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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.

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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). Second,

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.

General response: We thank the reviewer for his time and comments. We fully agree that the conditions that result in engine power loss are not adequately known (see response 1 below) and that our ability to measure ice water content is inadequate (already discussed at length in the introduction). More detailed information is provided per point-by-point responses below.

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.

Response 1: It is impossible to name such an IWC amount at least in part because it is "impossible to rule out the sufficiency of either long or short duration exposure events" (see p. 16509, line 26); it is also beyond the scope of this paper to discuss the many factors that affect engine response to ice ingestion, which may vary with engine type, operating conditions, and sequence of exposure. We can, however, usefully add the context of appendix D, revision to p. 16514, line 13: "As described by Grandin et al. (2014), an Airbus objective was to obtain preliminary measurements to evaluate newly proposed standards for engine performance in glaciated icing conditions (Mazzawy and Strapp, 2007). Conditions sampled included..."

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).

Response 2a: Equation 1 is correctly transcribed from the first relations shown in Tables 1 and 2 of Baker and Lawson (2006), but we stand corrected re Lawson et al. (2010). Correction to p. 16512, lines 12–16: "mass-dimension" should be "area-dimensional" and delete remainder of sentence and equation after "$D_{\max}$". Clarification to Fig. 1 caption: replace "following Eq. (1) (Baker and Lawson, 2006)" with "following Baker and Lawson (2006, their Table 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.

Response 2b: Re classification, please see response 1 to reviewer 2. Safety was not a leading concern during sample legs with this aircraft at the elevations and locations

targeted. On p. 16514, line 17: ", within the limits of safety" to be deleted.

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).

Response 3.1: For the lead authors of this manuscript it is a new experience to use data from a proprietary industry instrument for publicly funded research science. We agree that the level of documentation does not match that of most airborne cloud physics instruments, but do not believe that gap can be filled with a few sentences here. See response 3.2 for additional references reporting this data. See response 3.3 for reference to new measurements from more widely used instruments.

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.

Response 3.2: Following on response 3.1, at p. 16515, line 13: replace "(Roques, 2007)" with "(Roques, 2007; Dezitter et al., 2013; Grandin et al., 2014)".

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.

Response 3.3: Owing to the objective of investigating IWC and Z_e, here we are able to focus analysis on mass-weighted diameter measures, where shattering effects are relatively reduced. Also following response 3.1, to be added at p. 16520, line 18: "Leroy et al. (2015) more recently report $\mathrm{MMD_{eq}}$ typically 250–500 μm and weakly decreasing with increasing IWC over 0.5–3 g m^{-3} at -36°C in a system extensively sampled near Darwin during the recent High Altitude Ice Crystals / High Ice Water Content campaign, but similar measurements in another system yielding typical $\mathrm{MMD_{eq}}$ of 400–600 μm instead weakly increasing with IWC. Aside, we note that shattering artifacts that may contaminate airborne probe measurements are relatively reduced for higher-order moments of the size distribution such as mass [Korolev et al., 2013; Jackson and McFarquhar, 2014]. Since the Korolev et al. [2013] study was performed for probes with different inlet configurations, we expect that that general conclusion can be extended to the Airbus nephelometer. It has been found that size distribution measures such as MMDmax may be subject to roughly 20% uncertainty owing to shattering artifacts, for instance [Jackson and McFarquhar, 2014]."

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.

Response 3.4: Clarification re PSD measurements used in Fig. 6 and all later analysis to be added at p. 16519, line 16: "Although Robust probe data are available at warmer temperatures (see Fig. 2), we note that Cayenne and Darwin size distributions are available only for flight legs at temperatures colder than -40°C (e.g., see Fig. 5), aside from several short segments of flight 1422 reaching -33°C, which are not qualitatively different." Also at p. 16516, line 13: "here we consider only measurements taken at temperatures colder than -20°C, where liquid contributions are considered negligible" to be replaced with "in Fig. 2 we include only measurements taken at temperatures colder than -20°C, where airframe icing was non-existent or negligible; size distribution measurements are limited to temperatures colder than -33°C, as discussed further below."

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.

Response 3.5: Clarification re choice of eq. 2 to be added at p. 16517, line 24: "assuming a widely used relationship" to replace "assuming a relationship". Clarification re associated uncertainty to be added at p. 16518, line 13: "As discussed above, roughly a factor of two uncertainty in calculated IWC may be associated with uncertainty in the validity of Eqn. 1 or another such relationship (e.g., McFarquhar and Heymsfield, 1986)." We do not believe that these measurements offer an improvement over previously reported IWC measurements, but rather that "a large database of such measurements is not yet available" (see p. 16528, line 20).

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.

Response 3.6: Clarification to be added at p. 16517, line 19: "While capped columns are commonly present (see Part II), the majority of crystals appear irregular, as found in CEPEX anvils (cf. McFarquhar and Heymsfield, 1996), and the nephelometer images do not commonly produce images of sufficient clarity to distinguish rime or other morphological details." We consider our current approach adequate because IWC calculated from the size distributions without habit-dependent analysis agrees within uncertainty of Robust probe IWC over an order of magnitude in dynamic range, as shown in Figure 5.

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

Response: Reference to be added: "(e.g., Dezitter et al. 2013)".

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

Response: Correction to be made.

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