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Atmospheric Measurement Techniques Discussions

Interactive comment on "Intercomparison study and optical asphericity measurements of small ice particles in the CERN CLOUD experiment" *by* Leonid Nichman et al.

Anonymous Referee #1

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General Comments:

The manuscript reports results of cloud chamber experiments in the Cosmics-Leaving-OUtdoor-Droplets (CLOUD) chamber at European Organisation for Nuclear Research (CERN). Three instruments are operated in conjunction with the chamber: Particle Phase Discriminator mark 2 (PPD-2K, Karlsruhe edition) were compared with Cloud and Aerosol Spectrometer with Polarisation (CASPOL) measurements and images captured by the 3View Cloud Particle Imager (3V-CPI). Averaged path light scattering properties of the simulated ice clouds were measured using the Scattering-Intensity-Measurements-for-the-Optical-detectioN-of-icE (SIMONE) and single particle scattering properties were measured by the CASPOL.

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Discussion paper

While the manuscript makes a worthy attempt at evaluating, comparing and contrasting the measurements from the instruments, it falls woefully short in its present form. There are many unsubstantiated claims and problems that need to be corrected (if possible) before this paper should be moved from AMTD to AMT.

1. There is constant reference to sub- and super-saturated conditions (presumably w.r.t. ice) in the chamber, yet no RHice measurements are shown, and there is no mention if instrumentation was available to make the measurement. The lack of RH measurements creates an uncertainty in assertions that the air immediately surrounding the drops is subsaturated, saturated or supersaturated w.r.t. ice (or water for that matter).

2. T and P are measured, but inferring that all of the water drops become ice is not supported by any measurements, but only inferred by the instruments that are being evaluated for their ability to discriminate ice and water drops. The measurements (Fig. 1) suggest rapid nucleation by CCN and formation and growth of particles following a rapid expansion in the chamber. It is assumed that these are water drops and that the drops immediately freeze. However, the measured temperature only drops to about -35.5 C, not as low as the homogeneous freezing temperature. Also, the depolarization ratio reaches a modest maximum of 0.25. There is no way to confirm if all of the water drops froze, or not. I would like to see a similar time series for an expansion conducted at the colder temperature (below – 40 C) where homogeneous freezing is assured.

3. There is no standard for determining whether spherical particles are water drops or ice. The PPD-2K is assumed to be capable of distinguishing spherical water from ice based on individual particle diffraction fringes, but as shown in Fig. 7, the comparison between the scattering pattern in A (water drop) and D (sublimated ice), it is not possible to unambiguously determine spherical ice from a water drop.

4. One of the conclusions stated in the Abstract is that bulk averaged path depolarisation measurements of these clouds showed higher correlation to single particle mea-

surements at high concentration and small diameters of cloud particles. Yet, measurements of small (in this case < 7 microns) are only made by one instrument (CASPOL), and there is no way to determine why there is a (very poor) correlation (as shown in Fig. 8) and how to determine the physical significance. The statement in Section 3.4 lines 33 – 34 that the correlation in Fig. 8 is surprisingly reasonable leaves this reviewer bewildered. It looks to me like the correlation is terrible. The (max) R2 value of 0.35 in regions with small particles at high concentrations (where there is no way of actually knowing the shape of the particles) is nothing to brag about, and R2 = 0.01 in regions with low particle concentration is pitiful.

5. The 3V-CPI is the only instrument that provides actual images of these particles. Even though the CPI pixel size resolution is not optimum for resolving the shape of these small particles, the manuscript needs to show more images of particles. Specifically, show images of the water drops prior to freezing. Also, there is mention of columnar shapes identified by the PPD-2K, but no CPI images. Please show the CPI images that correspond with the PPD-2K derived columns. I don't understand the CPI measurements in Fig. 6. How are the gray squares calculated? Why are there multiple overlapping measurements at the same point in time? If each point represents an individual image analysis, then why weren't the other single particle measurements processed in this manner. Why are there not more CPI measurements in Figs. 6b,c?

6. There is no description of how the instruments were operated. Were the instruments installed in the cloud chamber? Was cloud air exhausted through the sample volume of the probes? Were the probes aspirated? Etc. There also needs to be more description of how the instruments were operated and how the measurements were processed. It is not straightforward how to measure the sample volume of instruments used to measure particle size distributions from a cloud chamber. How were the size distributions computed? The agreement in the size distributions shown in Fig. 2 is poor, often differing by an order of magnitude. How does this affect the results reported in the paper?

Overall, I am not sure what the takeaway messages are from this paper. None of the instruments tested are capable of unambiguously distinguishing ice from water. "Complexity" is discussed but never really defined, except to hypothesize that it is a "frost" layer. Based on the PPD-2K diffraction images, the instrument can show the difference between a spherical particle and a particle that is irregular in shape or has some surface "complexity", but there is no convincing explanation of how to apply this information quantitatively. The k value is mentioned, and in other papers there are examples of diffraction patterns from analogs and other shapes, but there is no comparison with high-resolution images of actual ice particles. The images from the PHIPS-HALO instrument in Schnaiter et al. (2016) do not have adequate resolution to provide useful information, except to distinguish columns from quasi-round particles. After looking at the diffraction patterns in Schnaiter et al. (2016) I cannot tell the difference between a distorted (analog) scattering pattern and one in this manuscript that is labeled as having surface complexity. There also appears to be no additional information on how well the diffraction patterns correlate with actual high-resolution images of ice particles in Vochezer et al. (2016). Ideally, an instrument capable of imaging particles with much higher optical resolution than the CPI should be used to compare with the PPD-2K. Could ice particles be captured on a cooled slide, placed in a cold box and photographed under a microscope? Even though the CPI only has adequate resolution to distinguish round, quasi-round and columnar shapes for particles $> \sim 30$ microns, I would still like to see a comparison between CPI images and PPD-2K diffraction patterns of the various particle shapes that are mentioned in the manuscript.

The SID family of instruments (including the PPD-2K) provide interesting and potentially useful measurements, but the quantitative utilization of these measurements in mixed-phase and in cirrus clouds with a combination of growing and sublimating particles is not clear. Measurements have shown that a substantial fraction of false irregulars are seen in all-water clouds (i.e., Johnson et al. 2014 JAS). Yes, certain pristine shapes can be identified: perfect spheres, column shapes and possibly hex shapes, but the large majority of ice particles in cirrus are irregular. How are these particles quantified?

The ability of the CASPOL to quantitatively distinguish water and ice is not demonstrated at all. The results vary with both particle size and concentration, leaving one to wonder what it is really measuring. There is good qualitative agreement with the PPD-2K in estimating asphericity in Fig. 6a, but no agreement in Figs. 6b and 6c. What is the explanation for this?

Specific Comments:

P. 2 Lines 19 – 20: I disagree. Shape is used more often than scattering intensity in mixed-phase clouds, and arguably more reliably. In many cases in mixed-phase (i.e., water saturated) clouds, ice particles rapidly grow to sizes where they can be distinguished from water drops using CPI imagery (see Lawson et al. 2015 – JAS).

P. 2 Lines 32 – 32: The measurement of particles smaller than 50 micron using the FSSP were contaminated with shattering. Delete this reference.

P. 4 Lines 1 – 3: "We then use the asphericity to determine the ice fraction in a cloud by prescribing an aspherical shape for all the ice particles, and hence assume that ice fraction is equivalent to an aspherical fraction." As discussed above, using the measurements presented in this manuscript, there is no way to unambiguously determine if asphericity explicitly distinguishes ice particles from water drops. This statement needs to be modified or deleted and then explained later in the text after it is understood that using asphericity is an estimate of ice fraction that is not well quantified under all conditions.

P. 4 Lines 10 – 14: This statement appears to be contradictory. If LWC is independent of updraft velocity, but stronger updrafts produce a higher concentration of smaller drops, which then freeze, how is IWC increased in stronger updrafts? This appears to violate conservation of mass.

P. 5 Line 6: 100 per cc is not necessarily a low concentration. Simulations now show that coincidence occurs at this concentration with the CASPOL and multiple scattering will occur in ensemble measurements. Please qualify this statement (and not by using 1980's references to the FSSP).

P. 5 Lines 26 – 30: Please show some quantitative evidence that 10-5 asphericity threshold actually applies to ice/water discrimination. Otherwise, please state that this is a subjective value based on visual analysis of the scattering pattern. Referencing Vochezer et al. (2016) is not sufficient.

P. 6 Lines 1: Detection of a bulk cloud phase is meaningless unless the cloud is all-water (T > 0 C), or known to be all-ice (i.e, colder than – 40 C). There is no quantitative information published (yet) on bulk measurements of the ice fraction in mixed-phase.

P. 6 3V-CPI: The references in this section are terrible, misleading and in one case unavailable. The 2D-S portion of the 3V-CPI should be referenced by Lawson et al. (2006) – JTech. The CPI portion of the 3V-CPI should be referenced by Lawson et al. (2001). Lawson et al. (2003) should be deleted. The Heymsfield et al. (2010) reference does not show particle habit classification schemes. This reference should be replaced by, for example, Lawson et al. (2006) – JAMC; Um and McFarquhar (2009) – QJRMS; Lindqvist et al. (2012) – JGR

Section 3.1.1: As explained above, there are way too many assumptions about what is happening during the first rapid expansion and for a few seconds or minutes afterward. How do we know that all of the drops froze instantaneously? Could there be a mixed-phase cloud and Bergeron process occurring after the rapid expansion?

P. 8 Line 16: How do you know there was no coincidence?

P. 12 Lines 1 – 9: There are several assumptions and generalizations in these lines that need to be deleted based on previous arguments in this review.

Conclusions: This also needs to be re-written to tone down all of the claims that are not substantiated. BTW – the measurements presented in this manuscript extend out to particle sizes of about 30 microns. On line 26 and another place in the manuscript the claim is that the results are valid out to 60 microns. Where does this come from?

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