Atmospheric Measurement Techniques Discussions

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Reply on RC1

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Author 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., https://doi.org/10.5194/amt-2022-87-AC1, 2022

The authors would like to express their appreciation for the level of detail of the review and effort of the referee. The comments are thoughtful and provide excellent insight into areas of the manuscript which can be improved. Each of the initial points made will be incorporated into a text revision: the reference to Chen et al. 2002 and the importance of attenuation over the 10 m OID signal path with more information on the OID inversion procedure, and more discussion on how Mie fundamentally applies to spherical particles rather than aspherical particles.

Replies to the line specific comments are given below in italic after repeating the comment:

(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.

(132) It is true that the molecular backscatter contribution to β is negligible in comparison to the particles. From Anderson et al., 2015: "The OID is not sensitive enough to measure molecular scattering that is commonly used to calibrate cloud lidar." Hence, it seems reasonable to add a statement near Eq. 1 similar to "While the backscatter coefficient includes scattering from both molecules and cloud particles, the OID is not sensitive enough to measure molecular only scattering (Ray and Anderson, 2015); therefore, it is assumed that all backscatter is from cloud particles."

(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.

(144) Attenuation does affect backscatter; however, since the OID uses a pulsed laser, close returns can be compared to returns from further away to assess if attenuation is a significant issue. This type of analysis indicates that attenuation affects the +7 case but not the other cases analyzed. We are working on how to address this point in more detail, which we willll incorporate into the revised manuscript.

(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?

(259) In this case, "distributed lognormally" is intended to mean that with progressively larger diameter particles, the intervals between diameters used in the calculation increases. Intervals between the smallest diameters used in the calculation (starting with 1 μm) begin at 0.0001 μm and increase to intervals of 3 μm at the largest diameter (30 mm). While this was described within the caption of Fig. 3, we will make it clearer within the text at line 259 by adding more details in the text.

(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?

(260) The backscatter efficiencies are arithmetically averaged. As evident from Figure 3, the averaged backscatter coefficient efficiency changes very little from one bin channel to the next. Weighting the averaged efficiency by how the particles are distributed within the channel would move the efficiency slightly (we would estimate 10 %) to smaller sizes in the case of ice, where the efficiency mostly decreases with increasing size and hence may increase the overall backscatter. The maximum percentage difference between scattering efficiency changes from one channel to the next is 17%; hence, 10% of this would be 1.7%, which is small compared to the observed difference between ECP and OID backscattering.

(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 conformed, 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.

(270) The authors appreciate the extensive work which went into the verification of the results in Figure 3. The differences described were of some interest, and it is appreciated

that there is an explanation. It is agreed that this is something which could be further acknowledged and explained within the Methodology section rather than waiting to be simply acknowledged within the Discussion section. This explanation will be added in paper revision.

(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.

(390) The authors agree that the Fig. 10 could be addressed further, particularly the lower panel. There does seem to be two difference regimes in the OID backscatter coefficient regression: one for the cold cases (primarily ice), and one for the warm cases (primarily water). This would seem to be an important distinction as this is clearly not accounted for in the ECP backscatter coefficient shown in the top panel. We will add additional regression lines for water and ice to Fig. 10, as well as discussion within the text.