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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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This paper addresses the quality of aircraft particle probe measurements in cirrus clouds and the implications for climate-relevant effects of cirrus. It is an important contribution to this topic, and is relevant to the interests of ACP. This paper should be accepted for publication in ACP after the following comments have been adequately addressed.

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 would be nice to have this upfront. I

was confused until later in the paper when I read the explanation.

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.

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

p. 5325, line 11: It would be useful if you would define upfront 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.

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

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 CVI's are affected by shattering, that would seem to contradict the results of Heymsfield et al. (2007).

p. 5327, paragraph staring 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 Fridland study. You expound on this on page 53338, so you should mention something to the effect that a CAS bias may affect the Fridland results.

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 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?

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

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.

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?

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

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.

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.

p. 5332, line 9: Change “more large crystals present” to “more large crystals are present”

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

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.

General comment, secition 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 IWC’s (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 be applicable to past missions, this would be immensely important.

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-3, but in this line you use m-3. A few other places you use L-!. I recommend you just use cm-3.

p. 5336, line 24: Change “quatitative” to quantitative

p. 5337, line 12: Change “we suggest that importance” to “we suggest that the importance”

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

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

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 . . .”

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

p. 5348, line 16: Remove s in “10 s cm-3”

p. 5348, line 28: change and to at

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

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.

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

Figure 6: IWC units are incorrect

Figure 7: Caption says “white trace”, but the flight path through the CPL/CRS data is a

black trace.

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.

Figure 16: Plot flight track on MAS image.

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