AUTHORS' GENERAL COMMENT

1) During the reassessment of the measurement data from the ML-CIRRUS campaign a remaining instrumental measurement artefact was discovered that falsified the *PAS* measurements of the PIP. Due to the malfunction of a temperature sensor a systematically corrupted *PAS* output was generated that turned out to be the exclusive reason for the strange behaviour of air speed ratios with the *TAS* (cf. Fig. 6c and accompanied discussion in the unrevised manuscript version). Concretely, the data of the broken temperature sensor in the PIP provided a constant temperature value of about 320 K. Thus, the resulting pitot-measured *PAS* was generally falsified which causes a deviation that never exceeds more than 10 % compared to the *PAS* of the other instruments.

Thus, we admittedly made the mistake to base a fraction of our previous conclusions on an erroneous data set. Nevertheless, it needs to be noticed that this has no implication on the general discussion about the compression of air that needs consideration to obtain particle number concentrations. As a consequence of our mistake the discussion concerning the possible impact of a flow disturbance that we assumed to be induced by the aircraft fuselage on the PIP measurement was of course eliminated.

Fortunately, the artefact was reproducible and we were able to adopt the missing temperature data from another properly working underwing probe (e.g. the CCP). The reconstruction of the *PAS* data for the PIP from the temperature measurement of another probe may bear uncertainties. However, a sensitivity study revealed:

When assuming that the applied temperatures deviate by ±20 K from the true conditions the resulting *PAS* would deviate by about ± 5 %. Further, for an assumed temperature deviation by ± 10 K the resulting *PAS* variability ranges between ± 3 %. Thus, the uncertainty of the *PAS* reconstruction is of minor degree compared to the uncertainty that is generally given for the *PAS* measurement (which is most sensitive to the pressure measurement of the pitot-tubes) and the uncertainty that arises from using the wrong air speed for revealing particle number concentrations.

All Figures were updated in correspondence to the reconstructed *PAS* data for the PIP. Any excessive influence from the aircraft fuselage to the underwing probe's measurements is not apparent anymore.

The revision contains new Figure 6c that is based on the reproduced *PAS* data for the PIP adopting temperature measurements from another instrument. In the discussion the speculations concerning the possible influences was modified:

- **Page 13440, L 17-23, eliminated:** << In comparison with the other probes the most severe differences were found for the correction factors of the PIP. Here, the geometric correction with $\frac{PAS}{TAS}$ exhibits the highest effectiveness (~ 20 %) for lowest flight speeds and decreases with increasing velocities up to 190 m s^{-1} (~ 10 %). At a certain point ($TAS \approx 190$ m s^{-1}) the degree of correction with $\frac{PAS}{TAS}$ takes a sharp turn and climbs again as higher air speeds are reached. For *TAS*-values greater than 190 m s^{-1} a correction with $\frac{PAS}{TAS}$ would not strongly deviate from a correction made with corresponding ξ-factor. >>

- **Page 13442, L 8-10, deleted without replacement:** <<When comparing the *PAS* obtained from the individual instruments a feature sticks out that seems to solely result from influences on the dynamic pressure from measurements with the pitot-tube.>>

- **Page 13442, L 20-28, eliminated:** << Apparently the *PAS* measured by the PIP increasingly deviates from the CCP-*PAS* as a function of flight speed. Therefore, the deviation is not of a constant character, but rather increases with increasing flight speed. This shows that the extraordinary behaviour of $\frac{PAS}{TAS}$ over the range of flight velocity (cf.

Figure 6, right panel) is only connected to the dynamic pressure proportion obtained from the PIP's pitot-tube measurements. Unless the PIP could be deployed at another *HALO* underwing position, at least for one flight for investigating this issue more closely, it appear conceivable that the effect originates from an external interference due the PIP's proximity either to the wing root or to the fuselage of *HALO*. >>

- **Caption of Figure 6: following sentence erased** <<At the PIP position (portside, innermost) the behaviour of $\frac{PAS}{TAS}$ as a function of TAS is different, presumably due to air flow disturbances by the aircraft's fuselage.>>
- **Caption of Figure 8: rephrased**

All plots (Figures 8, 9, 10 and 11) for which PIP data of *PAS* were used are revised concerning the data the plot is based on. Furthermore the parameterisation of ξ is recalculated for the PIP (Table 1) and correlations are recalculated (Table 1). Finally the correction factors for the PIP data changed (Table 2).

2) One of the major concerns arising from the different comments points towards the admittedly too generalized statement that "$v_p = PAS$". We reassessed the image analysis which was indicated to feature a strong subjective component. By means of an automated process we reanalysed the images of two flight sections of the ACRIDICON-CHUVA campaign. After selecting flight periods with relatively high ambient temperatures (> 0°C) or high static pressures (> 300 hPa) the new analysis (cf. Appendix B in the revised paper version for details) allows for selecting automatically

a) only images emanating from cloud drops that pass the detection region not too far away from the detection plane
b) only images with diameter of >75µm (5 pixels and more) in either main direction

for further analyses. False images were sorted out automatically.

Subsequently the used algorithm approximates an ellipse to each of the selected images. For all details, please refer to the new Appendix B in the revised manuscript.

The results of the reanalysis are summarized as follows:

1) $v_p \approx PAS$ is largely non-restrictively valid for cloud droplets of diameter < 70 µm.
2) For larger droplets of diameter 100 µm < D_p < 225 µm indeed other penetrations speeds need to be considered. From our analysis the penetration speed for these larger droplets is at the best described with $PAS < v_p < TAS$. From this finding an inertia correction is suggested in the revised manuscript that may be applied to compression-corrected data.
3) For even larger droplets an inertia correction can only be surmised as images of liquid drops of 0.5-6 mm diameter are lacking in our analyses.

However, once these particles freeze and thereby even develop extremities (forming dendrites, plates, hollow columns, bullet rosettes, etc.) the inertia of some larger particles may be diminished.

In essence the corrections of measured cloud particle number concentrations concerning the particles' inertia need to be applied carefully, the application depends on the particles' size, phase and habit. In contrast, the air's compressibility is a general feature to consider which is of increasing importance with the aircraft's flight velocity.

Ignoring for a moment the presence of particles – it is the air speed that defines the air volume probed per time interval. Thus, there is a difference (of up to 30%) between the probed air volume, if based on *TAS*, compared to the one based on *PAS*. The resulting particle concentration differs by up to 30% only due to presuming different speeds of air. That the compression occurs upstream of an obstacle to the flow (as provided by the probes) is consistently indicated by means of static pressure measurements of three independent instruments.

If then comprising particles within the system: The reassessment of our image analyses revealed that

a) for the fraction of small droplets with $D_p < 70$ µm (that penetrates the detection region with $v_p \approx PAS$, rather than with $v_p \approx TAS$) exclusively the compression correction with ξ results in a good approximation of ambient particle number concentration.

b) For the fraction of larger particles (with higher inertia) indeed the ξ –correction alone is not sufficient, hence the mass correction μ needs to be considered.

We demonstrated that the corrections (by ξ and, as one novelty, by μ) need to be applied in consecutive order to measured number concentrations whereas the air's compression always needs consideration for correctly determining the air sample volume. The subsequent particles' inertia correction is of variable efficiency dependent on the particles' size, phase and habit. We hope to have clarified this with the revision and the constructive reviews initializing this comprehensive reassessment are gratefully acknowledged.

Review #1

Alexei Korolev: 1. One of the most important assumptions in this study is that particle velocity is equal to the local air speed, i.e. $v_p=v_2$. This equality was assumed to be valid for particle sizes covering the CDP, CIP and PIP nominal size range (i.e. 2µm to 6.4mm). The authors justified this assumption by referencing the relaxation time for a particle with D<100µm calculated for a specific *T, P* and *TAS*. Such an approach leaves the reader with many questions, such as: is this equality valid for D>100µm? Would it work for other *T, P* and *TAS*? What is the accuracy of this equality $v_p=v_2$? etc. Early works (works on aircraft icing in 40s and 50s, King, 1984, 1985, JTECH; King et al. 1984, JTECH) showed that particle trajectories may cross air streamlines, and on approaching an obstacle, their velocities v_p may not be equal to the local air velocity v_2. Casual observations suggest that cloud particle velocities may significantly differ from the local air speed. Otherwise, cloud droplets would never impact with the aircraft surfaces resulting in ice buildup on the airframe, or hot-wire sensors would not work the way as they do, because cloud particles would fly around the heated element following the air streamlines. Modern CFD simulation tools ease calculations of particle trajectories around the obstacles with arbitrary geometrical configurations. It is proposed that the authors to perform a particle trajectory analysis based on CFD simulations for each individual instrument configuration for a variety of particle sizes *D* (i.e. from 2µm to a few mm in order to cover the CDP, CIP and PIP size ranges) and *T, P*, *TAS*. One of the major goals of this analysis is to identify the envelopes of conditions (*D*, *T, P*, *TAS*), when the assumption $v_p=v_2$ is valid and when it breaks.

Authors' reply:

We largely agree with the comments by Alexei Korolev. As this paragraph basically is a summary of the individual comments listed in the following. Hence, for details, we kindly ask to refer to the individual replies provided hereafter.

Indeed the equality of made assumptions (in particular $v_p=v_2$) is not given for all detected particle sizes. We tried to concretise this point by a comprehensive revision of the manuscript. The effects accompanied with the particles' inertia indeed needed further regard. Concerning this issue the manuscript should have improved with its revision. CFD simulations of an idealized case were included, for one set of atmospheric conditions with one underwing probe geometry that was assumed to exhibit an isoaxial alignment with the air flow. Nevertheless, we believe that the constraints immanent with CFD simulations cause the calculations at the best to deliver indications, rather than to provide an ultimate reference. Further comprehensive CFD simulations are doubtlessly needed for a detailed investigation concerning the variable air flow conditions and resulting trajectories of particles (of both phases, liquid and frozen), by accounting for a variable aircraft attitude as well as for different conditions of *p* and *T*, moreover by comprising the aircraft's geometry, the underwing pylon and the adjacent instruments. All these aspects to cover with CFD simulations may constitute an individual study.

Alexei Korolev: 2. Difference in particles inertia will result in spatial sorting of cloud particles both along and across the flight direction. Therefore, strictly speaking, the velocity corrections for concentration should include dependence on particle sizes and particle density. The authors may consider utilizing CFD simulation and particle trajectory analysis to elaborate this approach.

Authors' reply:

We agree with the referee and further investigated the opportunity to extract an inertia correction from the image data analyses. As one outcome of the reassessed image processing we are indeed able to suggest an additional correction factor μ that accounts for the particles' mass as a function of particle diameter D_p.

Alexei Korolev: […] This approach would work, if the sampled particles are 100% known to be spherical. The identification of spherical particles was done based on the visual assessment of the roundness of digital images of neighbouring particles. This method has a significant subjective component, which may create problems in reproducing similar results by other research groups. The authors may consider getting a "second opinion" about the sphericity of cloud particles from PHIPS, which, according to Fig.1, was installed onboard for this field campaign.

Authors' reply:

We agree with Alexei Korolev and reassessed the investigation based on the images taken during two flights and by using an automated procedure that approximates ellipses to those images that were previously selected with respect to certain criteria. Selection criteria were either relatively high temperatures during flight (for ACRIDICON flight AC07, e.g. >0°C) or high static air pressures (for ACRIDICON flight AC07, e.g. >300 hPa) to minimize the probability of comprising to many solid (frozen) cloud particles in the analyses. Details about this new procedure are described in the new Appendix B and the results are discussed in the text. The use of the described algorithm improved the study in different ways: (1) the selection of images is more objective, (2) even more individual images were selected and evaluated (nearly 400 instead of ~200 images) which improves the statics and (3) the uncertainties are better determinable for each data point. Indeed the new results indicate that the previous investigation was biased to certain degree.

Alexei Korolev: At low pixel resolution identification of particles' sphericity from their discrete images is a challenging problem. The accuracy of particle sizing (in terms of pixels) is one pixel (Korolev 2007, JTECH, Appendix A). Therefore, for an image with three pixels the uncertainty in identifying the aspect ratio is ±30%. Appearance of the 3- and 4-pixels spheres can be found in Fig.6 in Korolev (2007). These images suggest that at low pixel resolution, naturally asymmetric particles are *not* distinguishable from spherical particles and may fall into the same habit category. This will break the original assumption about sphericity of the sampled particles in Fig.4.

Authors' reply:

As described in Appendix B, for the newly applied automatic procedure exclusively particles of >5 pixel extension (in either image direction, in line with and perpendicular to the diode array) were selected for further analyses to avoid the over-interpretation of images of small-sized particles.

Alexei Korolev: Since the cloud particles in Fig.4 were sampled at temperatures below freezing, there is a good reason to think that some of them were in solid state and may not necessarily be spherical. Large liquid drops are known to change their shape due to drag force (Pruppacher and Klett, 1997). Deformed large liquid droplets may be observed on 2D imagery.

Authors' reply:

Though some of the analysed particles may not be entirely liquid, but partly frozen, the symmetry of their projected images still strongly indicates their spheroidal character. Therefore, upon other reasons, by the reassessment nearly 400 individual images were analysed. The threshold criteria for the image selection were set to either ambient air temperatures above 0°C (flight AC07) or static air pressures above 300 hPa (AC13). This leads to a certain level of statistics to avoid misinterpretations due to singularities. A significant difference between the results of the differently selects data sets of the two individual flights is not obvious.

Concerning drag forces:

Case 1) the falling droplet

According to Pruppacher and Klett 1997 : " ….for $D_0 \leq 280\mu m$ they (the drops) may, for all practical purposes, be considered perfect spheres." . The authors use the term "spheres" (or the deviation from spheres) very strictly. So, to detect the deformation of a liquid droplet by an instrument with 15 µm size resolution the droplet should be well in the mm-size range, cf. Figure 10-14 [Pruppacher and Klett 1997]. Apart from that a more recent study [Thurai et al., 2009; (doi:10.1175/2009JTECHA1244.1)] shows that the axis ratio remains larger than 0.95 for liquid drops of up to ~2 mm.

Case 2) the droplet penetrating a region of rapidly increasing pressure

Due to the pressure shock accompanied with the approaching probe a maximum Δp of 60 hPa is reached within milliseconds over a distance of ~0.5m upstream of the probe. Scaled to the dimension of a millimeter-sized drop the effective pressure difference between the front side and the rear (in line with the direction of flight) of the drop is about 0.12 hPa. The pressure difference to overcome the surface tension of a millimeter-sized to cause deformation should be a factor of about 30 higher [cf. Green, J. Appl Met., 14, p.1578 (1975) or Pruppacher&Klett (1997), Paragraph 10.3.2]. Additional aerodynamic impacts on the droplet shape are of increasing importance for droplets of sizes greater than 2 mm.

Synopsis:

The large falling drop (3-6mm) will doubtlessly be deformed to an oblate spheroid (with axis ratio of 0.65 for D_p=6 mm; Thurai et al., 2009). It may be conceivable that the perpendicularly acting drag force due to the rapid pressure decrease (Δp of ~0.4-0.7 hPa on 3-6 mm scales for the drop diameter) in combination with the water's surface tension pushes back the drop's shape towards a quasi-spherical shape.

Alexei Korolev: Were out-of-focus images included in the diagram in Fig.4?

Authors' reply:

Out-of-focus images are indeed included in the automated analysis, restricted to $Z_d < 4$ (Korolev, JTECH 2007). The size ("length of the image main axis, µm") of the out-of-focus particle is reconstructed according to the procedure and parameterisation provided by Korolev JTECH 2007. In the revised manuscript we concretised that the analyses also comprise images with Poisson spot (in case $Z_d < 4$) and that the true diameter of affected particles was re-calculated.

Alexei Korolev: As stated in the paper for the images presented in Fig.4 the expected limit for the aspect ratio is 0.75. Since most of the aspect ratios were found to be above 0.8, the authors concluded, that, "*the penetration speed of the vast majority of detected particles through an OAP's detection region may be better described by the PAS (v_2), rather than by the TAS (v_1).*" This conclusion seems to be premature. First of all, the scattering of the aspect ratios show a systematic tendency to be less than 1, but higher than 0.75. This suggests that $v_1 > v_p > v_2$. Second, extrapolation of the mean aspect ratio (Fig.4b) toward large particles suggests that it will cross the 0.75 level at approximately 500µm. So, it is expected that for particles larger than 500µm, their velocity in the sample area will be better described by the TAS (v_1) rather than by the PAS (v_2), Authors' reply:

We fully agree with Alexei Korolev that cited sentence was an overstatement –after re-assessment of the image analyses this passage, beside others, was rephrased and clarified. Throughout the manuscript the phrasing concerning this issue was corrected whenever necessary.

Still we want to emphasis that the most important point in this context is the speed of air through the detection region.

Ignoring for a moment the presence of any particle in the probed air – it is the air speed that defines the probed air volume per time interval. So there is a difference (of up to 30%) between the probed air volume, if based on *TAS*, compared to the one based on *PAS*. Hence, also the resulting particle concentration differs by up to 30% dependent on the presumed air speed. The assumption that the air passes the detection region with *TAS* bears no plausibility as it ignores the compression of air upstream of the obstacle to the air flow as provided by each underwing probe.

If the comprising also particles within the system: Our previously too generalized assumption that the particles move with *PAS*, rather than with *TAS*, holds as an appropriate approximation only for a limited fraction of particles.

a) For this limited fraction (that penetrates the detection region with *PAS* rather than with *TAS*) this means that ambient particle number concentrations are obtained by applying the compression correction with ξ results on the data of number concentrations under measurement conditions.

b) For the fraction of larger particles (with higher inertia) indeed the ξ-correction alone is not sufficient: additionally, the particles' mass correction μ needs to be considered. This is factually an issue that we did not assess appropriately in the initial manuscript version and that was highlighted by the entirely reasonable insistence by A. Korolev.

We hope to have clarified this important issue and to have improved the manuscript with the revision. The advice regarding the article's weakness at this point was a crucial contribution to an improvement and may be gratefully acknowledged.

Alexei Korolev: In this type of analysis the size of the particles in terms of pixels should be equal to a whole number. The absence of particle sizes multiple of pixel resolution (15µm) in Fig.4a is worrisome and it should be explained.

Authors' reply:

The new automated analysis uses the retrieval of the particles' true size as suggested by Korolev, JTECH 2007. The vast majority of data points are grouped at whole number multiple of the 15µm resolution. Nevertheless, due to the size reconstruction of out-of-focus particles a certain amount of data points falls into size ranges in between the distinct groups. Thus, also the resulting data points for diameter <75µm (Figure 4) are a consequence of reproducing the particles' size from the relation of image size and Poisson-spot size [in accordance to Korolev (2007)].

Alexei Korolev: The aspect ratio should be calculated as a ratio of sizes along the flight and PDA directions. From the text it is not clear how the aspect ratio was calculated. It should be better elaborated in the next version.

Authors' reply:

For a detailed description of the applied analysis procedure the Appendix B is implied in the revised manuscript version. With the description of the revised image analysis procedure the aspect ratio calculation is now well defined in Appendix B. The aspect ratio is calculated from the lengths of the main axes of an automatically approximated ellipse.

Alexei Korolev: 4. One of the best indicators of the effect of the local velocity on the particle sampling is preferential orientation of the columnar and planar ice particles on the 2D imagery. Examples of preferential particle orientation could be found in Wendish and Brenguier (2013) in Fig.6.17. Particle orientation is quite sensitive to the disturbances of the local flow. If the columnar (columns, needles) crystals appear as randomly oriented and the planar (plates, stellar, dendrites) crystals are not flipped, then the effect of the local air speed on the particle velocity can be neglected. I would like to ask the authors to examine the existence of preferential orientation of columns and flipped plates (dendrites) at different *P* and *TAS* and use it as proof of particle velocity adaptation to the local air speed.

Authors' reply:

We would prefer to limit the investigation and discussion to the topic of spheroidal particles. This is what major parts of the argumentation are based on.

As an additional side-aspect we add this statement (with reference) to the manuscript indicating that it may be worthwhile to further investigate this issue highlighted by the Referee. However we would like to refine that this may be an obvious feature only if an ice particle population (e.g. 5-10 particles in a row) of almost similar particle type, shape and size is probed. Even if only ice columns but of different size are present within the probed air volume a preferential orientation may be disguised and not visible from the recorded images anymore. The uniformity of the ice particle population requires that the particles must have been formed relatively undisturbed over a certain period of time which might be a very particular case to encounter in nature (e.g. frontal cirrus clouds). In the outflow of a convective system, for example, the ice formation process likely occurs less undisturbedly, and the resulting diversity of ice particle species may be larger. Following is added to the text:

Furthermore, in the progressed state of ice formation certain disturbances of the local flow field in the vicinity of underwing probes can cause an almost uniform population of ice particles (either predominantly columnar or planar) to pass the OAP detection region with a preferential orientation (cf. Wendisch and Brenguier, 2013; paragraph 6.4.1.2 therein). Such a preferential orientation would be visible in recorded 2-D images of these particles exhibiting shapes that are systematically aligned. This effect is indicative for an impact of the local flow field on the ice particles' airborne state while passing through the detection region.

Alexei Korolev: 5. Another important assumption in the paper is that the air speed measured by the Pitot tube is equal to the local velocity in the sample area. The location of the Pitot tube and the sample area are spatially separated and the local air speed may be quite different. It is proposed to simulate the airflow around the particle probes for different *T, P*, *TAS* and identify whether the difference between the airspeed in the Pitot tube location and the probes sample area is significant. Essentially, this means that the airspeed in the Pitot tube location should be recalculated to the TAS in the sample area location depending on *T, P*, *TAS.*

Authors' reply:

As simulated by Abdelmonem et al, ACPD 2016 by means of a CFD tool the pressure and temperature increase immediately upstream of an underwing probe is consistent with our findings. Their studies show an increase of up to ~50 hPa and a by ~10 K largely agreeing with the values measured with the pitot tube. The instrument's own pitot tube is closest to the instrument's sample area. Of course, this way the conditions well inside the sample area are only kind of approximated rather than perfectly reflected. However, it may be emphasised that the compression of air is largely reflected in the increased static pressure within the detection region compared to ambient conditions.

Moreover, the same way of argumentation which is used by the referee would hold for critically discussing the conventional and widely used practises to treat measured particle number concentration by generally insinuating the aircraft's *TAS* to determine the detection volume or to multiply the measured number concentration with $\frac{PAS}{TAS}$ with the supposed purpose to correct to ambient conditions. The *TAS* is measured even further away from the probe. Finally, as argued in the paper: if the *PAS* measurement was too wrong the resulting images were likely more distorted than they actually are. Thus described procedure is certainly not the ultimate way to account for all uncertainties accompanied with the underwing probe measurements but it may constitute an improvement compared to the conventional approaches.

Alexei Korolev: 6. Accuracy of the airspeed measurements by the probes' Pitot tube is critical for the quantification of the results of this study. In this regard, it is important to provide an explanation how the Pitot tubes were calibrated.

Authors' reply:

The accuracy of airspeed measurements by the probe's Pitot tube is critical for any of the conventionally used approaches to determine particle number concentrations from OAP measurements - if the *PAS* is used, or if the data are treated with the *PAS*/*TAS* factor to determine number concentrations. The Pitot tube is calibrated on occasion by the instruments' manufacturer when a technical service is conducted on the instruments. We have no reason not to trust in the manufacturer's ambition to perform this calibration in an appropriate way though details about the experimental calibration setup apparently underlie the company secret.

Apart from that it would be really surprising, in the case the Pitot tube calibration was severely biased, that two individual and entirely independent instruments (CCP and NIXE-CAPS) show largely agreeing *PAS* measurements during flight. Underlying only the static pressure measurements even three independent instruments deliver highly consistent data.

As shown in line17-23 of page 13436, the ξ-correction, as one of the results of this study, is almost insensitive to uncertainties of the measured *PAS*. ξ is affected by about ±3 % even if the measured *PAS* was ±20 % off (insinuated at this point as a very strong but unrealistic deviation).

Alexei Korolev: 7. The consideration of the effect of the local air velocity presented in the manuscript is suitable for the particle probe with a small sample volume, e.g. CDP. The CFD analysis of the airflow around the particle probes in Korolev et al. (JTECH, 2013) showed that for the probes with distributed sample volume (e.g. CIP, PIP) the local airspeed is changing along the sample volume. The non-uniform distribution of the air-speed in the sample volume adds complexity in the consideration of velocity corrections, and it should be discussed in the text.

Authors' reply:

With describing a procedure – to our best understanding – how to treat the data from underwing-operated OAPs we do not raise the claim of providing the ultimate solution of correcting the data. Much more OAP measurement uncertainties may not have been discovered or considered yet. However, on the way to gather particle concentrations we believe that accounting for compression upstream of the obstacle to the air flow field as provided by the probe's body covers a large portion of uncertainties. Nevertheless following sentence was implied in Section 4 of the revised manuscript:

Further effects may be considered which additionally concern the ability of the cloud particles to adapt to sudden changes of the flow field upstream of an underwing probe. CFD simulations by Korolev et al., 2013 for example indicate that the air speed locally varies along the sample volume. Thus, the complexity of velocity corrections increases when considering also an inhomogeneity of the air-speed distribution within the sample volume.

Alexei Korolev: 8. The airflow around the particle probe housing with a good approximation can be considered as adiabatic, which yields relationship between P and T as $\left(\frac{P_1}{P_2}\right)^k = \frac{T_1}{T_2}$, where $k=(c_P-c_P)/c_P$. This will reduce the number of variables in the equations, resulting in $N_a = N_m \left(\frac{P_1}{P_2}\right)^{1-k}$

and $N_a = N_m \left(1 - \frac{1}{2c_p\, T_1}\,(v_1^2 - v_2^2\,)\right)^{\frac{k-1}{k}}$. Actually the diagram in Fig.7b reflects the adiabatic relationship between *T* and *P* quite well.

Authors' reply:

As hereby motivated and also due to suggestions made by Referee # 2 we decided to move large parts of the ξ-derivation into the new Appendix A and additionally provide different approaches to examine the compression problem which exceeds beyond what was provided in the previous manuscript. In essence, suggested approach (by A. Korolev) is discussed in Appendix A denoted as ξ_{II} which combines the compression effect together with (partial) presumptions of adiabatic processes to occur. One further approach was formulated which accounts for the processes inherent with the compression to be entirely adiabatic (ξ_{III}) which provides an overall consistent extension of the approach leading to ξ_{II}. Our approach, as described with ξ_{I}, still contains measured variables (observations). The comparison of all approaches reveal best agreement between ξ_{III} and ξ_{I}. This means that our approach ξ_{I} (predominantly based on observational data) delivers the best approximation of what results from assuming all process to be entirely adiabatic (ξ_{III}). In the new Appendix A we comprehensively describe the derivation of the approaches, and we compare and discuss their advantages and disadvantages.

Alexei Korolev: In conclusion, I would like add that based on the particle trajectory analysis performed for the particle probes I believe that the thermodynamic corrections (Eq.11) for CIP and PIP may require including *D* in the correction coefficient. I realize that particle trajectory and CFD analysis are time and resource consuming, and it may result in a significant delay of the publication. In this regard, the authors may consider publishing these results only for the CDP for which the assumption $v_P=v_2$ may work quite well.

Authors' reply:

The correction for the air compression should be applied in any case, for any probe, any measured concentration and independently from the particle diameter, mass, or inertia for the following reason:

It is the air speed that defines the probed air volume per time unit. So there is a difference (up to 30%) between the probed air volume if based on *TAS* compared to the one based on *PAS*. The resulting particle concentration differs by up to 30%.

The inertia correction of larger particles only holds for a certain fraction of measured particles. For this fraction of larger particles (with higher inertia) indeed the ξ –correction alone is not sufficient – the mass correction μ needs to be considered.

Nevertheless, the plausible way to correct measured particle number concentration to gain ambient concentrations is

1) First to consider the compression of air as this is the basis for defining the probed air volume per time
2) And then to apply corrections that account for the inertia of those particles that are subject to it.

To generally assume that particles move with *TAS* appears as wrong as insinuating *PAS* as the general penetration speed over the entire particle spectrum. But *PAS* determines the probed air volume. Moreover, if accepting that particles with diameter smaller than 70 µm actually have the potential to adapt *PAS* for penetrating the detection region, this means that for a large fraction of a typical cloud particle spectrum the concentration is sufficiently corrected with ξ.

Alexei Korolev: I hope that the above comments will help in improving this paper,

Authors' reply:

We want to express our appreciation for this comprehensive but extremely constructive and helpful review that indeed motivated us to re-assess a significant part of our study which doubtlessly led to a huge improvement of this article.

REVIEW #2

[...]

Referee #2: 1. My biggest concern about derivation of the new correction factor proposed in this manuscript is the assumption that the inertia of the particles is negligible, resulting in equations *V*p=PAS and n/M=const. in the text. Particle size range covered by the instruments mentioned in this work spans over more than two orders of magnitude which translates to more than four orders of magnitude in variation of particle relaxation time. It would be difficult to accept that particle inertia can be neglected for this entire size range. In fact, decreasing trend of aspect ratios in Figure 4 confirms that particle inertia becomes more important as particle size increases. It might be more accurate to assume Vp=PAS at the lower end of the size spectrum and Vp=TAS at the higher end (larger particles). Moreover, data presented in Figure 4 is only for particle diameters smaller than 350 microns, perhaps because data for >350 micron particles has been scarce. Without sufficient data, it is not clear how it was determined that the assumption of negligible particle inertia is valid especially for the larger particle sizes under study.

Authors' reply:

We agree with the Referee's comment as well as with the reviews of the other Referees concerning this issue. Indeed, the general validity of made assumptions (in particular $v_p = v_2$) is not given for all detected particle sizes. We tried to concretise this point by a comprehensive revision of the manuscript with respect to this concern. The effects accompanied with the particles' inertia indeed needed further regard which should be improved with the manuscript revision. However, the reassessment of our approach also does not allow for concluding that particles of a certain (large) size move through the detection region with a speed equal to *TAS*. The result of the newly performed image analysis by means of an automated categorization and sizing procedure indicates that presumably liquid cloud drops of larger size ($D_p > 100$ µm) penetrate the detection region with a speed that is better described with $PAS < v_p < TAS$, rather than with $v_p = TAS$. Nevertheless it needs to be noted that this findings from the image analysis of presumably liquid drops may not hold for solid (frozen) cloud particles: Due to increased mobility of ice particles (particularly if the ice particles develop extremities) the inertia may be diminished. Indeed our case study of image analysis is limited at the upper end of the size spectrum, as correctly suggested by Referee #2, due to the gap of detection of larger cloud droplets.

Referee #2: Would an equation such as Belyaev and Levin (1974) or a similar correlation possibly developed for sub-isokinetic particle sampling in compressible flows provide a better estimate on enhancement of particles due to inertial effects?

Authors' reply:

The study of Belyaev and Levin is particularly aiming at the description of the aspiration efficiency of aerosol inlets that are used to collect aerosol particles from the free air flow and to lead these particles into an instrument (sometimes deployed within an aircraft cabin). The aspiration efficiency of an aerosol inlet strongly depends on the inlet's geometry, in particular the width of the inlet nozzle and the nozzle's wall thickness.

Picking up only a very few aspects of the Belyaev and Levin-approach: Here, the flow conditions (the ratio of the flow velocity inside the inlet, versus the ambient free air flow velocity) are of particular interest concerning isokinetic sampling as interactions between the nozzle flow and the inlet's walls have significant impact on the inlet's aspiration performance. Furthermore the aerosol inlet angle towards the free flow field is of particular importance (cue: iso-axial sampling) which has impact on the inlet's aspiration efficiency and thus on the particles to collect for subsequent detection.

In this sense, for example, the “inlet walls” of an underwing cloud probe are far away from the detection region and thus the particles of interest. The angle of the very small (punctual) detection region towards the free flow field is, within certain limits, comparatively less critical. For the measurement with underwing cloud probes the particles do not have to enter a small, sharp-edged nozzle of finite size.

Thus, it is conceivable that a particular solution of the Belyaev and Levin theories, after customizing all characteristics that basically concern the sampling with an aerosol inlet, is adoptable to the comparatively simple problem arising with air compression upstream of a flow obstacle. However, approaching this problem via thermodynamic considerations appears to be more straightforward than by trimming an approach that initially focusses at another problem.

Referee #2: 2. Please include explanation or reference on how the probe air speed (PAS) was calculated. If the underlying assumption in this work is that the airflow around the probe is compressible, wouldn’t temperature measurement be necessary for measuring the airspeed at the probe location also (Lenschow and Spyers-Duran, 1989)? If no temperature reading is available from the wing mounted probes and incompressible flow equation is used for calculation of airspeed, I suggest including a discussion on magnitude of the error introduced due to this simplification and clarification on what value was used for air density when calculating the velocity.

Authors’ reply:

The way the air speed is calculated from the instrument’s pitot-tube measurements is explained in the instrument manual:

Droplet Measurement Technologies Inc.; Data Analysis User’s Guide; Chapter II: Single Particle Imaging; DOC-0223, Rev A, 2009; in Paragraph 2.2.1 “Clocking Errors”; available on

http://www.dropletmeasurement.com/sites/default/files/ManualsGuides/Data%20Analysis%20Guide/DOC-0223%20Rev%20A%20Data%20Analysis%20Guide%20Ch%202.pdf

Of course, this is a reference that needs to be introduced in the manuscript, as it is in the revised manuscript version, provided that this reference is accepted.

However, the air speed measurement uses temperatures measured (with ± 3 K uncertainty) at each individual probe. It needs to be considered that the uncertainty of the temperature measurement is of minor importance for the resulting *PAS* (e.g. ± 3 K affects the *PAS* to vary by less than 1 %). In essence the resulting *PAS* is mostly impacted by the accuracy of the pressure measurement compared to the temperature measurement.

Referee #2: 3. Eq. (8) shows the change in enthalpy for a constant-pressure process, which is not the case for the compression process between states 1 and 2. General form of the enthalpy change has a an additional term not shown in Eq. (8), e.g., dh = cp . dT + [v - T . (dv/dT)p] dp, where v is specific volume and (dv/dT)p is partial derivative at constant pressure. Why was the second term ignored in Eq. (8)?

Authors’ reply:

We thank the reviewer for the correct representation of the total derivative of the enthalpy. In general, changes in temperature and pressure contribute to changes in the enthalpy, since the enthalpy is a thermodynamic state function which generally depends on temperature and pressure. However, for atmospheric conditions we can assume, as a good approximation of reality, that air behaves as an ideal gas. For an ideal gas ($p\,V = M\,\mathbf{R}_s\,T$ or $p\,v = \mathbf{R}_s\,T$), the second term (which describes changes due to the pressure) vanishes, thus we can approximate changes in specific enthalpy by $dh = c_p\,dT$.

In detail:

$$dh = c_p dT + \left[v - T \frac{\partial v}{\partial T}\bigg|_p \right] dp,$$

with the specific volume: $v = \frac{V}{M}$.

For air as an ideal gas it follows: $pV = M\,\mathbf{R_s}\,T$, or rather $pv = \mathbf{R_s}\,T$, such that $v = \frac{\mathbf{R_s}\,T}{p}$.

Thus, it follows

$$\frac{\partial v}{\partial T}\bigg|_p = \frac{\mathbf{R_s}}{p}$$

and therefore

$$\left[v - T \frac{\partial v}{\partial T}\bigg|_p \right] = v - v = 0$$

So in any case, even when following suggested approach, for an ideal gas it follows:

$$dh = c_p dT.$$

Referee #2: 1. Page 13430, L21-23 and Page 13431 L1-8 are difficult to follow. Please consider re-phrasing.

Authors' reply: The indicated text passages are largely rephrased in the revised version.

Referee #2: 2. Page 13436, L17-23, this section seems to suggest that the advantage of the new method is in smaller uncertainty compared to method of using airspeed ratio. The issue here is not about the decreasing the uncertainty using the same input data, but the accuracy or rather correctness of the formulation used. Please consider rephrasing this section to avoid misleading conclusions.

Authors' reply:

➔ We fully agree: in the revised version the following sentence is eliminated: Hence, with the sensitivity of a correction factor to uncertainties in measured PAS is reduced, whereas the air speed ratio PAS/TAS is unabatedly affected by any uncertainty in measured PAS

Referee #2: 3. I suggest eliminating "in" for the axis titles in the plots for better clarity, for example, replace "length of main image axis in um" with "length of main image axis, µm"

Authors' reply:

➔ Corrected as suggested

References:

1. Lenschow, D. H. and Spyers-Duran, P., Bulletin 23, Measurement Techniques: Air Motion Sensing, NCAR Research Aviation Facility, 1989, https://www.eol.ucar.edu/raf/Bulletins/bulletin23.html

2. Belyaev, SP, Levin, LM, Techniques for collection of representative aerosol samples, Aerosol Science, 5: 325–338, 1974.

REVIEW #3

General Comments

Referee #3: […] However, the authors seem to be missing a critical part of such a correction: the deviation of particles from the streamlines that will occur for some cloud particles. In the larger size ranges, particle velocity isn't necessarily equal to the local velocity, as is assumed here. This deviation from streamlines can result in concentration changes from freestream that are substantially different from those calculated based on changes in airflow/density alone. This is touched upon, but not quantified by the simple relaxation time approach mentioned. The other reviewers discuss this problem as well, so I'll only add that the authors should discuss and try to quantify this inertial effect. They should also refer to earlier work in this area, such as King, JTECH 1984, Norment, JTECH Dec 1988, Twohy and Rogers, JTECH 1993 and Dhaniyala et al., Aer. Sci. Tech, 2004.

Authors' reply:

We partly agree with the comments of Referee #3 – yes the particles' velocity is not generally equal to the local air velocity. Nevertheless the local air flow velocity determines the probed air volume probed per time unit and is thus essential for obtaining particle number concentrations. The impact of the particles' inertia to the resulting number concentration of detected events is considered in the revised manuscript. Any effect on the particles such as deviation from the streamlines which may result in the non-detection of particles is of course not quantifiable but this would be a general impact and thus would principally query the measurement technique. However any quantification of such effects based on our measurements were highly uncertain and detailed investigations related to this objective, e.g. by means of comprehensive CFD simulations are beyond the scope of this study (please also refer to replies to the comments of Referee # 1). The approach via the relaxation time – as used – provides only an indication that particles of certain size exhibit sufficient mobility to be struck by the compression region upstream of an underwing probe. The inertia effect– as far as quantifiable based on recorded detection events - is added in the manuscript revision together with some further discussion. The extremely valuable work of King and Norment is acknowledged in the revised version. The studies of Twohy and Rogers, JTECH 1993 and Dhaniyala et al., Aer. Sci. Tech, 2004 instead concern the flow field and impact immediately along the aircraft fuselage. Thus it is not necessarily clear if their conclusions are convertible to the cloud-probe positions underneath the aircraft's wings.

Referee #3: Without a detailed discussion of the above issue, I don't think the paper can move forward. As one of the other reviewers suggests, computational fluids analysis would be the best way to address this for complex, potentially interacting probe configurations. Also, wasn't some CFD analysis already done for the HALO underwing locations, with the potential effect of airflow distortion on particle trajectories examined when the locations were first selected? This would provide mean flow conditions upstream of the probes, and potentially, any effect of the wing and fuselage themselves on the air streamlines and particle concentrations. Some discussion of these results should be included, even if not fully available in the open literature.

Authors' reply:

- ➔ The first part of the referee's comment is related to the major concerns of A. Korolev (Referee #1) and anonymous Referee #2. Please refer to the general authors' reply provided in a supplemental document.
- ➔ The second part of the comment points towards CFD simulations made for the HALO aircraft. According to personal communications with the person who performed this study, Mrs. K. Witte (DLR, Oberpfaffenhofen) the results of this work, unfortunately are subject to a non-disclosure agreement with the aircraft's manufacturer which might explain why these results have never been published and thus are not available in open literature.

Referee #3: You might consider moving some details of the equations or other aspects to supplemental material, and the paper seems rather long and will be longer when the discussion of particle inertia is included.

Authors' reply:

➔ We understand that publications concerning experimental techniques are usually not interspersed with extensive theoretical derivations. But, as it is one crucial point of our argumentation, we provide the detailed derivation of the correction factors in a new Appendix A together with some further considerations and discussions that arose from the comments of A. Korolev (Referee #1) and the anonymous Referee # 2.

Referee #3: 13425, lines 21-22: The reason why underwing locations are widely used for cloud measurements should be explained, as this has not always been recognized. Similarly, on page 13427, lines 4-5: why were these particular locations selected? Presumably because of relatively undisturbed airflow based on earlier computations at DLR, which should be mentioned.

Authors' reply:

➔ The studies of King and Norment doubtlessly provide pioneer work concerning the objective of particular positioning the probes underneath an aircraft's wing and this works are acknowledged in the revised version. Concerning the computations at the DLR, please refer to the Authors' reply to one of the previous comments above. The concrete problem discussed at this point in the manuscript turned out to result from a previously unrecognised defect of a sensor (cf. Authors' general reply). The error was eliminated in the revision process and resulting conclusions are based on recalculations and were rephrased accordingly.

Referee #3: 13426, line 28: I believe "velocity" should be "speed".

Authors' reply:

➔ Corrected

Referee #3: 13446: I found this section hard to follow. Were size distributions from the tethered probe available to compare with those from the underwing probe to aid in evaluation of the correction factors? Or was only one probe available at any one time?

Authors' reply:

➔ Indeed, for the measurements with the Learjet-platform exclusively one instrument of its kind was deployed. Thus, there is no redundant measurement available for the Learjet platform as this is contrarily the case for the measurements with HALO that allow for manifold instrumentation with overlapping detection ranges.

Referee #3: 13446, lines 25-26: Again, I thought this was something that had been previously modeled when selecting these locations, and would be very useful information.

Authors' reply:

➔ Cf. comment above – however, as this concrete conclusion was affected due to a discovered error of the PIP's operation this point may already be clarified. Please refer to the reassessment of this issue in the revised manuscript version and the Author's general reply.

Referee #3: 13428, line 6: The comma after "both" instead should be after "following", and on line 7, the one after "measurements" should be removed.

➔ Corrected

Referee #3: 13431, line 28: "expresses" should be "expressed".

- ➔ Corrected

Referee #3: 13444, line 14: I think “where” should be “were”.

- ➔ Corrected