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Atmospheric Measurement Techniques Discussions

Interactive comment on "Ice particle sampling from aircraft – influence of the probing position on the ice water content" *by* Armin Afchine et al.

Anonymous Referee #1

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Review of "Ice particle sampling from aircraft-Influence of the probing position on the ice water content." By Afchine et al.

Recommendation: Might be acceptable for publication pending mandatory major revision

The subject matter of this paper is very timely and appropriate for AMT, examining how the positioning of microphysics probes affects the measured ice water content. The authors compare measurements of bulk ice water contents (IWCs) made on the roof of the aircraft fuselage against those attached under a wing of the HALO aircraft, and also compare measurements mounted on the fuselage side and bottom of the Geophysica. Based on their comparison, the authors claim that the reason the IWCs measured by the roof inlets deviate from those under the aircraft wing is caused by the

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Discussion paper

shadow-zone behind the aircraft cockpit. Although the authors do provide one good piece of evidence to justify their claims (their Fig. 10 that shows how the ratios of the roof/wing IWC with the mean ice crystal size), overall I found that many of the claims made in the manuscript were not well justified by the data presented and the authors did not consider all the nuances associated with the probe positioning. Provided that the authors are able to do a more thorough job of discussing the limitations of their findings and provide better justification of their conclusions, I feel that this paper should be ultimately accepted by AMT.

The first major problem with the manuscript is that the authors overly simplify the discussion of the flow around an aircraft and the impact of the positioning of the probe. Although their Fig. 1 (adapted from King 1984) is a great illustration of the conceptual flow around an aircraft, it is important to note that the King (1984) calculations show that for the three different aircraft shapes they examined that the width of the shadow zone and the concentration enhancement factors could be described in terms of the scaled fuselage radius and a parameter similar to the Stokes number. I did not see anywhere in this paper where the authors discussed the expected location of the shadow or enhancement zones based on the fuselage radius or this "Stokes parameter" for either the HALO or Geophysica. It would seem that such a calculation would be required to justify their conclusions. Note, that ideally a complete flow analysis around the aircraft would be completed, but I am aware that such an analysis would be well beyond the scope of the paper. However, this later calculation would be something well within what would be expected for the scope of this paper.

Second, the authors make no comments about the position of the probe away from the fuselage, above the roof, or underneath the wind of the aircraft. Knowing this location is very critical to determining whether the probe is in an enhancement or shadow zone. For example, probes underneath an aircraft wing that are either not far enough below the wind or not far enough ahead of the leading edge of the wing might also suffer some large effects from the flow around the aircraft. The authors have included no

discussion about this whatsoever in their paper. First, I think more details about the mounting location of the probes are required, and this position should be assessed in terms of the expected location of the shadow/enhancement zones from the King (1984) type analysis.

Third, I see that the scatter between the IWC derived from the side and wing-mounted probes of a factor of 2.5 quite large. Further, the scatter can go even beyond this mean 2.5 figure. This seems extremely large to me. What is the uncertainty of the IWCs that are measured by these probes? Is it as large as a factor of 2.5? If less, what is causing the large amount of scatter? There needs to be more thorough uncertainty analysis than is currently presented in the paper.

Fourth, the authors attribute the differences they are seeing to the locations of the different probes. I agree that this seems to be the most likely reason for the differences. But, it would seem that to properly attribute this to the location of the probes, experiments should have been performed where the probes were switched between the different positions to see that the same order of differences still occurred. Is such a switch possible given the mounting possibilities on the aircraft?

Fifth, the authors need to do a better job in characterizing the uncertainty associated with the derivation of IWCs associated from size-resolved measurements through the use of m-D relations. Whereas the authors do acknowledge these uncertainties, noting that the bulk IWC is less error-prone in comparison to the IWC from the PSD, I feel that they are rather premature in making the claim that their m-D relation has demonstrated the robustness of their connection between cirrus ice crystal size and mass. This again seems a bit suspect given the difference of a factor of plus or minus 2.5. This also seems quite large compared to some other studies that have studied the variance of m-D relations in how they are related to calculations of bulk IWC and comparison with that derived from size distributions. How would the use of other m-D relations compare? Would some also work within the 2.5 factor or would they be smaller/larger? These issues need to be addressed especially if a conclusion is going to be made

that “the agreement of the IWCS . . . demonstrates the validity of the m-D relation of Erfani and Mitchell (2016), slightly modified by Kramer et al. (2016) and Luebke et al. (2016).” There can be variations in m-D relations as the particle habits and densities can change not only with temperature, but also with the type of cloud being sampled.

In addition to the effect of the flow around the aircraft, there can also be impacts due to the geometry of the probe itself and the flow around the probe. For example, due to the flow around the CAPS probe, there are pressure perturbations around this probe that also might exist that could perhaps cause some flow distortions. Further, no comments were made about bouncing off the plane surface. Depending on where the probes are mounted underneath the wings, there can also be bouncing off of particles that can affect the measurements.

Finally, I think it is also very important that the conclusions made are specific. The authors may want to claim for that the particular probes mounted on the specific aircraft at the specific locations, there are certain things that can be said about preferred mounting locations. However, there simply is not sufficient evidence to generalize these findings to mounting locations on aircraft in general, or to locations in general (below wing, on roof, on fuselage, etc.)

Other Comments:

Page 5, line 19, What is sufficient distance?

Page 7, line 30: Was there any precipitation probe? What did the mass distribution function look like? Is there any possibility some mass is being missed in the IWC from the lack of particles above 937 micrometers being measured? Even if such particles are contributing minimally to the number, they can contribute more substantially to the mass.

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