Atmos. Chem. Phys. Discuss., 15, C4942–C4947, 2015
www.atmos-chem-phys-discuss.net/15/C4942/2015/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "High ice water content at low radar reflectivity near deep convection – Part 1: Consistency of in situ and remote-sensing observations with stratiform rain column simulations" *by* A. M. Fridlind et al.

J. Stith (Referee)

stith@ucar.edu

Received and published: 15 July 2015

General Comment. This paper is part of a two-part series focused on the meteorological conditions responsible for jet engine power loss. While it falls in the category of applied-aviation research, it includes enough topics of basic atmospheric physics to be of interest to readers of this journal. There are at least two major concerns with this (type of) study. First the ice water content conditions that result in engine failure are inadequately known, in terms of magnitude and horizontal extent (duration). Sec-

ond, our ability to accurately measure the ice water content is inadequate, especially when the wide range of possible atmospheric values is considered and considering that sizes and masses of ice particles ranging from about 10 microns to several millimeters must be sampled properly. Together, these two problems limit the utility of this paper and the authors should try to reduce these unknowns as much as possible and, if that is not possible, they should at least try to provide more detailed information than is presently included in the paper. Below, some suggestions are made in this regard and some problems that should be corrected or addressed are described. If proper improvements to the paper can be made, it should be published.

Specific Comments. Section 1. The introductory material provides a very useful overview of the problem and a history of the current situation. However, it would be improved by providing more complete information on what amount of ice ingestion is likely to cause a problem in modern engines. While this may not be known for all engines, there must at least be some minimum value that the authors believe would not be a problem and this information would help the reader better understand which of the sampled clouds are likely to be of interest. A second area which might provide more context for the present study is engine certification requirements (FAA, 2014), especially part 33, appendix D, which includes both length scales and magnitude scales for water content, yet these requirements are not referenced in the paper.

Section 2. The origin of eq. (1) is not clear. Contrary to what is stated the text, it does not appear to be in either Baker and Lawson (2006) or in Lawson et al. (2010). Lawson et al. (2010) used the area-mass relationship from Baker and Lawson (2006), not eq. (1).

The focus of this paper is on stratiform anvils associated with deep convection, yet they do not partition the in situ measurements to determine which ones were made in convective regions versus stratiform regions (e.g. in Fig. 2). This is a surprising omission, since regions with active updrafts would likely exhibit the highest water contents and the stated purpose of the measurements was to find and stay within the highest

IWC regions possible, within the limits of safety. While the aircraft might not have been equipped for vertical wind measurements, aircraft performance and flight conditions (e.g. vertical accelerations) might be useful proxies to segregate the data into convective and stratiform segments. It would also be helpful to state the safety limits or safety criteria, since they are significant experimental constraints on the reported data.

Section 3. This section, on the Airbus measurements, has several problems that should be addressed before the paper is published:

1. The nephelometer was designed for measurement of liquid droplet size distributions but used here for measurement of ice mass size distribution, yet the performance of the instrument for sampling ice is not discussed. Unlike most airborne cloud physics instruments, details on this instrument performance are not widely available in the literature, so the authors should devote more effort to describing its performance (e.g. see items 2 and 3 below).

2. The reference on the nephelometer (Roques, 2007) does not adequately describe the instrument, as it does not include information on the sample volume, the effects of out of focus particles, etc. Sample volume is of particular concern, since many of the clouds in this study contained large ice particles, which typically require a large sample volume (e.g. compared to liquid water measuring instruments) to sample effectively.

3. Of particular concern is that the effects of particle shattering and breakup for the nephelometer are not addressed in this study. Several recent studies (including those by the paper's co-authors!) have documented that particle breakup during sampling has a very significant effect on the measured particle size distribution. This has been demonstrated for traditional optical array instruments (Jackson et al., 2015, and references therein, provides a recent overview of the problem), but it is a problem for other optical instruments. In particular, larger particles, such as those greater than about 0.5 mm, which were likely present in these clouds, often break into fragments, and these fragments are likely to contribute significantly to the 100 to 500 micron-mode particles

which are a major topic of this paper. Without an understanding of the shattering and breakup of particles in the instrument, it is impossible to determine if the observed "self similarities" of particles in the 100 to 500 micron mode are real features of the clouds or instrumental artifacts of the sampling in conditions where large ice particles are present. This is also a subject of part II of this study (Ackerman et al., 2015), so this is a particularly relevant concern for these papers.

4. As the author's point out, the uncertainties in the Robust probe severely limit the interpretation of the collected data. The authors also make an assumption that liquid water contributions at temperatures below -20 °C are negligible. While that may be true for many cloud conditions (such as the stratiform regions studied in sections 4 and 5), it is a doubtful assumption for deep convection, where several studies have documented the importance of homogeneous freezing. This assumption further adds to the uncertainties of this study which could be reduced by partitioning of the data into convective and stratiform regions and perhaps using different assumptions for the two cases.

5. The Locatelli-Hobbs relationship (eq. 2), might not be the best choice for these clouds, compared to, for example, the Baker and Lawson (2006) area-mass relationship. It would be helpful to have an explanation of why eq 2 was chosen over other methods and a better explanation of the uncertainties in computed mass content associated with these types of assumptions would certainly be worth considering for the revised paper. As the author's point out, their IWC measurements are roughly a factor of two greater than measurements documented in the scientific literature to date, so it is important for the authors to demonstrate why they believe their measurements offer an improvement over previously reported IWC measurements in similar clouds.

6. The paper could be improved by including more information on the ice particle morphology. The nephelometer appears to provide excellent imagery of ice particles (e.g. as in Figure 1, Ackerman et al., 2015). This type of imagery has traditionally been used together with size distributions to explain the microphysical characteristics of ice,

yet the authors have not utilized this technique, which might offer significant insights into the nature of the ice environments that were sampled.

Minor Comments. Line 51-2. “industry concluded..” This seems out of place without a reference.

Section 7 first sentence. “power less” should read “power loss”.

References.

Ackerman, A. S., Fridlind, A. M., Grandin, A., Dezitter, F., Weber, M., Strapp, J. W., and Korolev, A. V.: High ice water content at low radar reflectivity near deep convection – Part 2: Evaluation of microphysical pathways in updraft parcel simulations, Atmos. Chem. Phys. Discuss., 15, 16551-16613, doi:10.5194/acpd-15-16551-2015, 2015.

Baker, B. A., and R. P. Lawson: Improvement in determination of ice water content from two-dimensional particle imagery. Part I: Image-to-mass relationships. J. Appl. Meteor. Climatol., 45, 1282–1290, 2006

FAA, Advisory Circular 20-147A, U. S. Government Printing Office. 2014

Jackson, R. C, G. M. McFarquhar, J. Stith, M. Beals, R. A. Shaw, J. Jensen, J. Fugal, and A. Korolev: An Assessment of the Impact of Antishattering Tips and Artifact Removal Techniques on Cloud Ice Size Distributions Measured by the 2D Cloud Probe. J. Atmos. Oceanic Technol., 31, 2567–2590.doi: http://dx.doi.org/10.1175/JTECH-D-13-00239.1, 2014

Lawson, R. P., E. Jensen, D. L. Mitchell, B. Baker, Q. Mo, and B. Pilson, Microphysical and radiative properties of tropical clouds investigated in TC4 and NAMMA., 2010: J. Geophys. Res., 115, D00J08, doi:10.1029/2009JD013017

Roques, S., An airborne icing characterization probe: nephelometer prototype, Smart Mater. Struct., 16, 1784-1788, doi:10.1088/0964-1726/16/5/032, 2007

Acknowledgment: The National Center for Atmospheric Research is sponsored by the

National Science Foundation. Any opinions, findings and conclusions or recommendations expressed in this review are those of the author and do not necessarily reflect the view of the National Science Foundation.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 16505, 2015.