Atmos. Chem. Phys. Discuss., 7, S428–S432, 2007
www.atmos-chem-phys-discuss.net/7/S428/2007/

Atmospheric Chemistry and Physics Discussions

ACPD

7, S428–S432, 2007

Interactive Comment

Interactive comment on "Observing cirrus halos to constrain in-situ measurements of ice crystal size" *by* T. J. Garrett et al.

T. J. Garrett et al.

Received and published: 5 March 2007

Response to reviewer 3

Main comments

1. Some clarification may be needed here in the text. Halos were observed about one half of the time when the plane was in cloud and near cloud top (p. 12, par 1). The back-seater photographed halos only a small fraction of the time, and it is only these times that were studied in detail, because it is only for these times that we can state with confidence the presence or absence of optical effects. Times when halos were not explicitly noted may still have displayed halos. However, even if halos were not observed, this might have been for many reasons, includ-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

ing that the optical depth between the airplane and the halo angle was either too low or too high (Section 2.2). Because clouds are highly variable in their density distributions, it would be very difficult to interpret even explicitly noted halo absence, even where crystal shape and size are measured well: a halo may be produced, but be too faint to be observed, or washed out by multiple-scattering. Essentially, halo presence is much more narrowly constrained than halo absence, so given the information we have, we are forced to focus on the former.

2. See our response to Point 1. We have an added problem however, which is that we lacked measurements of the habits of ice crystals about 20 μm across. The CPI, which was on the plane, simply lacks the resolution required. We could show the habits of larger crystals, but this would not add to the paper if the habits of smaller crystals could not be shown as well.

3. We are doing order of magnitude comparisons between the small and large mode, so the conclusions we present are insensitive to the exact method employed for processing the CIP data, within reason of course. Nonetheless, we will describe the method used better in the text. Specifically, that we dealt with out-of-focus particles by assuming that photo-diode array occultation is approximately conserved. A “donut-hole” may be placed in the center of the crystal, but also the crystal is spread out over a larger area. These are two compensating errors.

4. We have done the most extreme form of error analysis possible, which is to evaluate the plausibility of the results under an assumption that the small mode does not exist.

5. No cloud is homogeneous, and barring lidar and radar, an assumption of some form of vertical homogeneity is central to remote-sensing algorithms as well.

6. Statistical representation is present. Figure 7 shows data from the entire MidCiX project, including the explicitly-noted halo times, but not just for the explicitly-

noted halo times. We will make this more clear in the revision and describe the length of time Figure 7 represents.

Other comments

1. But the term "ice water content" is jargon and ambiguous. We will include both.

2. The original definition of effective radius is actually from Stephens (1978) or perhaps Hansen and Travis (1974). Either way, equation 1 is consistent with Foot's definition, and is general for all shapes. Fu's definition is a poor choice because it presumes hexagonal prism morphology. Eq 1 reflects revised definitions now assumed in, for example, MODIS processing of ice clouds.

3. No comment 3

4. By definition, refraction can be described using ray-tracing.

5. Yes, we agree this might be worth adding at some point. We suspect that cloud thickness is a dominant control of the visibility of halos, which of course is independent of habit considerations. This is largely ignored in most halo discussions, as the reviewer notes.

6. The halos noted and photographed by the back-seater are not consistent with orientation, because they do display, for example, the parhelic circle. But it is fair that it should be discussed in Section 4 whether the absence of 46 degree halos could be due to perturbations to the crystal basal face.

7. We acknowledge as much in the first paragraph of Section 2.1.2, and use the phrase "not favored" rather than "excluded" in the summary.

ACPD

7, S428–S432, 2007

8. The conclusions are insensitive to the details of CIN processing becasue the fraction of light missed by the CIN is narrowly constrained due to some very basic physics (e.g. Babinet's principle). It is an interesting point what the values of the asymmetry parameter were during previous studies. During the Arctic cirrus studies, g was low, and halos were consistently observed. The halos observed then were milky white. This was interpreted as indicating crystal imperfections. However, in hindsight this may be wrong. The aircraft used in the Arctic could not reach cirrus tops. It might have been that the halos were milky white simply because of multiple-scattering effects. Regardless, the discrepancy between g from the CIN and those from theoretical calculation is important, but not so big as to affect the very approximate estimates we found of an upper optical depth limit to halo formation implied by asymptotic theory.

9. We corrected the CIP imagery as described above. The conclusions are insensitive to the exact processing algorithm. Figure 4 indicates the size distribution is bi-modal, independent of likely errors in the first few channels of the CIP. Remember, we have done an extreme error analysis, by examining the plausibility of the CIP data being the only contributors to measured extinction, and found this was implausible. No reasonable adjustment to the CIP data would change the paper's basic conclusion.

10. This is for the entire project's measurement's of cirrus. We will include this information in the revision.

11. The Nevzorov probe is not included in the paper by Davis et al. It did underestimate, yes.

12. We will provide this information, but the data set is large compared to the variability in the normalized size distributions, so that the results presented should be very representative of the cirrus cloud sampled in general.

13. Again, the halos were present even when they weren't explicitly noted. Nonetheless, a rudimentary statistical comparison is a good suggestion. Certainly Figure 3 shows the mean values are very close to being the same. The spread is different, but halos are not visible in either cloud that is too thick or too thin.

14. This is addressed in the foot-note to Section 2.1.1

15. I think this is an interesting question. The Arctic cirrus described in Garrett et al. (2001) may have had some important differences to those studied here, where the air was moister, and coming from the Gulf of Mexico primarily. Possibly lower updraft velocities in the Arctic nucleated fewer, larger, and lower extinction ice crystals in the small mode. There was no meaningful upgrade to the CIN that should have affected the results. Consistency between FSSP type probes is low, even where the working principle is the same. It's hard to say why the Arctic and MidCiX cirrus measurements were different, without resorting to much speculation. We were careful in our conclusions to state only that, for cirrus in general, the optical dominance of small crystals near cloud tops cannot be precluded.

16. All measurements. This will be indicated.

17. The parameterization is for all data, not just when halos were observed. Halo phenomena may be common, but not observed from the ground due to multiple-scattering effects that are less of consideration for a plane flying near cloud-top. This will be clarified in the revision.

ACPD

7, S428–S432, 2007

Interactive Comment