Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-87

Anonymous Referee #1

Referee comment on "Technique for comparison of backscatter coefficients derived from in situ cloud probe measurements with concurrent airborne lidar" by Shawn Wendell Wagner and David James Delene, Atmos. Meas. Tech. Discuss.,
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The manuscript reports on a comparison between cloud Particle Size Distribution (PSD) and optical measurements of the backscatter coefficient. The former is provided by cloud probes (ECP), the latter from a short-range cloud Lidar (OID), both present onboard. Backscatter coefficients computed with Mie theory are compared to lidar-derived ones on four case studies, where 60 s flight segments, with each segment selected to represent a different meteorological condition (i.e. two liquid and two ice clouds), have been used.
Of the four case studies, one (on a warm cloud) show a satisfactory comparison, while the remaining three are less satisfactory, albeit the two backscattering determinations remain within the Three Sigma Rule for all of the time when warm cloud are considered, and for part of the time when cold clouds are considered.
The disagreement is more marked when changes in mean particle dimension and concentration (and maybe shape? The authors look at that as well) settles in. Surprisingly, in three out of the four case studies (those where the author excludes saturation problems in the OID data), the computed ECP backscatter result lower than the measured one. In fact, scattering from nonspherical particles can significantly differ from that of volume or surface equivalent spheres, and this is particularly true in backscattering, where the lack of positive interference effects of the waves propagating inside the particle generally leads to a depression of the backscattering for aspherical particles. Hence, the Mie theory should provide an upper limit to the backscattering from nonspherical particles. So, it is surprising how the Mie computation resulted systematically lower than the measured values. An inaccurate inversion of the OID data may play a role in it, albeit minor. In particular, the extinction used. Chen et al. (Appl. Opt, 2002) reported a variability of Lidar Ratio in cirrus clouds from 20 to 40 sr (at 532 nm). Given the extreme values of the backscattering coefficients for ice clouds, around 0.1 km-1 sr-1, this would deliver an attenuation changing, in the 10 meters path traversed by the OID signal, from 95 to 80%. This may be quoted, along with a more thoughtful description of the OID data inversion procedure, but does not explain the magnitude and sign of the mismatch. The authors honestly acknowledge an unaccounted source of systematic error, and investigate the effects of different methods to define an equivalent spherical particle (surface equivalence in the manuscript, fast circle in the supplementary material), possible biases in concentration, possible presence of more than one phase in clouds. That would dramatically change the computation of the backscattering for the cold cloud cases.
This analysis thus proposes possible causes for the mismatch but does not reach definitive conclusions. Furthermore, there is no discussion on the limits of applicability of the theory

of Mie to aspheric particulate matter. I believe this can usefully be added.
Overall the work is interesting, it provides regressions between backscattering (calculated and measured) and TWC in the clouds, and deserves to be published. However, the authors may consider expanding it according to the directions I have highlighted here, and more specifically, as reported below.
(132) "The backscatter coefficient is calculated…". This β has contribution both from molecules and particles. Given the very high particle β measured, the molecular contribution could be neglected but have to be mentioned.
(144) "…the primary error source is likely the inversion of the range-resolved Lidar signal to estimate extinction." This is probably true and cast its shadow on the following. Suppose a Lidar Ratio (LR) of some tens sr, given the highest β values reported in the study, the extinction coefficient ε=LR*β (by the way, why use σ instead of ε which is more common in the literature?) would be larger than 10 km^{-1} and the attenuation from even a distance as short as 10 meters could be significant, and could explain some of the mismatch between computed and measured β, reported afterward. The authors should dwell more on how do they invert their lidar signal, what are the hypothesis done on the LR they use, what is their - at least qualitative - impact on the uncertainties. As instance, are they using the same LR for liquid and ice clouds? Unfortunately, the quoted reference Lolli et al (2013) is of no help since it deals with the determination of colour ratio of rain droplets, explicitly neglecting extinction effects.
(259) "Backscatter efficiency values are calculated using MiePlot for diameters distributed log-normally between 1 μm and 30 mm." Not clear what "distributed lognormally" means here. Do you mean that the calculated efficiencies were calculated for radii equally spaced on a logarithmic scale from 1 μm to 30 mm?
(260) "Backscatter efficiencies are averaged for all particle diameters within each channel" Where they arithmetically averaged? Was an attempt made to choose the mean value of the radius in the bin so that it was perhaps more representative? For example, by weighing the average of the radii with an estimate of the concentration of the particles at those radii, which can be derived for example from the estimated slope of the PSD in that bin (the arithmetic average assumes that the distribution of particles in the bin is unform). Could this make things better?
(270) Figure 3 is quite interesting as it shows an increase of two orders of magnitude of the backscattering efficiencies for large particles, despite a relatively small change of the refractive index, from ice to water values. This was quite unexpected for me. I have taken the liberty of checking this result with one of the avatars of the BHMIE program which is at the core of the MiePlot package used in this work, and reproducing the same result. Still puzzled, I contacted Philip Laven (the author of the MiePlot package) who confirmed, with independent computation based on the Debye series approach, the correctness of the results of the paper. He explained that the 10th order rainbow is responsible for the increase in backscattering at 905 nm when the real part of the refractive index n = 1.3263 (the value chosen for water in the paper). The authors could underline the peculiarity of the factor 100 difference backscattered intensity at 905 nm between ice and water. In a sense, it is quite unfortunate that the choice of the 905 nm wavelength lead to such dramatic change in the backscattering from ice and liquid water, thus making the assumptions on the particulate phase very critical and impacting for the result. The reviewer thanks Philip Laven for the enlightening mail exchanges.
(390) Figure 10 lower panel is not sufficiently addressed in the text. There it appear two regimes in the TWC-OID backscattering regression. The authors should dwell more on that, and perhaps define two different regression lines.