<u>Point-by-point response to the second referee (Colin Gurganus)</u>

The authors would like to thank the reviewer for his valuable comments and useful recommendations.

RC: Section 5 should be strengthened with the addition of significantly more laboratory data to verify the operation of the new instrument. I believe that this manuscript would also be enhanced by combining the results from field deployments.

AC: In the paper submission phase we expanded the characterization section as requested by one of the reviewers. Adding atmospheric measurements or further characterization data is the subject of the part II paper and not relevant in the part I paper. However, to eliminate any concerns about the validity of operation, we would like to point to the published work by Schnaiter et .al. (2016) where we presented results from both parts of PHIPS-HALO (Scattering and imaging) and compared them with other instruments.

<u>Section 1: Introduction</u>

RC: There is a concerted effort in this section to describe a host of other aircraft deployed particle probes. This divergence is distracting because the PHIPS instrument is fundamentally different from forward scattering probes (FSSP, CDP, FCDP, SID), Optical array probes (2DC, 2DS, CIP, HVPS, etc.), and holographic imagers (HOLODEC). A discussion of these other probes is only relevant if a data inter comparison is to be presented in this article.

AC: We did not compare these instruments to PHIPS. They were only mentioned in the introduction as a kind of a survey on similar probes. It was also a demand by one of the reviewers in the paper submission phase

RC: The PHIPS instrument is most similar to a combination of a Polar Nephelometer and a Cloud Particle Imager (CPI) so comparisons should be made to these instruments only.

AC: This point is considered. Please check the modified text in sections 1 and end of section 3 and the added tables (Table 1 and Table 2)

RC: The PHIPS presents unique advantages by combining both of these instruments in one package, but the limitations compared to individual instruments should also be highlighted. The foremost of these is the very slow sampling rate for the imager, which may not be problematic in a cloud

chamber but might limit sampling statistics for high speed aircraft sampling. Issues like these would best be addressed with inter comparison of aircraft flight data for co-located CPI, Polar-Nephelometer and PHIPS instruments.

AC: The main function of PHIPS is the correlation between optical and microphysical properties of cloud particles. The limitation of sample statistics due to slow rate would be a secondary issue and, as the referee stated, would best be addressed with inter comparison of real data with other instruments. This is however the subject of the part II paper and not relevant in the part I paper.

Section 2: Basic Instrument Concept

RC: This section is fine, but personally I find a schematic figure describing the features PHIPS (like Figure 1 of Abdelmonem et al. 2011) much easier to understand than the current Figure 1 in this manuscript.

AC: The authors decided to keep the current figure since the readers can refer to the cited paper (Abdelmonem et. al. 2011)

Section 3: Basic Instrument Concept

RC: In the Imager section (3.1.2) the magnification of the camera is stated to be variable from 1.4-9.0, I would like to know what typical operating resolutions are. How big is the Field of View for a dual image of particles at various magnifications?

AC: A typical operating "optical" resolution is ~5.3µm (at 4X magnification) with a field of view ~ 2.19 x 1.65 mm. The corresponding particle size range is from ~10 µm to 1.5 mm. The imaging system is calibrated with a calibration slide before each campaign and after any change of the telescope magnification

Section 5: Modeling of the Instrument Response and Detection Range for the Scattering Optics

RC: Omission of any particle images is somewhat troubling. I am disappointed that no laboratory data is presented here for the imaging system. It should be fairly trivial to show images of the glass beads passing through the sample volume (as was done in Figure 4 of the 2011 PHIPS article). Images of the monodispersed particles would help to demonstrate the focus and sizing of the new instrument. Presumably the researchers have already preformed a sizing verification study with the imaging system, so including results from this work should be straightforward and not require

additional tests. It would also be helpful to show results for different magnifications, since that is an option for this instrument.

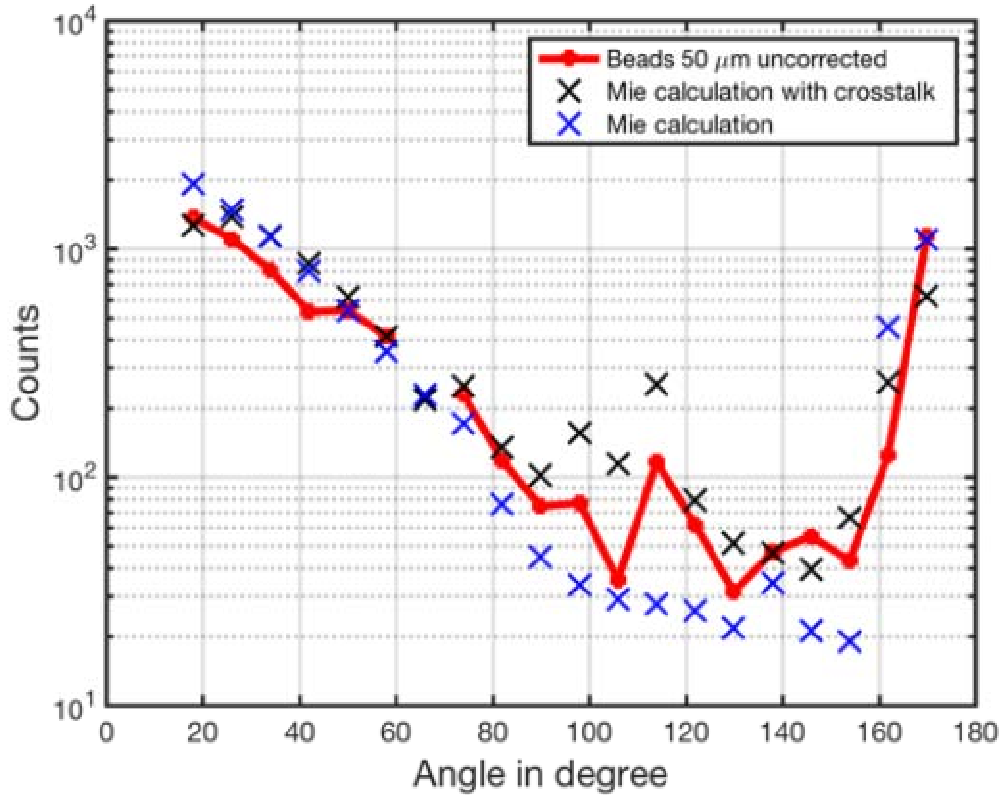
AC: We agree with the Reviewer that omitting particle images is troubling. Therefore, we added a collection of processed images from the glass bead measurements that were used to characterize the scattering part (Fig. 10 in the revised manuscript). The result of the respective image analysis is given in Table 4 in the revised manuscript. For the 50 µm standard both camera-telescope assemblies show a very good agreement with the manufacturer's specifications with deviations clearly within the uncertainty of the glass bead standard. In case of the 20 µm standard, assembly 2 oversizes the beads by 14 to 19% which is in agreement with the results by Schön et al. (2011) for this size range and which stems from the image analysis procedure. Assembly 1 oversizes the 20 µm beads by additional 15% which can be attributed to the reduced magnification compared to assembly 2. This magnification-dependent oversizing is additionally shown in Fig. 11 in the revised manuscript. Respective changes to the text are also added to the revised version.

RC: The discussion of scattering theory and the Nephelometer design is reasonable and thorough, and I appreciate the detailed simulations in Figure 7. However, I am disappointed in the laboratory data presented in Figure 8. I am most concerned by the appearance of the "crosstalk corrections," which are not mentioned in the previous sections or in the earlier 2011 PHIPS article. "Calibration factors" are mentioned in section 3.2 of the 2011 article but it is unclear what the difference between these and "crosstalk correction" is. While the discrepancy between observed phase function and a Mie theory phase function, might be explained by channel crosstalk, there is no effort to prove that this is the cause of the discrepancy. If an empirical correction factor is required, it should be extensively verified for as many particle sizes and types as possible. Different empirical corrections may be required for each particle type (aerosol, water, ice, etc.), and if that is the case than perhaps a redesign to eliminate PMT crosstalk would be warranted.

AC: The 2011 prototype instrument uses individual and optically separated photodiode detectors to measure the angular scattering function. Each of these detectors has its own adjustable gain and, therefore, the calibration factors given in the 2011 publication is mostly related to the different gain settings. Of course, these calibration factors also comprise differences in the field of view and transmission efficiency of the individual optical channels (composed of lens, fiber, and photodiode detector). In the airborne PHIPS instrument, due to space limitations, we use a multi-anode photomultiplier array (MAPMT) as described in detail in Section 3.1.1. The light coupling between the PMMA fiber system and the MAPMT is not ideal and results in a crosstalk of at least 15% as shown by the results of optical design calculations (Section 3.1.1. and Fig. 2 of the manuscript). This crosstalk *was verified by successively coupling laser light from an integrating sphere into the individual fibers while the other fibers were blocked. The result of this laboratory characterization of the MAPMT is shown in Fig. 3 in the revised manuscript. We have changed the text accordingly.*

RC: I would like reasonable justification that channel crosstalk can explain the phase function discrepancy. This could be accomplished by covering some of the collection optics (ever other channel, or all channels except those with large deviation) and repeating the glass bead tests. If crosstalk is the explanation than you should see better agreement with the theoretical MIE curve with this simple test.

AC: A similar laboratory test as suggested by the Referee was performed (please see our answer to the "crosstalk correction" above). In addition, the laboratory deduced crosstalk was applied to the theoretical Mie curve of the 50 µm glass beads (Fig. below). This figure shows that (a) the crosstalk related features in the side and backscattering range of the raw scattering function can indeed be reproduced and (b) there are additional correction factors that stem from the differences in the detector field of view and the fiber transmissions that makes a correction solely based on the laboratory deduced crosstalk impossible.



RC: The description of the glass bead calibration procedure should be provided. It is unclear if the glass beads were aspirated through the sample volume (to simulate aircraft speeds) or simply allowed to fall through the instrument.

AC: The glass beads were dispersed by a home-build particle disperser consisting of a small glass bulb equipped with inlet and outlet tubing. By supplying pressurized particle-free air to the inlet tube the glass bead powder in the bulb is aerosolized and is finally ejected through the outlet tube. Respective changes to the text are also added to the revised version (section 5, below Figure 8).

RC: More particle sizes should be tested to verify the crosstalk correction factors. It should be fairly easy to test a multitude of mono-dispersed glass bead sizes in the simulated size regime (5-200um).

AC: The authors agree with the reviewer on the importance of using more particle sizes for crosstalk correction verifications. This will however be done for the part II paper along with improving the coupling of the signals to the MAPMT to reduce the crosstalk.

RC: In addition to glass beads, a water droplet test should be performed to verify that the crosstalk correction factors are valid for particles with different index of refraction.

AC: Actually, we already did a comparison of PHIPS-HALO with the Polar Nephelometer (PN) of the Université Blaise Pascal, Clermont-Ferrand, France. This comparison was performed during AIDA cirrus cloud simulation experiments and can be found in (Schnaiter et al., 2016). In general, a reasonable agreement of the two instruments found in ice clouds which indicates that our correction scheme also works for ice particles.

Specific Comments

RC: Page 2 line 8. Here different scattering theories are discussed, and it is ostensibly implied that new Nephelometer measurements will help to improve our understanding of particle scattering. This is somewhat false because a "crosstalk correction" factor must be applied to the PHIPS data based on a theoretical Mie scattering curve. Therefore the Phase function data is constrained to Mie theory, so a discussion of other scattering theories is superfluous

AC: So far this is correct until the new design of the signal-to-PMT coupling is applied. It is a technical issue which will be overcome in future measurements. The following sentence is added to the

"Summary and outlook" section: "The crosstalk issue requires the application of cross talk correction factor to the PHIPS data based on a theoretical Mie scattering curve which limits the Phase function data to Mie theory. There is an effort started to redesign the coupling of the signal to the PMT in order to eliminate the crosstalk in future measurements"

RC: Page 2 line 39. Inter comparison between the CASPOL and a CPI would be more relevant to the PHIPS instrument, which records a true image of a particle unlike the SID instrument.

AC: A detailed comparison between the PN and CPI and the two parts of PHIPS (scattering and imaging, respectively) is considered in the revised version of the manuscript. Please check the modified text in sections 1 and end of section 3 and the added tables (Table 1 and Table 2)

RC: Page 7 line 8. In figure 2 the expected crosstalk value of 15% is stated, but here a value of 20% is presented. Which value is correct?

AC: 15% (corrected in the revised version)

RC: Page 17 Line 25. A maximum resolution of 2um for the imaging system is stated, but in section 3.1.2 a maximum resolution of 0.72um is implied (6.45um pixel / 9X magnification). Can you please explain how you arrive at a 2um resolution limit?

AC: The maximum "pixel resolution" is indeed 0.72. The optical resolution, which is more relevant, ranges from is 7.2um to 2.35um for the low (1.4X) and high (9.0X) magnification, respectively.

Schnaiter, M., Järvinen, E., Vochezer, P., Abdelmonem, A., Wagner, R., Jourdan, O., Mioche, G., Shcherbakov, V. N., Schmitt, C. G., Tricoli, U., Ulanowski, Z., and Heymsfield, A. J.: Cloud chamber experiments on the origin of ice crystal complexity in cirrus clouds, Atmos. Chem. Phys., 16, 5091-5110, 10.5194/acp-16-5091-2016, 2016.