Dear reviewer,

thank you for careful reading of the manuscript and for providing good and constructive suggestions.

We considered almost all of the comments and suggestions.

All essential changes in the main text body are **marked in red.**

Reviewer 1

Ansmann et al. provide an update to the widely used POLIPHON technique for deriving several aerosol microphysical properties from lidar observations. Due to the simplicity of the POLIPHON framework, it has been widely adopted in the lidar community. In this manuscript, the authors incorporate a larger set of AERONET observations covering a broader range of aerosol conditions and provide updated optical-to-microphysical conversion parameterizations for multiple aerosol types, including continental haze, biomass-burning smoke, volcanic sulfate, mineral dust, and marine aerosols. The updated parameterizations include wavelengths commonly used by ground-based ceilometers, thereby enabling their direct application for deriving aerosol microphysical properties. The study also includes the ATLID operating wavelength (355 nm), and the aerosol types considered in POLIPHON are consistent with those used in the ATLID aerosol model. This alignment should facilitate the application of the POLIPHON approach to spaceborne lidar observations and help bridge the transition from CALIPSO to EarthCARE products.

I have a few minor comments that are mostly aimed at highlighting some missing information and discussion that may further improve the clarity of this already well-written manuscript. I recommend publication in Atmospheric Measurement and Techniques after the authors address these comments.

Minor comments:

Table 2: It would be helpful to indicate the expected uncertainties associated with each of the particle microphysical properties, similar to what was provided in previous studies by the authors (e.g., Table 1 in Ansmann et al., 2019).

We added typical uncertainties in the Table 2 (page 6).

Line 121: It would be worthwhile to state that conditions with RH>75% are particularly relevant for aerosol observations in the vicinity of clouds, where retrieving aerosol microphysical properties accurately is key to studying aerosol-cloud interactions.

We added this information in Sect. 2.4 on page 5-6.

Line 140: Choudhury et al. (2026) recently reported a similar inefficiency of dust particles (with radii larger than 100 nm) to act as CCN, based on their observed negligible impacts on liquid cloud droplet concentrations. I suggest the authors provide a short discussion on whether dust n_{100} can still be used as a proxy for dust CCN concentrations. The parameter n_{60} may instead be more appropriate, as the authors later suggest that these particles may be contaminated or mixed/coated with soluble continental aerosols. It should also be noted that such mixtures represent internal mixing states, whereas the aerosol backscatter separation technique of Tesche et al. (2009) and Mamouri and Ansmann (2014) assumes externally mixed aerosols. This inconsistency may introduce additional uncertainties in the separated aerosol properties, which may propagate to the converted microphysical properties.

We added a discussion in Sect. 2.5 (page 7). Our observations in polluted Cyprus, Cabo Verde (polluted winter season), and Tajikistan (complex mixtures of aged, contaminated dust and anthropogenic haze) clearly indicated that the aerosol can be regarded as an external mixture of contaminated dust and non-dust aerosol. As long as hygroscopic aspects do not play a role, the depolarization characteristics of dust remain unchanged (when compared to pure dust particles).
Note, that we work on a paper based on multiyear Dushanbe observation of the role of aged, contaminated dust on liquid-water cloud formation. We added the publication of Choudhury, G., Goren, T., & Tesche, M. (2026). Satellite observations show negligible impact of mineral dust on cloud droplet number. Geophysical Research Letters, 53, https://doi.org/10.1029/2025GL120234 to the references.

Section 3.3: Reference to AERONET v3 is missing in the manuscript:

We added in Sect. 3.1 and in 3.3: Sinyuk, A., Holben, B. N., Eck, T. F., Giles, D. M., Slutsker, I., Korkin, S., Schafer, J. S., Smirnov, A., Sorokin, M., and Lyapustin, A.: The AERONET Version 3 aerosol retrieval algorithm, associated uncertainties and comparisons to Version 2, Atmos. Meas. Tech., 13, 3375–3411, https://doi.org/10.5194/amt-13-3375-2020, 2020.

Lines 261–274: This section may be confusing to readers. The rationale for treating the wavelengths 355 nm and 1064 nm separately is not clearly explained. In particular, it would help to state at the beginning of the paragraph that the absorbing properties of dust and smoke in the UV cannot be accurately reproduced by simply extrapolating AOT from other larger wavelengths. Presenting this point upfront would clarify why special attention is required for the 355 nm channel. In addition, the statement in line 264 about downloading additional AOT data appears unnecessary and could be removed to improve clarity.

We changed the text accordingly, see Sect. 3.3, pages 12-13.

Line 275: It is unclear what is meant by "closest to the observations". Please clarify which observations are being referred to here.

We changed the description in Sect. 3.3: We considered only those 355 and 1064 nm AOT values in the further analyses that were temporally closest to the observations used in the version-3 inversion procedures.

Line 371: It is also worth noting that spaceborne lidar applications rely on aerosol observations in the lateral vicinity of clouds to study aerosol–cloud interactions. However, enhanced hygroscopic growth at high relative humidity can increase aerosol scattering by up to ~80% near these cloud boundaries (Twohy et al., 2009). Therefore, studies considering POLIPHON application to spaceborne lidar should consider excluding profiles in direct contact with a cloudy column (approximately ~5 km for a CALIOP Level-2 profile).

We added this aspect in the discussions, in Sect. 3.5, page 17.

I presume that the range reported for the mean values in Table 12 corresponds to one standard deviation. I suggest including similar ranges in Tables 8–11 for other aerosol types. This would also allow the uncertainty range to be propagated to the overall mean of the conversion factors reported in Table 13. Also adding similar standard deviations to other tables (Like Table 14) would improve clarity.

We added the one-standard-deviation information in Table 12 (caption), and added the one-standard-deviation values in Tables 13 and 14 (now Table 14 for smoke and Table 15 for sulfate). We leave out to add this information in Tables 8-11. The information about the coefficient of determination (R2) provides already the information about the accuracy. We do not want to overload Tables 8-11 by adding all these numbers.

Typos and language edits:

Line 15: replace “observations and that permit” with “observations that permit”

Improved

Line 17: replace "monitor aerosol” with “continuously monitor” and delete “and this continuously” part at the end.

Improved

Line 52: replace “considering” with “representing”

Improved

Line53: replace “caused by” with “because of”

Improved

Line 141: There is an additional space before the period. Also, remove the word “side” after “On the other hand…”

Improved

Line 147: Check the citation format Line

Improved

187: Correct “and and”

Improved

Line 192: remove “study”

Improved

Line 370: Correct “microphysiccal"

Improved

Line 415: Please rephrase the sentence *“The highest R^2 values are given in the regression analysis to obtain $c_{s,i}$and $c_{v,i}$”*. For example, change it to *“The highest R^2 values are usually obtained for $c_{s,i}$and $c_{v,i}$.”*

Improved

Line 540: Remove repeated words “at lower extinction”

Improved

Dear reviewer,

thank you for careful reading of the manuscript and for so many useful and detailed comments.

We considered almost all of them in the revised version. Motivated by the paper of Cordoba-Jabonero et al. (2023), we now include even tropospheric volcanic sulfate aerosol in our studies. More details are given below.

All essential changes in the main text body are **marked in red.**

Reviewer 2

This paper presents an update of the conversion factors used in the POLIPHON method for deriving the mass concentration and both CCN and INP concentrations related to five key aerosols at four relevant wavelengths regarding the vertical aerosol observations from active remote sensing networks (ceilometers, lidars) and space lidar missions. In addition, this update is performed by applying an approach (least-squares estimation, LSE) different from that used in previous works of the authors. Therefore, the outcomes of this work are rather relevant, representing an extension of the conversion factors at other wavelengths and for other types of aerosol, and hence it deserves to be published. However, some issues should be deeply discussed before it is accepted for publication.

General comments:

The title of the work should be modified, because it is ambiguous. Neither lidar nor ceilometer profiles are used to obtain the conversion factors for retrieving the aerosol microphysical properties and CCN/INP information needed for determining mass and CCN/INP concentrations. Instead, the AERONET products are used to obtain the conversion factors, which would be applied with the ceilometer and lidar extinction profiles (explicitly not shown in this work).

We changed the title.

In a previous work of the authors (Ansmann et al., 2019b), a relevant, noteworthy variability in the conversion factors for dust in dependence of the regional/continental dust source was investigated. This matter deserves to be commented and discussed in the present work, where that variability has not been taken into account. Section 4.4, as it is now, is incomplete, leading to confused conclusions.

We added a discussion on regional aspects now in Sect. 4.2.

Specific comments:

Page 2, lines 47-57: Authors explain why they used in this work a better approach (LSE) for retrieving CCN-related conversion factors, different from that previously used (e.g., Mamouri and Ansmann, 2016), which is based on the methodology of Shinozuka et al. (2015), due to the discrepancies found with the findings of Kulkarni et al. (2025) in the case of continental aerosol pollution, and for preventing noisy n_{50} values and removing potential outliers. However, it is not explained for the other aerosol types (n_{100}, n_{250}, …), which the Shinozuka's approach could be suitable for. It should be discussed and clarified.

We extended the discussion to cover n100 in Sect. 1, on pages 2-3. All the other conversion factors (providing n250, surface, volume) were never derived by using the Shinozuka approach.

Page 13, Table 5: Please, clarify in more detail why the interval of AOT (532 nm) is restricted up to 0.5 for mineral dust cases, as there could likely be higher AOT values. This can affect the selection of cases used in the LSE approach.

The discussion of this aspect is added in Sect. 3.3.

Pages 18-19, Figures 3-4: It should be included the corresponding figures for all the aerosol case studies shown in Table 7, and not only for mineral dust (IZA, Fig. 3) and continental pollution aerosol (BEI, Fig. 4). That is, include, please, also the figures for marine aerosol (AMS) and continental pollution (PRE).

We follow this suggestion, see improved Figure 3 (a,b,c,d). Figure 4 is therefore canceled.

Page 29, Figure 9: Please, indicate which one of the conversion factors shown in Ansmann et al. (2019b), and denoted as A19,d, is used in this Figure to represent the 2016 conversion method for dust. Different regional/continental conversion factors depending on the mineral dust source are shown in A19.

Good point, we forgot to mention: We used n100,d = 8 .0 x ext^0.8. This describes roughly the averagen100,d when using Saharan and Asian dust conversion factors. We state that now in Sect. 4.4.

Pages 31-32, Summary and concluding remarks: It is mentioned about the conversion factors for volcanic ashes (though not included explicitly in this work), as well as for the stratospheric volcanic sulphate aerosol in the manuscript in overall. But the tropospheric volcanic sulphate aerosol is not mentioned. Authors should introduce some discussion, at least, regarding those also relevant types of aerosol omitted. This would improve the work. As a reference, but you can find more, see Córdoba-Jabonero et al. (2023).

Motivated by this comment, we downloaded the AERONET data for the stations La Palma (Sep 2021 to Dec 2021), Mindelo (Sep 2021, Gebauer et al., ACP 2025) and, in addition Leipzig, (Eyjafjallajoekull, 19-20 April 2010, Ansmann et al., JGR, 2011). We added the stations in the map in Figure 2, in Table 3 (AERONET station), and in the analysis Sect. 4.4, in the new Table 15 (for volcanic sulfate) and in Figure 7.

Other minor comments:

Page 1, line 18: Revise the final of the sentence: "…with high vertical resolutions and this c ontinuously".

Improved

Page 2, line 35: Add a 'line feed' before "(1) The aerosol profiling …".

Improved

Page 3, line 61: Correct 'POLIPHPON' by 'POLIPHON'.

Improved

Page 3, Figure 1: Correct 'Llidar' in the first box of the figure by ''LIdar.

Improved

Page 6, line 131: Correct 'particles density' by 'particle density'.

Improved

Page 10, Table 3: In the first column, add a space between 'Lanzhou' and '(SACOL)'.

Improved

Page 10, line 235: The wavelength of 532 nm is missing.

Improved

Page 13, line 296: Correct 'AERONT' by 'AERONET'.

Improved

Page 15, Table 6: For marine particles, the expressions for the conversion factors $c_{\mathrm{s,m,amb}}/4$ and $c_{\mathrm{v,m,amb}}/8$ in the 2nd column should be replaced by $c_{\mathrm{s,m,amb}}$ and $c_{\mathrm{v,m,amb}}$, for consistency with $s_{\mathrm{m,amb,j}}$ and $v_{\mathrm{m,amb,j}}$ in the 4th column that already appear corrected by '1/4' and 1/8', respectively. The same corresponding modifications for the Continental aerosol pollution.

Improved

Page 16, line 351: Remove the comma after 'conversion factors'.

Improved

Page 17, Table 7: Please, consider revising the number of decimals of the quantities. They could be rounded to 2 decimals in overall. The same for the rest of tables with values, when possible and consistent.

Improved

Pages 25-26, Tables 13-14: Please, include the SD values also in these tables as in Table 12. This will clarify the dispersion among the computed values of the conversion factors.

Improved

Page 29, Figure 9: In the caption, replace the comma after 'red dashed' by a semicolon.

Improved

Page 30, line 511: Replace 'significant' by 'significantly'.

Improved

Page 30, lines 539-540: The term 'at lower extinction' is twice repeated. Please, remove one.

Improved