Amt-2022-87 Authors' Initial Response to Anonymous Referee #3 14 July 2022

The authors would like to thank referee 3 for taking the time to write an extensive and well composed review of the manuscript which provides thought provoking suggestions and questions. Replies to the line specific comments are given below in italic after repeating the reviewer's comment.

Major Comments:

Backscatter coefficient derivation from ECP data

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

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

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

$$\boldsymbol{S = \frac{Q_{ext}}{Q_{back}},} \qquad \textit{(R2)}$$

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).

It is true irregular shaped ice crystals scatter differently than spherical ice crystals, and clouds have irregular shaped ice crystal as shown in the inserts of Fig. 4. There are two size parameters (radius/diameter) that are important: the radius in Eq. 4 of the paper for deriving backscatter coefficients and the diameter of the scattering efficiency at 180 degrees (Fig. 3).

Typically these are the same; however, it is not apparent that they would be for non-spherical particles.

First, note that the uncertainties for determining particle diameter from 2-dimensional images are large (see the uncertainty analysis within the paper). The main manuscript uses an area-equivalent diameter, and the supplemental material uses a fast-circle diameter. The area-equivalent diameter is typically more acceptable since it is the equivalent diameter of a sphere which has the same area as that observed within the 2-dimentational probe images. Hence, when converted to a radius, squared, and multiplied by π in Eq. 4, it provides the area of the imaged particle. There could be biases that are important to acknowledge in obtaining the area of a particle from a 2-dimentional image if the particles are not randomly oriented either in the probe images or in the Lidar backscatter volume. We will add an additional note on this potential bias in the manuscript around line 295.

There is also the diameter in the backscatter efficiency equation (Figure 3), which is currently the same as the diameter used in Eq. 4; however, since non-spherical and spherical particles do not scatter the same, a different diameter may be better to use as the equivalent backscatter diameter than the area equivalent diameter for obtaining the backscatter efficiency. Additionally, it is not clear that the backscatter coefficient in Eq. 4 should be related to the area equivalent diameter since the backscatter is dependent on surface waves, and the interference of the surface waves (see comment/discussion of Referee 1).

In conclusion, this manuscript uses the most accept diameter for ice particles, and the supplement presents the fast-circle diameter for comparison, which we believe is the best that can be done. We will add a sentence or two related to this topic in the discussion section of the paper. While it is beyond the scope of this article, future work could use the ECP/OID data set to determine the diameter that provides the best agreement with the Lidar measurement for ice clouds.

The authors apologize as it was believed to be clear in the manuscript that a lidar ratio is not assumed for the calculations. The OID measures attenuated backscatter at different ranges and then fits a curve to the exponential function in Eq. 1. Since the lidar only samples along the wing of the aircraft (see Fig. 2), the cloud is homogenous enabling the curve fit. The attenuation is alpha, and the true, unattenuated backscatter is the y-intercept of the curve from the data fit. The lidar ratio is then derived from these two measured values. Therefore, the lidar ratio changes depending on the cloud conditions, and no single value is applied specifically for ice cases. Hence, the aircraft lidar processing is different than satellite based lidar processing where clouds sampled are not homogenous along the lidar beam at different range gates. We will include this information in the manuscript beginning at line 136.

This definition of the backscattering efficiency is correct, and a denominator of 4π is included. This equation will be added to the updated manuscript at line 245 as Eq. 5.

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?

The lidar ratio is derived by curve fitting the OID measured attenuation and the unattenuated backscatter, allowing for the lidar ratio to vary with the changing cloud conditions. As the sampling volume is relatively small, limited to only 10 m from the aircraft, attenuation by ice crystals is very limited, so a correction with the two-way transmissivity is not necessary. The authors apologize that this is not clear in the text, and this information will be added in the following manuscript beginning at line 136.

Minor comments:

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

   This is an error by the authors and will be corrected in the following manuscript version.

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.

   It is correct that the OID measures both Stokes vector (I) and (V), however (I) was neglected to be mentioned as the results of (V) are the focus of this study. For clarity line 96 will be updated using "With the 905 nm the OID is able to measure both the first Stokes parameter (I) as well as the fourth Stokes parameter (V) (Liou and Yang, 2016; van de Hulst, 1981). The fourth Stokes parameter (V) is the focus of this study, and the 1550 nm wavelength channel is not used.

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?

   McFarquhar Et al. (2017) discusses this point on page 15, and Figure 11-7 using a CIP probe. Their results indicate that setting a threshold of 50 % results in derived particle diameters 100 µm less than those derived using a 70 % threshold, with this difference increasing with smaller sized particles. This citation will be added at line 156.

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

   This is an error by the authors and will be corrected in the following manuscript version.

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

   This is an error by the authors and will be corrected in the following manuscript version.

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).

   To be more consistent with Cairo et al. (2011), line 234 will be updated in the following manuscript to "As Mie theory strictly applies to spherical particles, previous work has found that aspherical particles tend to have enhanced side scattering compared to spherical scatterers, resulting in Mie codes producing larger backscatter coefficients by an average of 2 or more (Cairo et al., 2011). However, this uncertainty is less than those associated with measurements of cloud particle sizes.

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., $Qext.$ = ~2), so that the use of projected area equivalent spherical radius is relevant for both liquid and ice particles in the present analysis.

   The authors respectfully disagree that this should be considered as a projected area equivalent sphere, as the surface waves mentioned in line 265 prevent Q_{back} from reaching an asymptote and becoming proportional to the cross-sectional area of the particle. Instead, a more appropriate labeling in line 246 would be "a backscatter equivalent sphere".

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).

   In Eq. 1, an assumed lidar ratio is not necessary. The OID measures attenuated backscatter at various ranges and then fits this curve to the exponential function in Eq 1. The *attenuation is alpha, and the true unattenuated backscatter is the y-intercept of the curve. The lidar ratio is then derived from these two measured values. Thus, while the backscatter efficiency is necessary to calculate the ECP backscatter coefficients, it is not necessary for understanding the OID data.*

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

   The values 0.0001 μm and 3 μm are intentional as they are to indicate the intervals between diameters used for the individual backscatter efficiency calculations, not the intervals in diameters between the average backscatter efficiency calculations. This is not clear in the manuscript. For the following manuscript, line 272 could be updated to "Intervals between diameters used for the individual backscatter efficiency calculations range from 0.0001 μm to 3 μm."

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.

    As mentioned previously in comment 8, a lidar ratio is not assumed but rather repeatedly derived by using the measured attenuation and the true unattenuated backscatter in a curve fit. Thus, the lidar ratio changes to match the environment which encounters little attenuation due to the relatively small sampling volume.

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.

    While a backscattering efficiency bias likely has an effect to some degree, as mentioned in comment 6 and in Cairo et al. (2011) calculated backscatter coefficients using Mie theory code on aspherical particles should be a factor of at least 2 higher than those that are measured. Thus, the backscatter efficiency bias cannot fully explain why the OID backscatter coefficients are higher than those calculated from ECP data. However, this bias is still important to acknowledge and to emphasize this face line 295 can be updated in the following manuscript to "The third standard deviation is used as a threshold in accordance with the Three Sigma Rule (Pukelsheim, 1994) since the OID is in development and Eq. (7) does not include systematic errors (e.g. systematic bias in the backscatter efficiency due to spherical particle assumptions)."

References

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.

McFarquhar, G. M., and Coauthors, 2017: Processing of Ice Cloud In Situ Data Collected by Bulk Water, Scattering, and Imaging Probes: Fundamentals, Uncertainties, and Efforts toward Consistency. *Meteorological Monographs*, **58**, 11.1-11.33, https://doi.org/10.1175/AMSMONOGRAPHS-D-16-0007.1.