Review of the manuscript: “Thermodynamic correction of particle concentrations measured by underwing probes on fast flying aircraft” by Weigel et al.

Overview: This work considers an important question regarding the effects of local air velocity on the particle concentration measured by airborne probes. In this paper, the authors took the next step towards refining the correction coefficient of the measured particle concentration by utilizing a thermodynamic approach. The consideration of the thermodynamic correction factor is based on several assumptions, which unfortunately, did not receive sufficient justification in the manuscript. In my opinion, in its present form, the paper is not ready for publication in AMT. However, because of the great importance of the considered question, and the large anticipated impact of this work on the cloud instrumentation community, I would encourage the authors to address the questions listed below and resubmit the manuscript.

Comments:

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.

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.

3. The purpose of the diagram in Fig.4 is to illustrate that the particles adapt their speed to the local velocity. This justification is based on the idea that the images of spherical particles appear as circles (aspect ratio =1) if the TAS clock rate is set accordingly to the local air

speed. Otherwise the particle image will appear as an ellipse elongated along the flight or a photodiode array (PDA) direction, i.e. the particle image will be oversampled or undersampled. This approach and the diagram itself generated several questions listed below.

(a) 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.

(b) 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.

(c) 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.

(d) Were out-of-focus images included in the diagram in Fig.4?

(e) 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),

(f) 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.

(g) 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.

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 Brengiuer (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.

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.*

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.

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.

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 $k = \left(c_p - c_p\right)/c_p$. 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 $N_a = N_m \left(1 - \frac{1}{2c_pT_1}\left(v_1^2 - v_2^2\right)\right)^{\frac{k-1}{k}}$. Actually the diagram in Fig.7b reflects the adiabatic relationship between *T* and *P* quite well.

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.

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

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