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Comment

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

**E. Jensen**

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### **Response to Anonymous Referee #2**

We appreciate the helpful comments and suggestions. Addressing these comments has improved the manuscript. Specific responses below.

**p. 5324, line 24: Change "...small crystals are indicated throughout the clouds (...)** (Garret et al, 2005)" **to say something to the effect of "... small crystals are indicated throughout the clouds ... (Garret et al, 2005), which would not be expected based on physical grounds because ... (ref)". The authors give a physical explanation of why clouds should have variable populations of small crystals (e.g., sedimentation, sublimation, deposition) later in the paper, but it**

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would be nice to have this up front. I was confused until later in the paper when I read the explanation.

Done.

**p. 5324, sentence beginning on line 20, and sentence beginning on line 25: This argument does not support the questioning of the CAPS measurements on "physical plausibility grounds", as the paragraph states. It relies on believing the remote-sensing measurements to claim that the values derived from CAPS are unreasonably small. I would recommend changing the opening sentence of the paragraph to "The measurements of abundant small crystals have been questioned for several reasons". I do agree that it is cause for questioning both the in situ and remotely sensed measurements.**

Agreed. We have changed the opening sentence of the paragraph as suggested.

**p. 5324, line 28: Provide reference for reff comparisons between CAPS and remote sensing measurements.**

We are not aware of a publication specifically discussing comparisons between MODIS and CAPS effective radii for anvil cirrus. However, the MODIS effective radii for anvil cirrus are typically 30–40 microns. We have added a reference to support this statement.

**p. 5325, line 11: It would be useful if you would define up front what "large concentrations of small crystals" means. A definition based on the number concentration in a given size interval or based on a certain percentage contribution to extinction seems to make the most sense.**

Agreed. After "large concentrations", we have added ( $> 1 \text{ cm}^{-3}$  crystals smaller than  $\approx 50 \mu\text{m}$ ).

**p. 5326, line 10: Change "surface area density" to "surface area density (i.e., extinction) from CIN"**

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Done.

**p. 5326, paragraph starting on line 17: It seems as though this paragraph is implying that CVIs are subject to particle shattering because studies using CVIs reported large small-crystal concentrations. Is that the point? Or is the point to simply state that some studies have observed large amounts of small ice? Please make the point of this paragraph more clear. If you are claiming that CVIs are affected by shattering, that would seem to contradict the results of Heymsfield et al. (2007).**

We have added the sentence "CVI measurements of ice concentration can be subject to shattering artifacts if multiple shattering fragments contain sufficient soluble or insoluble material to produce detectable residual particles." to clarify that we are indeed implying that CVI measurements of ice concentration can be subject to shattering artifacts. Some investigators using CVIs do not report ice concentrations because of concerns about shattering artifacts. Heymsfield (2007) only used CVI measurements of ice water content, which should not be affected by shattering artifacts.

**p. 5327, paragraph starting on line 1: This paragraph needs to make clear the fact that the implications of a CAPS bias in small particles may change the conclusions of the Fridlind study. You expound on this on page 53338, so you should mention something to the effect that a CAS bias may affect the Fridlind results.**

Done.

**p. 5327, line 10: Here, or at some other point in the paper perhaps, you should show quantitatively or at least in more detail why probes with arms are less likely to be susceptible to shattering than those with inlets. It is not obvious from the information you have provided. Are there flow-modeling papers that support this statement? Is the physical edge of the arm expected to produce less shattering than FSSP-type inlet surfaces (i.e., is there a smaller subtended cross-section)? Or are you simply relying on the fact that for the instruments with arms, the**

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**shattered fragments have to travel a further distance perpendicular to the flow (i.e., from the arm to the center of the detection volume) to be counted by the detector, whereas for FSSP-type instruments effectively half of any shattered fragments will enter the instrument and be counted?**

We are not aware of any flow-modeling studies that directly compare instruments like 2D-S with FSSP-type instruments with inlets. However, as the referee suggests and as we discuss in the revised manuscript, the 2D-S protruding surfaces (detector arms) are indeed further away from the sample volume than is the case for the CAS inlet. Also, as shown in Figure 1, 2D-S has sharp discs mounted on the interiors of the detector arms. These discs are intended to deflect shattering artifacts out of the sample volume.

**p. 5327, line 24: Use of affects and effects in the same sentence is awkward. I recommend you remove the word effects.**

Corrected.

**p. 5328, line 11: Remove the sentence beginning with "As shown by Heymsfield ...". It is irrelevant to this paragraph, and it has already been mentioned twice in the introduction.**

Done.

**p. 5328, line 19-21: A general comment concerning the statement "... the relative enhancement of the small-crystal concentration is dependent on the natural concentration of small ice crystals". From reading the paper, I don't understand why the relative enhancement of small crystals by CAS should be dependent on the number of small crystals present. You explain well why the enhancement depends on the number of large crystals, but don't provide adequate explanation of why it should also depend on the number of small crystals present. Is this a counting or dead-time issue?**

We have removed the statement "...the relative enhancement of the small-crystal con-

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centration is dependent on the natural concentration of small ice crystals" at this location because it is out of place. Later in the paper, we go into greater detail, providing a clear reasoning why the relative enhancement of small-crystal concentration should depend on the natural small-crystal concentration.

**p. 5329, sentence beginning on line 27: See comment above (p. 5327, line 10). A better explanation is needed here.**

See response to comment above.

**p. 5330, paragraph beginning on line 21: Where was the 2D-S mounted on the DC-8? For that matter, where were the CAPS instruments mounted on the DC-8 and WB-57? Please add information to this paragraph.**

As described in the revised manuscript, on the DC-8 2D-S and CAPS were mounted on wing pylons that put them well away from the wing.

**p. 5332, line 2: Are there any fluid modeling calculations or wind tunnel experiments to support this claim? There has been some work on flow modeling around the WB-57, but I don't know whether it addresses this issue adequately.**

The question of whether shattering artifacts from the wing leading edge could cross the streamlines and reach the 2D-S sample volume is difficult to answer. We examined the flow calculations done for particles bouncing off the airframe, but there are numerous uncertainties involving (for example), the directions and speeds of shattering artifacts. We decided that we cannot rule out the possibility that shattering artifacts reach the 2D-S sample volume, and therefore we just presented the data and interpretation as if this were a possibility.

**p. 5332, line 9: Change "more large crystals present" to "more large crystals are present"**

Done.

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**p. 5332, sentence beginning on line 20: See related comment above**

We have added the following text to clarify this issue:

"A simple example will explain why this result is expected: Assume that a fixed mass of crystals large enough to generate shattering artifacts produces  $1 \text{ cm}^{-3}$  enhancement in the measured small-crystal concentration. If the natural concentration of small crystals were  $0.1 \text{ cm}^{-3}$ , then the relative enhancement caused by shattering would be large (about an order of magnitude); whereas if the natural concentration of small crystals were greater than  $1 \text{ cm}^{-3}$ , then the relative impact of shattering on measured ice concentration would be less important (less than a factor of 2)."

**p. 5332, line 21: Change "does" to "do"****p. 5333, line 28: Change "visa" to "vice"****p. 5334, line 10: Should also cite McFarquhar et al (2007) here.**

Done.

**General comment, section 2.3: This section either needs more work, should be removed, or should be renamed "Quantifying shattering artifacts in TC-4 CAS data". If the point is to develop a CAS correction that can be applied to data from other missions, then that should be stated more clearly. Related to this, it doesn't make much sense to compare CAS concentrations with 2D-S IWCs (as in Figure 6), because you've effectively already conveyed this information in Figures 4-5. If you want to include Figure 6, it would make more sense to compare the CAS concentrations with the CIP IWC, since the 2D-S is presumably not present in other missions, and therefore any CAS corrections needs to be able to rely on the CIP.**

**It would be even more interesting if you came up with a CAPS correction using only the CIP, applied it, and compared the resulting distributions to 2D-S using the TC-4 data. If a reliable correction could be made for the CAPS data that would**

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**be applicable to past missions, this would be immensely important.**

This is a reasonable comment, and we agree that it would be preferable to use CIP for the IWC rather than 2D-S. However, it would be a considerable amount of work to redo the analysis using CIP, and we argue that it is not important for two reasons: First, the purposes of presenting these correlations are (1) to show how tight the correlation is when spatially uniform cloud segments are used, and (2) to provide a qualitative guide to estimating when shattering artifacts might be important for CAS measurements. As further emphasized in the revised manuscript, we do not intend these correlations to be used as quantitative estimates of the impact of shattering.

**p. 5336, line 12: A general comment. You should pick a unit for concentration and stick with it throughout the paper. Mostly, you use cm<sup>-3</sup>, but in this line you use m<sup>-3</sup>. A few other places you use L<sup>-1</sup>. I recommend you just use cm<sup>-3</sup>.**

We exclusively use cm<sup>-3</sup> in the revised manuscript.

**p. 5336, line 24: Change "quatitative" to quantitative**

Done.

**p. 5337, line 12: Change "we suggest that importance" to "we suggest that the importance"**

Done.

**p. 5337, line 19: Define (and cite, if possible) MIDCIX**

Done.

**p. 5341, line 24, and p. 5342, line 19: For comparison, what are the values of the CAPS reff?**

At this point in the paper, we are focusing on the 2D-S measurements. The structure of the paper was to start with comparisons and instrument evaluations, then to evalu-

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ate the importance of small crystals based on the measurements deemed to be most reliable.

**p. 5346, line 12: Change "The correlation indicated by the CAPS data is exactly what you would expect ..." to "The correlation indicated by the CAPS data is to be expected ..."**

Done.

**p. 5347, line 20: Change 100 L-1 to consistent units. cm-3 recommended.**

Done.

**p. 5348, line 16: Remove s in "10 s cm-3"**

Done.

**p. 5348, line 28: change and to at**

Done.

**Figure 2: You should include WB-57 CAPS data here, and in the discussion in the text. Also, make abscissa consistent between Figures 2 and 3. Are they both maximum dimension?**

At the time of writing (18 months after the end of the TC4 mission), final data from the WB-57 CAPS instrument was not available. We have changed the abscissa title on Figure 2 to "Maximum dimension" for consistency with Figure 3.

**Figure 4: To be clear, this plot or caption should mention that it is DC-8 data. Also, you should include WB-57 data here, even if the correlation is not as good. I presume the scatterplot data would appear more flat if the WB-57 2D-S is subject to undetected shattering, but that would provide evidence for your point.**

We now state that Figure 4 is DC-8 data. As discussed above, the WB-57 CAPS data was unavailable.

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**Figure 5: You should include a 4th panel with the WB-57 CAPS data, for completeness.**

See response above.

**Figure 7: Caption says "white trace", but the flight path through the CPL/CRS data is a black trace.**

We have corrected the caption.

**Figure 14: Mark cloud boundaries with horizontal lines. Also, for comparison with Figure 12 it would be useful to include height (km) as the axis on the right.**

It seems clear from examination of Figure 14 where the cloud boundaries are (where the heating rates go to near zero). Therefore, marking the cloud boundaries and including a height scale seem unnecessary.

**Figure 16: Plot flight track on MAS image.**

We have added the flight track. In the process of doing so, we discovered an error in the time of the MAS image. With the corrected MAS image, we have changed the discussion in the text. See the response to referee #1 for details.

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

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