

Interactive comment on “On the importance of small ice crystals in tropical anvil cirrus” by E. J. Jensen et al.

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Response to Anonymous Referee #2

p. 5324 I. 28. Please provide references and include for example the conclusions in work by Roskovensky et al. (2004) and Davis et al. (2009).

The Roskovensky et al. (2004) paper described comparisons between MODIS optical depth retrievals and CAPS (as well as CIN) measurements of thin cirrus. We do not think this paper is relevant for the discussion in this manuscript for two reasons: (1) As discussed further below, we do not believe that comparison with remote-sensing retrievals is an appropriate way to demonstrate that in situ measurements are accurate because there are numerous uncertainties associated with remote-sensing retrievals

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that are difficult to quantify; and (2) in thin cirrus, large crystals are predominantly absent, and shattering artifacts are potentially negligible.

We agree that the conclusions of the Davis et al. (2009) paper are relevant and should be included. We have thus added a discussion of the Davis et al. study in the introduction.

p. 5325 I. 2. In liquid clouds the large mode and small mode are highly correlated also due to purely physical reasons. Can physical reasons be ruled out for ice clouds?

In clouds with vigorous vertical mixing, gravitational size sorting might be overwhelmed by dynamical motions, possibly resulting in the correlation mentioned in liquid clouds. As described in the manuscript, in stratiform cirrus clouds, the physical expectation is that gravitational sorting should result in regions with abundant large crystals and relatively few small crystals. The TC4 2D-S data shows that such regions do exist.

p. 5326 I. 5 Please site the rebuttal and associated conclusions by Garrett (2007) and Gerber (2007).

The debate between Heymsfield (2007) and the rebuttal papers mentioned primarily concerned the accuracy of the Cloud Integrating Nephelometer (CIN) instrument. It is not the purpose of this manuscript to address the accuracy of CIN (we have removed any mention of this instrument from the revised manuscript), and therefore we do not think it is appropriate to rehash the debate between these papers.

p. 5328 What were the conclusions of Miloshevich and Heymsfield (1997) regarding the concentrations of small ice crystals?

Miloshevich and Heymsfield (1997) did not discuss the concentration of small particles in cirrus. This paper was focused on the design and performance of the VIPS instrument.

p. 5330 The 2D-S probe is described as a cousin of the CIP probe which itself is

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related to the 2D-C probe. The 2D-C probe was well-known to be characterized by a rapidly decreasing and poorly characterized sample volume for smaller sizes. Since small particles are the focus of this work, please describe the improvements in the 2D-S in this area as well as remaining uncertainties. Remaining concerns are briefly alluded to on p. 5332 l. 12, but all uncertainties in all instruments should be estimated for the sake of intercomparisons. If there are uncertainties that are not yet quantified, they should be stated up front in the instrumentation section along with the discussion of the unknown uncertainties associated with shattering. For example, concentration errors in FSSP-type probes have been characterized in numerous papers (e.g. Baumgardner et al., 1992) and should be discussed. Shattering errors remain to be quantified. Have errors with the 2D-S probe been quantified or not? What are they?

We agree that additional detail on the uncertainties in 2D-S small-crystal measurements should be added. The following discussion is included in the revised manuscript: "The uncertainties in 2D-S determination of particle size are described in detail by Lawson et al. (2008) and Korolev (2007). With regard to uncertainties in ice concentration, it should be noted that the 2D-S represents an improvement over CIP and 2D-C probes in that faster electronics and smaller pixel size are used. Both of these improvements should result in improved accuracy of small-crystal concentration measurements. It has been demonstrated that with the faster electronics the 2D-S can detect and count small particles at airspeeds up to $\approx 230 \text{ m s}^{-1}$. Likewise, the smaller pixel size allows detection of crystals that might have been missed by the CIP and 2D-C probes with larger pixel size.

The 2D-S probe shares the problem of rapidly decreasing sample volume with decreasing particle size that CIP suffers from, and this decreasing sample volume is accounted for in the same way for each of the probes. A thorough investigation of the errors associated with depth of field effects on sample volume and corrections for out of focus particles has not been done. Therefore it is difficult to quantify the uncertainties in 2D-S

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measurements of small-crystal concentrations."

p. 5333 l. 10 Are there no other explanations that can be provided for the observed correlation? Correlation never implies causation, although it can provide support for a hypothesis, particularly in the absence of other plausible explanations.

As stated later in the manuscript, there is no reason to expect that an undercounting of small-particles by the 2D-S probe would result in such a correlation with large-crystal mass. As we state the case in the revised manuscript, if shattering on the CAS probe is responsible for the CAS/2D-S discrepancy, one would expect the correlation seen in the data. One can never rule out the possibility that there is something going on that nobody has thought of. However, shattering artifacts are entirely expected, they are readily apparent in interarrival time distributions (for instruments that measure interarrival times), and shattering artifacts on CAS would seem to be a plausible explanation for the observed correlation.

p. 5333 l. 15. Is the increase in the correlation coefficient statistically significant given the effective sample size corrected for serial auto-correlation in the data set?

Rather than detract from the flow of the manuscript with a discussion of statistical significance of the difference between correlation coefficients at different airspeeds, we have simply removed this sentence. In any case, the increase in shattering artifacts with airspeed might be weak so long as the airspeed is above some threshold, and the dependence would likely be muddled by a number of other factors such as crystal size distribution and habits.

p. 5334 l. 1 Can crystal fragmentation during gravitational settling or mixing with partially evaporated ice crystals be ruled out (Zender and Kiehl, 1994; Bacon et al., 1998)?

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We have included a comment that breakup of sublimating crystals can produce fragments and a citation to Bacon et al. (1998). We are not aware of any laboratory experiments suggesting that large numbers of small crystal fragments can be produced by large crystal collisions or during evaporation. It seems much more likely that an inlet impacting a large crystal at $\simeq 200 \text{ m s}^{-1}$ will produce large numbers of small fragments.

p. 5337 The CAPS measurements from CRYSTAL-FACE and MidCIX were evaluated by comparisons with IWC and extinction probes (Davis et al., 2007; Garrett et al., 2005; Garrett, 2007), and all of these probes were evaluated using independent remote sensing methods (Roskovensky et al., 2004; Garrett et al., 2005; Noel et al., 2007; Davis et al., 2009) using a variety of techniques based on CERES, MODIS, and CALIOP. Please place the conclusions described on this page within the context of the quantitative conclusions provided by these studies.

As discussed above, we have included a discussion of the results from Davis et al. (2007). Comparisons between CAPS measurements of IWC and bulk IWC measurements are not relevant since small particles likely don't contribute much to IWC. As discussed above, Garrett (2007) was focused on accuracy issues involving the CIN instrument, which is not the subject of this study. As also mentioned above, Roskovensky et al. (2004) focused on thin cirrus, for which shattering was likely not an issue. Noel et al. (2007) showed comparisons derived from lidar retrievals and CIN; again, we are not addressing the accuracy of CIN in this manuscript. Lastly, as discussed further below and commented on in the revised manuscript, we do not believe that comparisons with remote-sensing retrievals are a valid way to determine the accuracy of in situ measurements.

p. 5339 The discussion about aerosol on p. 5339 is very interesting but it appears to distract from the main theme of the paper.

In the "Implications..." section, the discussion of how the conclusions of this manuscript affect the results from the Fridlind et al. (2004) paper is an important component.

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It seems appropriate at this point to provide a balanced discussion of the evidence for/against boundary-layer aerosols affecting anvil cirrus. To our knowledge, such a discussion has not appeared in the literature previously.

p. 5343. 1st paragraph. The discussion here is not entirely convincing for two reasons. First the relevant time scale for fresh nucleation would be the period associated with the observed waves. Can the period be estimated? If it was close to the Brunt-Vaisala frequency then crystals would evaporate and be replenished on similar timescales. If it was much slower than they wouldn't. Second, the most recent studies of the deposition coefficient for cirrus ice crystals by Magee et al. (2006) point to values much lower than the values used in Fig. 11. These calculations should be included in Fig. 11 and it should be acknowledged that while the absence of small crystals in 2D-S measurements could mean that the Magee et al. (2006) results are in error, as is stated, the reverse could also be true.

The first argument given here does not seem relevant for discussion at hand. We are addressing the issue of persistence of ice crystals detrained into anvil cirrus by deep convection. Also, a gravity wave would not necessarily produce ice crystals that would appear and disappear with the periodic temperature oscillations of the wave. Since there is a supersaturation barrier to ice nucleation, the wave could just provide sufficient cooling to trigger ice nucleation, and the air could remain supersaturated in the warm phase of the wave.

Although we believe there is mounting evidence that the very low deposition coefficients reported by Magee et al. (2006) are inconsistent with field observations of cirrus (e.g., Comstock et al. (2008), which is discussed in the revised manuscript), we have included curves in Figure 11 based on the low deposition coefficients. We have reworded the conclusion of this paragraph to simply state that deposition coefficients < 0.1 are inconsistent with the observations presented here.

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Fig. 14. It is a bit difficult to reconcile the results in Fig. 14 with those in Figure 8. What would make this clearer is plotting the y-axis in Fig. 14 with respect to height, plotting β_{ext} rather than SAD in Figure 8 and including a plot of the estimated accumulated β_{ext} versus height next to the profile of forcing (or at least the estimated optical depth).

The intent here was not to provide a detailed analysis of the radiative properties of the anvil cirrus. That would be beyond the scope of the manuscript, and considerably more detail would be required. Rather, we simply wanted to show that the small crystals have a negligible impact on the radiative properties of the clouds. We believe the figures and discussion make this point clearly.

Fig 14. From what I can guess, the optical depth is about 6 based on the SAD measurements and assuming a cloud depth of 3 km. If true, the anvil must be very aged, as the sun's disk would just be visible through such a cloud, and that is not normal experience given that anvils normally look dark from underneath. If not for this case, then at least for the case in Fig. 16, a rough comparison should be provided comparing space or MAS -based retrievals of optical depth with those derived from the 2D-S. Is there rough consistency? Since effective radius imagery is provided as rough justification for the 2D-S measurements, optical depth estimates should be provided also, particularly as it is on optical density that the described forcing and heating rates primarily depend.

The intent of this manuscript was not to do a detailed comparison between remote-sensing retrievals and the in situ measurements. That would be beyond the scope of the manuscript, and considerably more detail would be required. A separate paper on comparison between MAS retrievals and TC4 in situ measurements is in preparation. To be clear, we did not intend to lend credence to the 2D-S measurements based on comparisons between MAS retrievals. We feel this is an inappropriate approach to validating in situ measurements. There are numerous sources of uncertainty in the remote-sensing retrievals, and these uncertainties are difficult to quantify. In fact,

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much of the motivation for TC4 and other recent airborne-science field experiments has been using in situ measurements to evaluate remote-sensing retrievals (not the other way around). We have added caveats to the manuscript to make it clear that the agreement or disagreement with remote-sensing retrievals does not necessarily imply that the in situ measurements are correct or incorrect.

p. 5348 l. 14 I believe the probe modified by Knollenberg was an FSSP not a 2D-C. Please describe the modifications and why they may not have been contaminated by artifacts.

According to Knollenberg's paper, it was a modified 2D-C. From Page 8640:

"Prior to this work we had measurement capabilities for aerosol from 0.1 to 3.0 μm using an active scattering aerosol spectrometer probe (ASAS-X) and a two-dimensional Grey imaging probe for large precipitation-type particles greater than 40- μm size. The lack of measurement capability between 3 and 40 μm had to be rectified since the tops of tropical cumulonimbus anvils often concentrate ice crystal mass in this size range. To fill this gap, we modified the two-dimensional probe to include a light scattering spectrometer subrange with a nominal 2- to 100- μm size range. Because of the limitations in mounting locations a standard light scattering spectrometer probe (model FSSP-100 manufactured by PMS) covering a 3- to 45- μm size range and often used in place of the two-dimensional probe could not be flown simultaneously with the two-dimensional probe."

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5321, 2009.

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