in this study? Maybe this sentence should be omitted, the citation of Wiegner at al. should be deleted here anyway, as it is mentioned in 12416/24.

- 12414/27: The full overlap of MUSA (405 m) does not agree with the number given in Table 1.

- 12415/8: Citation of a paper "in preparation" is the second best option. If it is not available when this manuscript will be published, please add an older paper as citation (if available) as 'backup'.

- 12415/10: When "attenuated backscatter" is mentioned first (and as it is the most relevant quantity discussed in the manuscript) it should be defined by an equation! It should be made clear that it requires calibration as it is the ratio of the range corrected signal and the lidar constant (i.e., move Eq. 3).

- 12415/11: It should be clarified what type of CHM15k is used: the "old" or the "Nimbus" version. This is essential because the Nimbus version automatically provides an overlap correction, and the automated adjustment of the sensitivity is quite different.

- 12415/17: The reader might be confused when a "normalization constant" is introduced here. How is it related to the lidar constant $C_L$ and the constant $CC$ mentioned later (see also similar comments below)?

- 12416/4: The O'Connor et al. method is based on the fact, that the integration of the attenuated backscatter over a cloud must give a known value. If not, $C_L$ must be re-scaled. So, by applying this method, the range corrected signal (calibrated or uncalibrated) is required as input. Thus, it must be made clear in the text that "the so-called normalized sensitivity backscatter coefficient" is proportional to $P\,z^2$ (if this is the case).

- 12416/5: "...therefore, attenuated backscatter profiles can only be obtained using the cloud calibration technique..." (see also 12417/20 and 12420/23). According to Eq. 1. this method can easily be applied to any ceilometer, as one only needs a signal-ratio at ideally one (realistically at range of a few range bins) height for calibration. Why isn't this method used for the CT25k? It would be much easier, maybe more precise, better comparable with the other 'ceilometer vs. MUSA'-comparisons, and the paper would be homogenized.

- 12416/7 ff: This paragraph includes a discussion. Maybe the authors should restrict themselves here to pure facts; the discussion (and a list of future demands for the manufacturers is coming later).

- 12416/20: "a fixed lidar ratio"; this is indeed necessary for daytime operation as long as Raman scattering cannot be used. But: in the manuscript the authors also use a constant lidar ratio (12415/21) though only night time data are used. If I misunderstood this, please emphasize the use of the actual lidar ratio whenever it was used. What are the consequences for the accuracy of the MUSA-calibration?

- The factor 0.0015 should be explained a little bit: is this a very hard requirement or do 90% of all measurements pass this requirement? Is it clear, that "problems with the sudden change of the calibration factor" are excluded by the 0.0015-criterion? What is meant with the "sudden change of the calibration factor automatically ..."? This sounds as if the ceilometer is self-calibrating? Isn't it the sensitivity that is automatically changed?

- 12416/24: "The use of relative calibration...". This is true, but it is not stated whether or not it was applied in this study! It is different for the CHM15k and the CHM15k-x (the latter is covered by the Wiegner and Geiß-paper) and is not required for the Nimbus version (see comments above).

- 12417/7: "discrepancies with respect to advanced or elastic..." What is an "advanced lidar profile"? Raman? Then, discrepancies are inherent as a ceilometer does not provide this. Why is "calibration ... often mandatory"? Why only "often"?

- 12417/14: "several parameters". Please make suggestions! What about a parameter, that monitors the high voltage of the APD of the CHM15k-x: is this relevant/available for the CHM15k during INTERACT?

- 12417/17: "...full access to the instrument information". What is "full"? In the sense of "all parameters", which leads me back to the previous comment.

- 12417/20: See above. Why do the authors apply a different calibration procedure for the CT25k?

- 12417/Eq. 1: I dont understand this. From the lidar equation:

$$P = C_L \ z^{-2} \ \beta \ T^2$$

we get

$$CC = \frac{P_{ceilo}z^2}{\beta'_{musa}} = \frac{P_{ceilo}z^2}{P_{musa}z^2}C_{L,musa} = \left(\frac{P_{ceilo}}{P_{musa}}\right) \ C_{L,musa} = C_{L,ceilo}$$

So, as $C_{L,musa}$ is known (Klett inversion with Rayleigh-calibration), $C_{L,ceilo}$ can be determined directly from the ratio of the signals at any range. This would be consistent with the general lidar equation. The ratio of the signals is a normalization factor and may be called $CC$ (or better, $C_C$), but the expression as it is defined in Eq. 1 is a lidar constant! Anyway: the accuracy of the determination of $C_{L,ceilo}$ depends on the accuracy of the calibration of MUSA. This should be emphasized and discussed.

- 12418: Fig. 1 is never mentioned in the text.

- 12418/8 (Fig. 2): The x-axis should be the date and not "number of case". First, to see the real temporal trend, and second to make a "case" of one inter-comparison distinguishable from a "case" from another ceilometer inter-comparison. By the way: why are the cases different for the different ceilometers: I only found the 0.0015 criterion; maybe a somewhat more detailed explanation should be added here (see also 12427/6). Are the CT25k-"cases" cloudy situations, while the other cases concern cloudfree conditions?

- 12418/Eq. 2: A $z^2$ is missing in the numerator. Then, it is consistent with the lidar equation (see above). Please explain what is the difference to $CC$ in view of the previous comment.

- 12418/14: I think it is better to call $T$ the transmissivity and not $T^2$.

- 12418/17: "... the variability of CL is 15%". This should agree with the magenta dots in Fig. 2. However, they vary between 1 and 6 with a mean of 2 or 3. Where is the 15% coming from, at least it is not obvious from the figure? In contrast the blue dots seem much "more constant" though it is stated that the variability is 58%. This is also confusing! Are the curves mixed up?

  If $C_L$ is meant to be $C_{L,musa}$ then this should be clearly stated. And explain why $C_{L,musa}$ varies over almost one order of magnitude (similar comments follow).

- 12418/18: "... was moved for an ...": Does that mean that MUSA was not available for the complete INTERACT-campaign? This should be specified for the sake of completeness.

- 12418/24: Where is the "internal" temperature of the ceilometer measured? At the laser, at the housing, or elsewhere? If unknown, ask the manufacturer. A clarification would help to follow the discussion of the correlations.

- 12419/2: "and the ceilometer temperature sensor within...": please add "external" to make clear which of the three "ceilometer temperatures" is meant. Otherwise this is not a surprise: it should be possible to measure temperature with an accuracy of $1\,\mathrm{K}$.

- 12419/3 ff: It is stated that the "...behavior of the internal temperature of the ceilometer looks quite well correlated with CC". What is a 'behavior'? What is 'looks quite well'? If a correlation coefficient is given for the ambient temperature (0.6), it should be possible to calculate it for the internal temperature as well, and to omit such vague expressions. So: What is the correlation between the internal temperature and CC? How large are the correlations between these temperature and $C_{L,musa}$? They seem to be similar, that leads me to the question on the quality of MUSA. I don't expect a high end instrument as MUSA to be so temperature sensitive.

  Furthermore, I have some fundamental problems with this paragraph: I would expect that the temperature of the detector has the by far largest influence on the signals. This temperature is however more or less stable. Why should the internal or external temperature influence CC? When the authors consider this as a realistic reason for the trends of CC, it would be necessary to write about those instrumental features that might be responsible for this effect (maybe ask the manufacturer). Could it be that the laser diode or the optical alignment is so sensitive to the ambient temperature (seems implausible)? If the ceilometer is indeed so sensitive to 'normal' temperature changes (during INTERACT it changed less than 20°C!) then the potential to extract quantitative aerosol profiles ($\beta_p$) seems to be quite limited (and makes it more or less useless for this purpose).

- 12419/14: It is not very clear what the message of the personal communication with Wiegner is: it seem that a user should replace broken parts in due time, in particular as it can be expected that they do not work forever. In such a case it is recommended that the manufacturer provides sort of an early warning message.

- 12419/20: Are there any information available from the manufacturer what the parameter "state of the laser" means (if not, a possible correlation cannot be understood)? Does the "number of laser pulses" just indicate the ageing of the diode (meaning that it should be replaced)?

- 12419/27: "no significant changes [of the background light] are expected". Does this agree with Fig. 3 when a factor of approximately 5 can be found? Or are these values so small (compared to daylight conditions) that they indeed can be considered as "more or less constant". On the other hand, a strong correlation between the temperature and the background light is found (12420/7), which sounds plausible if it is understood as electronic noise. Please extend the explanations.

- 12420/2: It would be nice to have regression lines in Fig. 3 (before and after 2. September).

  Please add regression lines also in Figs. 2 and 6.

- 12420/4: See also one of the previous comments: it would be advantageous to replace "case" by "date", and to explain why the number of cases (22 vs. 47) is different for Jenoptik and Vaisala.

- 12420/16: I appreciate the units $m^{-1} sr^{-1}$ as given in the text! Please use these units in all figures as well, not $Mm^{-1} sr^{-1}$! There are no reasons to use 'Mm': this is a scale that is absolutely irrelevant for vertical soundings of the aerosol distribution ('km' might be acceptable as well)!

- 12420/23: The "cloud calibration" is used to determine a "calibration constant". According to Sect. 3 this is $CC$, thus this symbol should be used here. See also my comments on how $CC$ and $C_L$ are related. If the concept of $CC$ is introduced by the authors it should be used throughout the paper whenever it is possible to facilitate the reading. Furthermore, a plot similar to Fig. 2 (left panel) should be included – then the paper is much more homogeneous. Such a figure is also required to fully benefit from Fig. 5. See also my comments on 'why is the cloud calibration applied'?

- 12420/25: According to the arguments of the authors the recommendation should not be "probably on the scale of months" but a calibration as a function of ambient temperature (if the authors' conclusions hold).

- 12420/26: Here "CC" appears the first time within Sect. 3.2. Please make this clear before (see previous comments).

- 12421/3: The IWV is plotted over time. This is reasonable and another argument to change "case" to "date". It should be explained why a full year is plotted, whereas INTERACT lasted only for 6 months.

- 12421/10: Again, $CC$ is discussed but not shown in a figure (see previous comments).

- 12421/15: Fig. 6, showing $CC$ for the Campbell ceilometer is another reason to include the corresponding figure for the Vaisala. For three cases it seems that $CC$ is zero. Is this possible? Are the four cases with $CC$ around 10 and more just malfunctions that should be ignored? For the remaining cases $CC$ seems quite stable (give average and standard deviation).

- 12421/17: Though most of the readers can guess what "SD" is, it must be explained.

- 12421/22: Internal and external temperatures are mentioned but not shown as before (Jenoptik). Is there a specific reason for this? What is the 'internal' temperature in case of the CS135s; this information should be added as it is specified (to some degree) for the other ceilometers (laser or detector).

  By the way: it would be useful to extend Tab. 1 (or adding a new Tab. 2) for a list of the most relevant housekeeping data stored in the data files of each ceilometer. In particular, in view of the authors' request to store system parameters, this would make sense. It would also help to substantiate the last sentence of this section ("other available system parameters").

- 12422/8: "stability of the overlap factor": What is meant by 'factor'? Isn't it a height-depending function?

- Sect. 3.4: Just to get it right: the ratio $\beta'_{ceilo}/\beta'_{musa}$ is determined as follows: $\beta'_{musa}$ from Klett-inversion, and $\beta'_{ceilo}$ from forward integration using $C_{L,ceilo}$ from the calibration process. Then, the problems with the variability of $C_{L,ceilo}$ and $C_{L,musa}$ also affect the following results of the overlap functions. To separate the issues of the stability of the systems (i.e. the individual $C_L$'s) and the stability of the overlap-function, it seems to me that it is more adequate to match the (uncalibrated) signals of the ceilometer and MUSA at a range, where full overlap is guaranteed and discuss the resulting ratios. As a consequence I expect a much better stability of the overlap functions for all ceilometers. In the present state I can't imagine any physical reason for the enormous variability presented in the manuscript (are there loose parts in the systems?). A consequence of the procedure presented in the manuscript is that the ratios shown in Fig. 7 do not approach 1 at 2 km (or so; it seems to be a consequence of a wrong calibration). So, how can the user benefit form the results? I also do not understand what the reader can conclude from the vertical bars: it primarily reflects the uncertainty of the calibration; nevertheless the overlap function

can be smooth with a comparably small uncertainty. Both effects should be considered separately. The author's conclusion, that a time-dependent overlap correction must be applied, seems unrealistic: how shall this be possible? What is the criterion to change from one function to another?

This section requires major revisions.

- 12425/Eq. 3: Attenuated backscatter should be defined when it first appears.

- 12425/9: $T^2$ should be replaced by $T$.

- 12425/11: "... has been neglected". Better: "... has been set to 1".

- 12426/3: Where is the 1% error coming from? Is Ansmann's paper considering the situation discussed here – I don't think so (their paper was on Raman lidars)? A rough estimate shows:

$$\frac{1064}{905} = 1.176$$

whereas

$$\left(\frac{1064}{905}\right)^{1.5} = 1.275$$

This is much more than 1%.

- 12426/6 (Fig. 8): A discussion of Fig. 8 is more or less lacking and should be added. For this purpose it might be useful to limit the vertical range to 4 km. In the lowermost part it seems (from visual inspection) that the Jenoptik signals are the worst.

- 12426/16: Why is it "useful to recall...", in no case signal above 4.5 km are shown.

- 12426/18: I am surprised that the authors state that the "agreement ... looks good". What is meant by "for the whole time series": only one profile is discussed. Is "below 1300 m" a typo (should it be "above 1300 m")? The deviation of the CHM15k-profile is very large! In case an overlap correction is applied it would probably be even worse. On the other hand the Vaisala-profile is very low and underestimates the aerosol backscattering already below 1 km. In summary I would expect a somewhat more critical discussion here. Maybe a re-evaluation is required.

- 12426/21: It is true that two ceilometers are affected by water vapor absorption. Nevertheless, the Campbell profile reproduces nicely the reference

profile, whereas the Vaisala doesn't. Do the authors mean that the absorption is responsible for the "noisy retrieval" of the Campbell and the total attenuation of the low energy Vaisala-ceilometer? On the other hand, the perfect agreement between MUSA and Campbell is surprising.

- 12426/25: "In this section ...quantitatively". This should have been the case already in Fig. 8!

- 12426/28: Omit the sentence "In addition, the relationship..." or make clear that different wavelengths are considered! Otherwise it is confusing.

- 12427/6: "The number of cases ... is not the same ... described in Sects. 2 and 3": See corresponding previous comment.

- 12427/3 ff: It might in general be difficult to get quantitative conclusions from Fig 10, nevertheless it would be nice to have a quantitative measure to be interpreted (are there any ideas?). From the figure it is not visible what happens at $1.0^{-10}$ m$^{-1}$ sr$^{-1}$ and what the relevance of this threshold is (it is very very low!). Reminder: change the Mm$^{-1}$ sr$^{-1}$ in the figure to be consistent with the text.

- 12428/5 ff: I don't see any reason for introducing $\alpha_p(355)$. A comparison between $\alpha_p(355)$ and $\beta_p(1064)$ only has a limited benefit: the ratio depends on the lidar ratio and the Angström exponent, i.e., on the microphysics of the particles, and thus, different 'cluster' will appear according to the aerosol type. A comparison between $\alpha_p(355)$ and $\beta'(1064)$ makes even less sense because a quantity at range $z$ is compared to a quantity depending on the range from 0 to $z$. As a consequence attribution of a pair of measurements to an aerosol type is hardly possible. Why do the three distributions (left column of Fig. 10) look so different?

  The concept behind this part of the manuscript must be presented in a convincing way, and the results must be discussed in detail. Otherwise just compare $\beta'(1064)$ of MUSA to $\beta'(1064)$ or $\beta'(905)$ of each of the ceilometers as it was done in Fig. 4 for the CT25k.

- 12430/16: "...experimental setup of CHM15k has the better performance". A clear ranking is missing (and certainly hard to provide), so the main criteria on which this statement is based should be briefly summarized.

- 12430/20 ff: Items 1 and 2 could be shortened, in particular when conclusions rely on estimates rather than on extensive calculations. Item 3 only concerns one out of three ceilometers. In total I expect that the conclusion must undergo significant changes when my comments (in particular on Sect. 3 and 4) are considered.