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Interactive Comment

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

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I recommend the following issues be addressed in a revised manuscript

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

p. 5325 l. 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?

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

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

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

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.

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?

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)?

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.

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

Eq. 1 How is IWC and β_{ext} calculated? The link between particle diameter and mass is not straightforward, and the link between β_{ext} and SAD is not stated. Since the discussion in this section is primarily focused on radiation, it would facilitate interpretation to use the radiative property β_{ext} rather than the physical property SAD for Figures 7 to 9

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.

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).

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

ACPD

9, C149–C153, 2009

Interactive Comment

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.

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.

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