Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-297-AC2, 2021

Atmospheric Measurement Techniques Discussions

Open Access

AMTD

Interactive comment

Interactive comment on "PHIPS-HALO: the airborne particle habit imaging and polar scattering probe – Part 3: Single Particle Phase Discrimination and Particle Size Distribution based on Angular Scattering Function" *by* Fritz Waitz et al.

Fritz Waitz et al.

fritz.waitz@kit.edu

Received and published: 18 January 2021

We thank the Greg McFarquhar for his helpful comments. These comments helped to substantially improve the manuscript. Below we give detailed answers to the individual reviewer comments in *cursive*.

This study uses data collected by the PHIPS-Halo during two field campaigns (ACLOUD and SOCRATES) to develop a method to determine the phase of individual cloud hydrometeors from the light scattering measurements made by the probe. The algorithm takes advantage of differences in features between the angular scattering of spherical and aspherical particles to show that it can be determined with a 98% accuracy whether the particles are liquid or ice (i.e., spherical or aspherical). They also present a method for deriving the particle size distributions from the measured data. Information about particle phases is desperately needed because mixed-phase clouds are still not well understood. Further, the full potential of the PHIPS-HALO probe has yet to be realized because both particle size distributions and particle phase distributions have not been routinely been made available in a short time period after the conduct of field campaigns. As the material in this paper works to overcome both of these shortcomings, it should be published as soon as possible and represents a good contribution to the refereed literature. The paper is well written and technically sound so does not require a lot of revisions in that respect. Nevertheless, I think there are a couple of aspects that should be better explained in the paper so that the limitations, as well as the strengths, of the PHIPS-HALO for providing size and habit information are well outlined.

We thank the reviewer for this encouraging general comment. Below we have addressed the proposed revisions.

My major critique of the paper is that I think more information about the statistical representativeness of the data that are available from the PHIPS HALO probe should be included. The sample volume and/or sample area of the PHIPS-HALO probe should be explicitly stated. How does that compare against the sample volume/area from the commonly used. How does that affect the averaging time over which representative particle size distributions and particle phase distributions are available? For example, McFarquhar et al. (2007) calculate the required time that particle size distributions would need to be averaged over in order to obtain statistically significant particle size distributions that they defined to mean 100 particles in each size bin (so that there was a 10% uncertainty assuming the statistical uncertainty was proportional to the square root of the number of counts in each size bin). Figure 9 defines the sensitive area of the PHIPS-HALO as ranging from 0.002 $cm2$ to about 0.01 $cm2$ depending on the size of the particle. Assuming a roughly 150 m/2 air speed, this would give sample volumes ranging from 30 to 150 $cm3$ per second, or about 0.15 liter/second. This would seem to be quite a bit less than that of the 2DS/2DC class of probes. This does not negate the benefits of the PHIPS-HALO probe, but rather would seem to suggest that the more detailed phase/shape information available from the PHIPS-HALO probe has synergy with the more frequent data available from the optical array type probes that are better suited for deriving the fine resolution structure of clouds. This point, including explicitly comparing the sampled areas/volumes and numbers of particles between probes should be explicitly shown in the paper, and also mentioned in the abstract.

We recognise the concern of the reviewer and have included more information about the statistical representativeness of the PHIPS probe. We would like to remind that PHIPS as a single-particle probe needs to have significantly smaller sampling volume compared to shadow imagers to ensure that only one particle occupies the sampling volume when measurements take place. However, the sampling area of PHIPS does not significantly differ from other single-particle probes. Of course, single-particle instruments have the disadvantage of a smaller sampling volume compared to shadow imagers, which will affect the statistical representativeness of the measurements. Therefore, we added a section (4.4) discussing exemplary sample volume values and required averaging times, including a comparison with the common shadow imagers. Also, statistical uncertainty bars were added to the PSD figures for the three case studies in section 5.

Detailed Comments Page 1, line 6: evaluated would be a better word than validated.

The phrasing was adjusted accordingly.

Page 2, line 13: “a large sampling statistics is required” reads awkwardly and should be rephrased.

The phrasing was adjusted accordingly.

Page 3, line 15: the plural of aircraft is aircraft.

The phrasing was adjusted accordingly.

Page 4, line 7: Suggest adding Um et al. 2011 ACP to the list of references as they considered scattering functions of several models of quasi-spherical ice crystals

Um et al., 2011 was added to the list of references.

Page 4, Figure 2: Can you state what are the maximum dimensions of the two ice crystals that are considered in the figure?

The diameter of the two exemplary particles used in the figure are 119.6 μm for the droplet (blue) and 165.8 μm for the ice particle (red). This was added to the caption of the figure.

Page 7, line 23: It should be noted and discussed why there are a lot of differences in the nature of the distributions between the observed and modeled particles in Figures 4 and 5. Does this suggest that there are some limitations in how well the theoretical models are representing the actual observed particles?

We agree with the reviewer, that a detailed comparison and discussion of the feature-parameter-distribution-plots (Fig. 4 and Fig. 5) is missing. The following discussion was added in section 3.3:
“The plots show that the distribution of the four aforementioned feature parameters are clearly distinct for droplets and ice and thus represent features that can be used to discriminate droplets from ice. Further, it can be seen that these normalized occurrences (f_i) are normally distributed. The distributions of the four feature parameters based on the measurements (Fig. 6)

show a similar trend to the simulations (Fig. 5). The width of the distributions of feature parameters for measurements is much broader compared to the simulations. The main reason for this is the single-orientation of the measured crystals compared to the orientation-averaging that was used in the simulations. Orientation-averaging tends to smooth out features in the ASFs and thus cause more narrow feature parameters. It should be also noted that the theoretical computations are for idealised crystals, whereas it can be assumed that individual atmospheric ice crystals can have ASFs that deviate from idealized shapes. Nevertheless, the mean values of the distributions agree very well. The only exception to this is the mean value of the distribution of droplets for f1, which is shifted slightly to larger values compared to the simulations. This is to be expected because the "Mie-comparison-feature" f_1 is based on the relative difference between the measured and calculated ASF. This difference is much smaller for simulated particles as discussed in 3.1.1."

Page 9, line 3. This may be a stylistic thing, but when I see 41.000 I think there are 41 particles. I think the authors mean 41,000. Unless this convention is demanded by ACP, I would use a comma rather than a period.

The phrasing was adjusted accordingly throughout the whole document.

Page 9, line 3: Related to my major point above, I think it would be very interesting to compare the number of particles that were measured by the 2DC/2DS for the same periods during these field projects. That would help clarify information about the statistical representativeness of the data.

We agree, in general, on the importance of a comparison of the PHIPS and 2DC/2DS probes, including the discussion about sampling rates and statistical representativeness. This discussion takes place later in the text (see answer to the major comment above). However, at this particular location *in the text, a comparison with the total number of measured particles by the 2DC/2DS would not be representative since we have restricted our calibration dataset to carefully selected particles with available stereo-images. Only a sub-set of triggered particles have a stereo-image since the imaging rate of the camera is restricted. Additionally, only images that show clearly distinctive particles are used, whereas images that show cut-off, out-of-focus or multiple particles, are excluded. Furthermore, the calibration of the phase discrimination algorithm is based on images of individual particles, hence, for this particular application, the sampling rate is not relevant.*

Page 11, Figure 6. Was any effort made to go back and look at the particles that were misidentified to determine why they were misidentified? I agree that 98% is outstanding (and better than classifications that are based on other probe data), but it would still be interesting to know why the discrepancy for just these few particles. Was there any chance that the manual identification of these particles was incorrect?

All misclassified particles (in total 289) were investigated individually and the manual classification was double-checked. An overview of examples of misclassified particles and (possible) reasons for their misclassification are given in the SI (S4).

Page 13, Figure 8. Most probes that measure small particles have smaller sensitive areas for smaller particles than larger particles (e.g., see Figure 9 in this paper). But, as I understand it the vertical axis here is number of particles rather than some measure of concentration per bin or number distribution function. It would be informative to include another plot that shows the calculated number distribution function since that is a physically meaningful quantity, especially since the caption reads that this is a particle size distribution (PSD).

The PSD distribution in Fig. 8 is showing the comparison (or rather agreement) of the PSD based on the ASF and the images for a sub-sample of *particles with stereo-images. Therefore, a number concentration would not be representative for the cloud and we give the concentrations in arbitrary units.*

Page 18, Figure 11. The caption should specifically state the averaging period for which each of the plotted points corresponds to.

Each point corresponds to an averaging period of 30 seconds. This was added in the caption accordingly.

Page 22, Line 10. To make this study more accessible, it would be nice to have the codes used available on github or some other code repository.

We agree with the reviewer. The current version of the code can be accessed via https://doi.org/10.5281/zenodo.4321316.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-297, 2020.