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Full title: Technique for comparison of backscatter coefficients derived from in-situ cloud prove measurements with concurrent airborne Lidar
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The paper evaluates the backscattering coefficients derived from both cloud probe measurements and lidar deployed in an aircraft for both liquid and ice cloud cases. The authors describe the measurement instruments and their data processing methods in detail. Also, a rich volume of discussions in the uncertainty of derived backscatter coefficients is given in the manuscript. These parts are quite well-written. However, I am concerned by a crucial assumption of ice crystals to be ice spheres used for entire analyses of ice cloud cases. Also, some ambiguity in the variables in equations is found. Please find my comments below. The topic presented in this paper is suitable for Atmospheric Measurement Techniques. I recommend Major Revisions to reconsider the manuscript for publication.

Major comments

<u>*Backscatter coefficient derivation from ECP data*</u>

First of all, the assumption of ice sphere for the derivation of the backscatter coefficients from ECP data would cause a significant systematic bias. As confirmed by many aircraft observations of ice crystals, a majority of ice crystals have nonspherical shapes. The scattering properties of nonspherical ice crystals differ significantly from those of spherical ice. Although the authors claim that the uncertainty of the backscattering coefficients associated with the spherical ice assumption is much less than the uncertainty from particle size distribution (PSD) measurements (*Line 234–236*), these uncertainties would involve systematic biases, which will be carried over computing the backscattering coefficients from the PSD measurements.

To improve the validity of the present analysis for ice clouds, I suggest the authors add the following analysis to the ice cloud cases. To convert the extinction coefficients from backscattering coefficients through the inversion process, we use the lidar ratio (*S*; extinction-to-backscatter ratio) of ice clouds that is empirically determined for each hydrometeour, and the latest value of the lidar ratio for ice clouds is, for example S = 32 sr at 532 nm (Holz et al., 2016). The lidar ratio at 905 nm could differ slightly from the one at 532 nm due to a slight difference of the real part of the ice refractive index between these wavelengths, but it should be quite consistent. The authors are strongly encouraged to perform the additional ECP data analysis with a lidar ratio of 32 sr for ice cloud cases.

Also, I noticed that the backscattering efficiency is introduced in Line 238 without a definition. Please clearly define the backscatter efficiency in the corresponding sentence. I believe that the authors defined the backscattering efficiency of a single particle as

$$Q_{back} = \frac{Q_{ext}\omega P_{11}(\pi)}{4\pi}, \tag{R1}$$

where Q_{ext} is the extinction efficiency; ω is the single-scattering albedo; and $P_{11}(\pi)$ is the scattering phase function at 180° degree. I would like to clarify if the authors include a denominator of 4π. If the above is correct, the lidar ratio can be described as

$$S = \frac{Q_{ext}}{Q_{back}}. \tag{R2}$$

Otherwise, I am wondering if the above definition differs from what is actually defined because the paper states that the fourth Stokes component V is the focus of the study (Line 97).

Backscatter coefficient derivation from OID data

As seen in Eq. (1) in the manuscript, the lidar signals from a certain location of ice clouds relative to the location of the aircraft can be attenuated by ice crystals in between the two locations. Therefore, the lidar signals need a correction with the two-way transmissivity to obtain the backscatter coefficient. The authors cite Lolli et al. (2013) for the extinction coefficient inversion for the present analysis, this paper is for rain droplets and the predefined lidar ratio for rain droplet (i.e., 50 sr in Lolli et al., 2013) may be inaccurate for small liquid droplets and ice clouds. Please add a few sentences describing how the extinction efficiency is derived for both liquid and ice cloud cases. In particular, what lidar ratios are used to estimate the extinction cross-section through the inversion of lidar measurements for each ice and liquid cloud case?

Minor comments

1. *"Hulst (1981)"* should be *"van de Hulst (1981)"* throughout the manuscript.

2. Lines 96–97 *"The 905 nm bean enables measurement of the fourth Stokes parameter (V) (Liou and Yang, 2016; Hulst, 1981) and is the focus of the study, …"* I got an impression that the lidar instrument measures only the fourth component of the Stokes vector (*V*) from the manuscript. However, it actually measures the first component of the Stokes vector (*I*) in addition to the fourth component according to Ray and Anderson (2015), doesn't it? Please clarify it.

3. Lines 155-156 "*Images are produced when at least one array element is "shadowed" (i.e., reduced in intensity by 50% or more).*": Is there any reference that discusses the accuracy of estimated particle area from 2D-S with this approach?

4. Lines 187–190, Eq. (2): Use the italic font for scaler variables in the main text to be consistent with Eq. (2).

5. Line 226 *"geometric"* should be "geometric optics".

6. Line 234-236 *"While Mie theory …"*: Cairo et al. (2011) states in Page 561 that *"Generally speaking, aspherical scatterers depress the forward and back- ward scattering and enhance the side scattering with respect to surface equivalent spheres, so an overestimation of the backward scattering may be expected when using Mie codes. An educated guess of such overestimation can be provided by looking at studies comparing the phase function of aspherical vs spherical scatterers, which suggest an average overestimation of the Mie backscattering coefficient by a factor 2, which may possibly get as large as a factor 4 or more, depending on particle sizes and shapes (Mishchenko et al., 1996)."* Please revise the corresponding sentence to be consistent with the statement by Cairo et al. (2011).

7. Lines 246-247 "an equivalent sphere": This should be clearly stated as "a projected area equivalent sphere" in order to avoid confusion with a volume equivalent sphere. The

backscattering coefficient is proportional to the cross-sectional area of a particle for large size parameters (i.e., $Q_{ext} = \sim 2$), so that the use of projected area equivalent spherical radius is relevant for both liquid and ice particles in the present analysis.

8. Lines 267–268: This statement is inconsistent with Lolli et al. (2013) that use a predefined extinction-to-backscatter ratio (or the lidar ratio) to estimate the extinction coefficients from lidar signals. Thus, the backscattering efficiency is necessary for to interpret OID data (as clearly indicated in Eq. 1).

9. Figure 3 caption: 0.0001 μm should be 0.0001 mm. Also, 3 μm should be 3 mm.

10. Figure 5a: What lidar ratio is used to derive the extinction coefficient for the OID analysis for this case? As Lolli et al., (2013) use a lidar ratio of 50 sr for rain drop that is significantly larger than those of cloud liquid droplets (~20 sr), the two-way transmissivity could be overestimated, so that OID derived backscattering coefficient might be underestimated. The authors are encouraged to clarify this.

11. Line 378 *"..., which indicates an unaccounted source of systematic error."*: I think this may be due to the backscattering efficiency bias associated with an ice sphere assumption. In Lines 295-296 the manuscript says *"Eq (7) does not include systematic errors (e.g., uncertainty in backscatter efficiency)"*. I suggest the authors to mention that one of unaccounted errors would be a systematic bias in backscatter efficiency.

Reference

Holz, R. E., and Coauthors (2016). Resolving ice cloud optical thickness biases between CALIOP and MODIS using infrared retrievals. *Atmospheric Chemistry and Physics*, *16*(8), 5075-5090.