

Final response to the editor

General remarks

- Figure 1 was slightly modified in accordance with a comment of referee 2.
- Appendix B was added with two figures in accordance with a comment of referee 2.
- Appendix C was added with one table in accordance with a comment of referee 2.
- A scaling error in Figure 11 was corrected.

Response to Referee #1

We thank the reviewer for the constructive comments.

General comments

5 months are a short period to draw conclusions for the applicability of the method over a multi-year data set. In those 5 months you cover a large range of typical spring and early summer conditions. Please discuss risks in extrapolating your findings to the multi-year data set.

↳ The limitations of a 120 day dataset are now emphasized in the manuscript.

“Additionally, it must be emphasized that the measurement period of 120 days does not resolve possible annual or long-term trends and that the results may be site-specific.” (Conclusion)

You wrote that part of your results are “valid only for the site- and instrument-specific set-up”. Could you elaborate on the value of this paper for the overall scientific community?

↳ In the conclusion of the manuscript, it is now clarified that this study highlights the potential and limitations of ceilometers for aerosol remote sensing.

“This study highlights the potential of automatic lidars and ceilometers for aerosol remote sensing.” (Conclusion)

Specific comments

Page 2, line 38 - This network will soon be integrated into the ACTRIS network

(Pappalardo, 2018; Laj et al., 2024). -

Note: Wasn't Earlinet part of ACTRIS from the beginning?

↳ This is correct, and the manuscript was updated accordingly.

“This network was integrated into the Aerosol, Clouds and Trace Gases Research Infrastructure (ACTRIS) network (Pappalardo, 2018; Laj et al., 2024).” (Introduction)

Page 2, line 40 - the total number of installed instruments is currently limited to 27 -

Note: I don't think the number is limited, it's just low for the reasons you mention. I suggest to remove "limited to".

↳ The manuscript was updated accordingly.

“[...], the total number of installed instruments is currently 27 (TROPOS, 2025), [...]” (Introduction)

Page 2, line 53 - This study aims to quantify the uncertainty of using an existing multi-year ceilometer dataset to derive aerosol properties. -

Note: This is misleading. You may aim to apply your workflow to the multi-year data set. However, here, only 5 months of data are used. Please rephrase.

↳ The manuscript was updated accordingly.

“To quantify the uncertainty in using an existing multi-year ceilometer dataset to derive aerosol properties, this study evaluates a five-month dataset.” (Introduction)

Page 4, line 75 to 77 - JOYCE is equipped with a suite of remote sensing, in situ and radiation measurement instruments. These instruments are among others an ABB

Atmospheric Emitted Radiance Interferometer (AERI), a Metek MIRA-36 Doppler cloud radar, two ceilometers (Vaisala CT25k, Luft CHM15k, Sec. 3.1), a RPG humidity and temperature profiler and a CIMEL sun photometer. -

Note: Present separately the instruments that are used in this study, and any additional instrumentation at JOYCE. Or remove those which are not relevant to this study.

↳ The Atmospheric Emitted Radiance Interferometer was removed from the list. An additional sentence was added, to indicate, which instruments are directly used in this study and which instruments are used as references or to run the Cloudnet target classification.

“The instrumentation comprises a Metek MIRA-36 Doppler cloud radar, two ceilometers (Vaisala CT25k and Luft CHM15k; see Sec. 3.1), an RPG humidity and temperature profiler, and a CIMEL sun photometer. In this study, only the ceilometer measurements are analyzed using the sun photometer to estimate the lidar ratio (Sec. 4.2), while the remaining instruments provide input for the Cloudnet target classification (Sec. 3.3).” (Observational site)

Page 5, line 89 - following equation (Hervo et al., 2016):

Note: The lidar equation is older than that. Hervo has described it well for the

application to ceilometers, but the equation itself was used long before that. Klett 1981 could be used as a reference for example.

↳ The references were updated to include both publications:

“(Klett, 1981; Hervo et al., 2016)” (3.1 Ceilometers)

Page 6, line 114 - It does not filter out any particles larger than 10 -

Note: I'm not sure how to understand “filter out” in this context. Do you mean that no particles larger than 10 micrometre pass through the instrument for sampling, or that only particles larger than 10 micrometre are sampled? I guess it's the first, but then I'd suggest rephrasing this sentence.

↳ In contrast to other PM10 inlets with a defined cutoff size, this inlet does not block aerosols with a certain size. This sentence is confusing at this point and is not required to understand the setup. Therefore, this sentence has been removed from the manuscript.

Page 6, line 118 - OPS setup is illustrated in panel (b) of in Fig. -

Note: remove “in”

↳ The typo was corrected.

Page 7, line 128 - taken as 1.5 g cm⁻³ -

Note: Why this value?

↳ Without knowledge about the “real” particle density, this value was selected as a typical density for aerosols in the range specified by Osborne et al., 2024 as already indicated in the manuscript.

“[...] the particle density (typically in the range 1.2 to 2.5 g cm⁻³ (Osborne et al., 2024)) taken as 1.5 g cm⁻³ [...]” (3.2 Optical Particle Sizer)

Page 8, line 143 - At JOYCE, the Cloudnet target classification -

Note: Out of curiosity, which of the ceilometers is used for the Cloudnet target classification at JOYCE?

↳ The Cloudnet target classification at JOYCE is based on the Lufft CHM15k. This information has been added to the manuscript.

“At JOYCE, the Cloudnet target classification, which utilises the Lufft CHM15k ceilometer, [...]” (3.3 Cloudnet)

Page 11, figure 3 caption - (e) ground air pressure -

Note: Measurement height is mentioned for all parameters. Is air pressure measured at 2 m? You could add it here for consistency.

↳ Yes, the air pressure was measured 2 m above ground. The manuscript was updated accordingly.

“[...], (e) ground air pressure p at 2 m, [...]” (Figure 3, caption)

Page 13, line 226 - In the time range from 0:00 to about 8:00 low aerosol mass concentrations of -

Note: From figure 4 it looks like aerosol mass concentration was low until around 9:00.

↳ Yes, that is true. The manuscript was updated accordingly.

“In the time range from 0:00 to about 9:00 low aerosol mass concentrations [...]” (5.1 Dataset overview and example day)

Page 15, line 260 - the statistical relation obtained here is valid only for the site- and instrument-specific set-up. -

Note: And it is only valid under certain conditions. Those which were observed in the 5 months and not filtered out.

↳ An additional sentence was added to point this out.

“Additionally, they represent only measurements during defined environmental conditions, as described above, from a five-month dataset.” (5.2 Aerosol mass concentration and attenuated backscatter coefficient)

Page 17, line 272 - extinction coefficient of $1.4 \times 10^{-3} \text{ km}^{-1}$ -

Note: Do you have an uncertainties estimate of this?

↳ We estimated an uncertainty of 25 % for the extinction coefficient based on the signal resolution of the CT25k. The paragraph was changed accordingly:

“The intercept value can be converted to a pure air backscattering coefficient of about $(1.7 \pm 0.4) \times 10^{-4} \text{ sr}^{-1} \text{ km}^{-1}$. Taking the theoretical Rayleigh scattering lidar ratio of $8\pi/3 \text{ sr}$, an extinction coefficient of $(1.4 \pm 0.4) \times 10^{-3} \text{ km}^{-1}$ at 120 m was obtained. This estimate is in reasonable agreement with a value of $(1.50 \pm 0.04) \times 10^{-3} \text{ km}^{-1}$ for 906 nm derived from the literature for the measurement conditions (Bucholtz, 1995).” (5.3 Aerosol mass concentration and extinction coefficient)

Page 17, line 273 - with a literature value of $1.53 \times 10^{-3} \text{ km}^{-1}$ -

Note: Did they report uncertainties? Your value might be within the uncertainty range.

↳ The previously stated literature value of $1.53 \times 10^{-3} \text{ km}^{-1}$ applied for standard atmosphere conditions at sea level and is very accurate (0.1 %). However, the actual value depends on temperature and pressure. We therefore derived a reference range of $(1.50 \pm 0.04) \times 10^{-3} \text{ km}^{-1}$ based on the observed mean temperature and mean ground pressure (corrected for a height of 120 m) where the uncertainty covers the standard deviations of temperature and pressure.

“[...] a value of $(1.50 \pm 0.04) \times 10^{-3} \text{ km}^{-1}$ for 906 nm derived from the literature (Bucholtz, 1995).”
(5.3 Aerosol mass concentration and extinction coefficient)

Page 23, line 314 - In contrast, the C_m to β_{Mie} relationships and the S_{Mie} exhibit a much greater variation -

Note: I'm not sure, where I can see evidence of this. Please explain which figure or table you are referring to. Doesn't this contradict R values shown in table 3, which are higher for $C_m \sim \beta_a_{Mie}$ compared to $C_m \sim \sigma_a_{Mie}$?

↳ This statement refers to the dependence on refractive indices in Tab. 3, which is now clarified in the manuscript.

“In contrast, the C_m to β_{Mie} relationships and the S_{Mie} exhibit a much greater dependence on refractive index, compared to the C_m to σ_{Mie} relationships (Tab. 3) because of strongly increasing backscattering efficiencies with increasing refractive index.” (5.4 Calculation of aerosol backscatter and extinction coefficients)

Page 23, line 322 - linear relationships between C_m and σ_a are robust with regard to variations of refractive -

Note: Again, I am a bit lost here. What exactly do you mean with "robust"? The lower R values indicate that $C_m \sim \sigma_a_{Mie}$ is less "robust" than β . Also, values of r_3 are decreasing with increasing m . Could you please elaborate?

↳ The statement regarding robustness refers to the changes of the C_m/σ slopes with refractive index. This is relevant because a natural aerosol will be composed of particles with different refractive indices and the composition can vary with time. This is now clarified in the manuscript.

“Nevertheless, the theoretical calculations summarized in Tab. 3 confirm that the slopes of the empirical linear relationships between C_m and σ_a are robust with regard to variations of refractive indices in the particle phase. This is relevant because a natural aerosol will be composed of particles with different refractive indices and the composition can vary with time.”
(5.4 Calculation of aerosol backscatter and extinction coefficients)

Page 26, figure 11 - Mean aerosol number size distribution $dC_n/d \log D$ (A), cumulative, relative contribution to total aerosol mass concentration C_m (B) and to simulated aerosol extinction coefficients σ_{Mie} (C) (01 February to 20 June 2023 at the meteorological tower in 120 m). -

Note: I find this caption confusing. Suggestion: "A) Mean aerosol..., B) cumulative, relative ..., and C) cumulative, relative contribution to simulated (01 February...)"

↳ The caption was updated accordingly.

“A) Mean aerosol number size distribution $dC_n/d \log D$, B) cumulative, relative contribution to total aerosol mass concentration C_m , and C) cumulative, relative contribution to simulated aerosol extinction coefficients σ_{Mie} (01 February to 20 June 2023 at the meteorological tower in 120 m).” (Figure 11, caption)

Page 28 - It is recommended to reevaluate the approach with state-of-the-art instrumentation for future applications. -

Note: I'd also suggest to extend the time period of this experiment and investigate the behaviour of different aerosol types. Possibly it could be an option to find an existing data set from elsewhere. I know that Mace Head used to have an aerosol inlet on a small tower. They also have two ceilometers (Lufft and Vaisala, both older models though). There might be other sites sampling aerosols at an elevated height with a (Vaisala) ceilometer.

↳ We thank the reviewer for this suggestion. We consider contacting researchers from other sites who may have comparable datasets, as now pointed out in the conclusion. A continuation of the study at JOYCE is currently not foreseen because of the discussed limitations of the in situ instrument.

“This could be achieved for example by using already existing long-term datasets from other field sites.” (Conclusion)

Technical corrections

Page 23, line 320 - in the aforementioned ceilometer networks (Osborne et al., 2024) in agreement with the value used in this work. However, a -

Note: There is a typo: aforementioned

↳ The typo has been corrected.

Page 25, line 330 - correctly discarded because of scattered light signals that are too high. On the other hand, such particles will contribute to the -

Note: There is a typo: particles

↳ The typo has been corrected.

Page 26, figure 11

Note: There is a typo in the secondary y-axis label: contribution or contributions

↳ The typo has been corrected.

Response to Referee #2

We thank the reviewer for the constructive comments.

All abbreviations need to be explained (when introduced) in the abstract, but again, in the full text body.....

↳ *The abbreviation for ACTRIS is now also explained in full text body.*

Line 27: What about the role of aerosol particles in ice nucleation processes?

↳ *The role of INPs is now introduced in the manuscript with an additional reference.*

“Moreover, the presence and composition of aerosols play an important role in ice nucleation processes (Burrows et al., 2022).” (Introduction)

Line 30: Explain PM.

↳ *The manuscript was updated accordingly.*

“(PM: Particulate Matter)” (Introduction)

Line 37: Explain EARLINET.

↳ *The manuscript was updated accordingly.*

“[...] are included in the European Aerosol Research Lidar Network (EARLINET) (D’Amico et al., 2015).” (Introduction)

Line 38: Explain ACTRIS.

↳ *The manuscript was updated accordingly.*

“[...] was integrated into the Aerosol, Clouds and Trace Gases Research Infrastructure (ACTRIS) network (Pappalardo, 2018; Laj et al., 2024).” (Introduction)

Line 52: To my opinion an automatized trustworthy lidar data analysis at 906 nm is only possible in combination with AERONET sun photometer observations (AODs at 870 and 1020 nm). Is that the case, at least for Juelich? Any comment on that is appreciated.

↳ *Supporting the ceilometer analysis based on AERONET products would be a promising approach. The a-Profiles algorithm, used in this thesis, has plans to implement this. However, this feature has not yet been implemented. JOYCE is already contributing to*

AERONET. A sentence to highlight the current limitations and a possible synergy with AERONET was added to the manuscript.

“However, a-priori assumptions on the aerosol optical properties have to be made (Sec. 4.2). Although not yet implemented, there are plans to integrate AERONET (Aerosol RObotic NETwork, Holben et al. (1998)) aerosol optical depth measurement into the retrieval algorithm to address this issue (Mortier, 2022).” (Introduction)

Figure 1 is confusing: JOYCE instruments seem to be on top of a container with a vertical extension of 20m. Please indicate a building in the sketch, a building with windows, for example...

↳ For clarification, a door and windows were added to Figure 1.

Line 100-101: The overlap profile correction is a severe error source in your attempt to compare the ceilometer profile data with the in situ measurements, just 100 m above the ceilometer. And it doesn't matter whether the lidar is a monoaxial or biaxial system. In the case of the CHM15, the overlap is almost complete at 350 m height above the lidar, that makes sense. Then it is difficult to compare these data with the in situ observation at 100 m height above the ceilometer because the actual overlap is not well known and continuously varies a bit with temperature in the ceilometer system. The overlap function typically shows a diurnal cycle. That means the correction remains problematic and introduces uncertainties. And all these overlap problems are overcome in the case of the Vaisala ceilometer? You can even make use of the lowest range gate (i.e., the first signal bin)? The overlap profile is super constant during the day, over days, weeks, and months?

After many decades of lidar experience in the field, I must say: This is impossible! A slightly varying overlap profile must be considered for every system. And for overlap values less than 0.7 (with 1 for complete overlap), the correction is almost useless, the errors can become extremely large. Thus, we need clear statements about the uncertainty in the overlap correction in the case of the Vaisala ceilometer. At what height is the overlap complete, at what height is an overlap of 0.7 reached? These numbers should be mentioned.

↳ The overlap function for the Vaisala CT25k ceilometer is not published. However, a manufacturer-based overlap correction is internally applied. Based on the manufacturer's specification (Münkel et al., 2006), usable data can be assumed to start at 0 m. However, possible artefacts in the lowest 70 m were described for a comparable ceilometer (Vaisala CL31, Kotthaus et al., 2016). We assume that at 122 m (102 m above the instrument) the measurements are representative. This information was added to the manuscript.

“However, hardware and software related artefacts were reported for measurements below 70 m for a comparable ceilometer (Vaisala CL31) (Kotthaus et al., 2016).” (3.1 Ceilometers)

Line 114: What is the cutoff signature of the optical particle counter? Probably even particles with diameters of 2-5 μm will not be counted correctly when 10 μm particles are not counted at all.

Are the particles dried before the optical measurement? In the apparatus, the temperatures are probably significantly higher than outside (in the atmosphere) so that the particles lose water before the optical measurement is conducted. Please expand the discussion on this point.

↳ In contrast to other PM10 inlets with a defined cutoff size, this inlet does not block aerosols with a certain size. This sentence was confusing at this point and is not required to understand the setup. Therefore, this sentence has been removed from the manuscript.

↳ This following information about the size range of the instrument has been clarified in the manuscript.

“Size bins were selected based on a predefined protocol (TSI Default) as summarised in Tab. A1 in the appendix to cover the full size range the instrument provides (0.3 - 10 μm).” (3.2 Optical Particle Sizer)

↳ We thank the reviewer for raising the point of higher temperature within the OPS setup. We added the information that “the air was not actively dried” in Sec. 3.2. The potential consequences of unintentional drying are now discussed in a new paragraph in the manuscript:

“An additional factor is the possibility of unintentional drying of the aerosol in the OPS. The sample stream was not actively dried to preserve the ambient size distribution. Hygroscopic growth within the sampling line is not likely because the temperature inside the box is slightly higher than outside, caused by the waste heat of the instruments. Accordingly, a drop in RH is expected, which could lead to a size-decrease of a fraction of particles prior to detection. It is expected that at higher relative humidities the effect is more pronounced because growth factors typically increase non-linearly with humidity (Titos et al., 2014). To identify a potential bias, the regression analysis for the relation between σ_a and C_m (Eq. 15) was reevaluated as a function of the upper limit in ambient relative humidity. The results are presented in Section C in the appendix showing an increase in the slope with decreasing humidity limits. Though within the estimated uncertainties, these data indicate a clear trend consistent with a drying effect by a temperature increase in the OPS. This trend is levelling out towards the lowest limit of 50 % RH. Accordingly, EMC values decrease to values in better agreement with literature.” (5.5 Size distributions and potential OPS artefacts)

↳ Additionally, the related Table C1 has been added to the appendix:

Appendix C: Humidity Effects

Table D1. Linear regression analysis of the relation between σ_a and C_m for different upper limits in relative humidity (RH), showing number of data points N , correlation coefficient R , slope of the regression line, uncertainty of the slope and extinction to mass coefficient (EMC).

RH Limit [%]	N	R	slope [$\mu\text{g m}^{-3}\text{ km}$]	MAPE [%]	EMC [m^2g^{-1}]
50	1345	0.67	690	35	1.5
60	2372	0.67	680	35	1.5
70	3392	0.68	630	35	1.6
80	4258	0.70	570	35	1.7
90	5108	0.73	490	37	2.0
95	5443	0.73	460	39	2.2

Line 165: Did you check the quality of the determination of the calibration constant by comparing the height-integrated extinction profiles with respective AERONET photometer observations (at 870 and 1020 nm)?

Just an idea, in cases of good agreement (on clear sky days with well-mixed boundary layer and an almost aerosol-free free troposphere, no cirrus) one could use the AERONET size distribution to check the in situ measured size distribution (Is there an aerosol drying effect or not?) and one could even fill the in-situ-measured size distribution for sizes below 300 nm with values from AERONET.... Just for the comparisons later on (simulations versus observations).

↳ The following paragraph has been added to Section 5.3:

“In contrast, a comparison of the vertically integrated σ_a with AERONET aerosol-optical-depths (AOD) shows a significant underestimation. This can be explained by the low SNR of the CT25k at higher altitudes (Markowicz et al., 2008) leading to an underestimation of the derived σ_a . The comparison is provided in Section B of the appendix.” (5.3 Aerosol mass concentration and extinction coefficient)

↳ The following paragraph and figure about an AERONET AOD comparison have been added to the appendix:

Appendix B: AERONET Comparison

Based on the results of this study, a comparison of ceilometer (CT25k) aerosol optical depth (AOD) with AERONET AOD data was performed. It was found that ceilometer AOD was significantly lower than AERONET AOD (Fig. B1). This can be explained by the reduced signal-to-noise ratio of the CT25k ceilometer at altitudes above about 1.2 km (Markowicz et al., 2008).

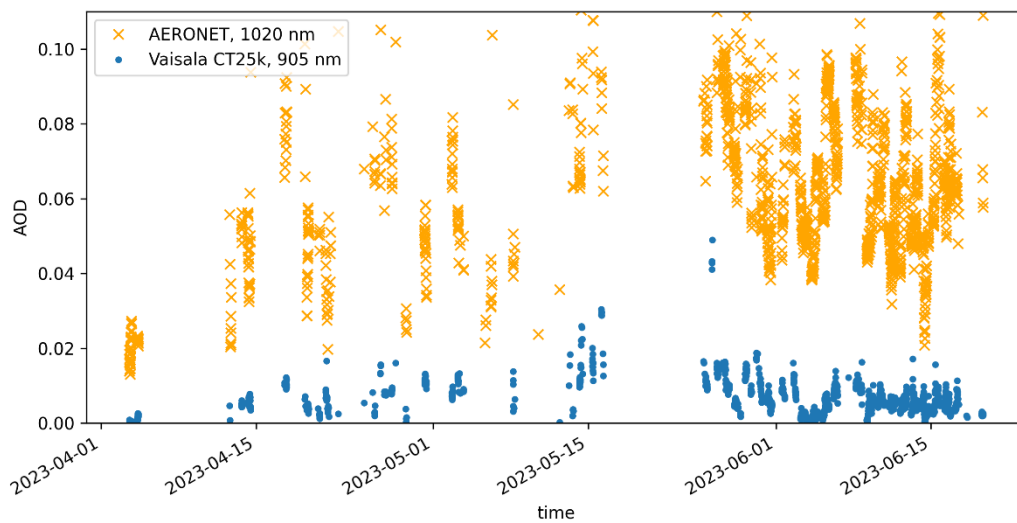


Figure B1. Timeline of aerosol-optical-depth (AOD) from AERONET and vertically integrated σ_a from Vaisala CT25k for the period 01 April to 20 June 2023.

↳ The idea to complete aerosol size distributions below $< 0.3 \mu\text{m}$ from AERONET observations is now discussed in the manuscript:

“An approach to quantify the contribution of particles $< 0.3 \mu\text{m}$ based on AERONET showed an underestimation of the total volume concentration by this limitation of approximately 20 % as shown in Fig. B2 of the appendix. However, it should be noted that AERONET refers to column data, whereas the OPS refers to a measurement altitude of 120 m. For this reason, the 20 % are considered as an upper limit, assuming a higher fraction of large particles in the boundary layer.” (5.5 Size distributions and potential OPS artefacts)

↳ In addition, the following analyses was added to the appendix:

“The contribution of the different aerosol sizes bins to the total aerosol volume concentration as a comparison between OPS measurements at 120 m and AERONET column data is shown in Fig. B2.”

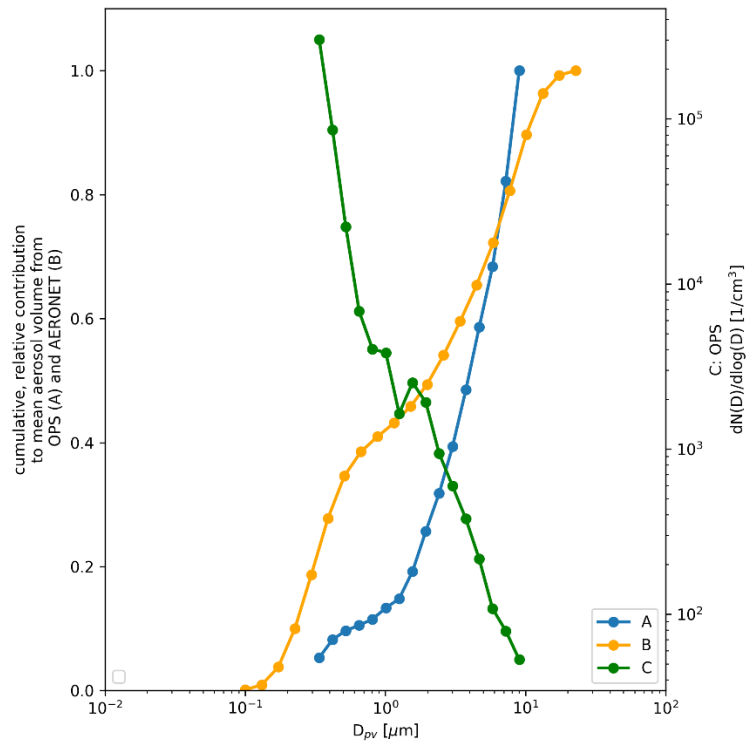


Figure B2. Cumulative, relative contribution to mean aerosol volume from OPS measurements at 120 m (A) and AERONET data (B), C: number size distribution (OPS at 120 m) and column number size distribution (AERONET). All for the period 01 April to 20 June 2023 where AERONET data were available.

“The lower limit of the OPS measurement range at $0.3 \mu\text{m}$ causes an underestimation of the total volume concentration by approximately 20 %, assuming the same contribution for particles $< 0.3 \mu\text{m}$ for AERONET and the OPS.”

Lines 174-189: If you use the Klett method or the mentioned forward method, the impact of uncertainties in the overlap correction is always large (in the near range). At the end you should compare the ceilometer-derived AOD with respective AERONET observations, for many cases, to check the quality of the ceilometer extinction profiles.

↳ The use of AERONET AOD as a reference was discussed in the previous comment and is addressed now in the new Appendix B and Figure B1.

Line 226: If the conditions were ideal for the comparison with in situ observations, they were ideal for AERONET comparisons as well (I hope). Then, you can make use also of the size distribution derived from the AERONET observations... Please expand discussion on this.

↳ The use of AERONET size distributions as a reference was discussed above and is addressed now in the new Appendix B and Figure B2.

Line 279, Eq.(15): The extinction-to-mass conversion factor is $460 \text{ km } \mu\text{g m}^{-3}$. If I compare this conversion factor with the respective extinction-to-volume conversion factor for 910 nm (continental aerosol pollution) in the recently published discussion paper of

Ansmann, A., Hofer, J., Mamouri, R.-E., Haarig, M., Baars, H., and Wandinger, U.: Aerosol microphysical properties and CCN/INP information from lidar and ceilometer profiles: POLIPHON update, EGU sphere [preprint], <https://doi.org/10.5194/egusphere-2026-648>, 2026.

I end up with an extinction-to-mass conversion factor of about $675 \text{ km } \mu\text{g m}^{-3}$ when assuming a density of 1.5 g cm^{-3} (and my computations do not have a bug). Please check by yourself. One should consider these numbers 675 vs 460 in the discussion later on.

↳ The following has been added to the manuscript:

“A recent study reported an 911 nm extinction-to-volume coefficient of $0.448 \text{ Mm } \mu\text{m}^3 \text{ cm}^{-3}$ for continental aerosol (Ansmann et al., 2026), confirming a lower EMC of $1.5 \text{ m}^2 \text{ g}^{-1}$ based on a common density of 1.5 g cm^{-3} .”

↳ We attribute the difference (675 vs 460) to lower detection limit of the OPS at $0.3 \mu\text{m}$, as already discussed in the manuscript (Sec. 5.5).

Line 291: ... and measured OPS size distributions. Did you assume ambient conditions? Did you consider drying of aerosol particles in the OPS?

In all subsequent simulations one may try to fill the size distribution (for sizes smaller than 300 nm) by means of AERONET size distribution products or, if available. You may also make use of standard in situ observation at Juelich, covering the size distribution down to 10-20 nm.

↳ The Mie calculations were performed based on the measured OPS size distributions, which were taken without active drying. The possibility of unintentional drying was discussed above.

↳ The limitations of using AERONET size distribution products for 120 m were discussed above.

↳ There are no additional standard in situ observations available in Jülich.

Lines 325-336: Again, where the particle dried or not? Was it possible to really measure ambient aerosol optical properties and thus the size distributions of aerosols at ambient conditions. This is what the lidar measures. Please expand the discussion on this topic.

↳ There is no active drying. The possible magnitude of unintentional drying was discussed above.