

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

Anonymous Referee #2

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Review of “Observing cirrus halos to constrain in-situ measurements of ice crystal size”, by Garrett et al.

Although I applaud attempts to use halo phenomena for serious scientific research, I believe that in its current form this paper falls short of making the case that a limited number of aircraft observations can prove anything about constraining the sizes of cirrus ice crystals. The paper seems more concerned with supporting the view that cirrus clouds are inhabited by numerous small ice crystals, too small to be detected by most aircraft probes but often indicated by laser forward scattering probes (the FSSP or CAS) designed to size and count cloud droplets. But the evidence in this case is too speculative, and in some cases actually contradictory, to support publication, although the paper is well written, well cited, and interesting to read. My major concerns follow.

1. Good halos are not omnipresent in cirrus, so there is some luck in seeing them. I wonder why this particular field project seems so successful in this regard. There is evidence (Sassen 2005) that the chemical nature of the ice-forming nuclei can affect crystal shape and halo formation, so what was the history of the air masses sampled in this study? The halo of 46 degree radius is exceedingly rare (I have never seen a bright persistent one in over thirty years of trying), and there is no evidence provided that the “back-seater” actually observed such here. It is obvious that Fig. 5 shows an arc, not a halo, which is a result of horizontally oriented (large) ice crystals. This disproves their hypothesis, does it not.

2. As a frequent flier, it is probably true that 22 degree halos can be observed near cirrus cloud top, and in many cases they are too transient and vague to be readily observed from the ground. This suggests that the microphysical conditions near cirrus cloud top can be favorable for halo formation in that those ice crystals are newly-formed and probably more pristine. Yet, it appears that most of the in situ data presented comes from heights anywhere in the cirrus, and have little to do with cloud top. In Fig. 2 we see a good amount of very high extinction data (up to $>10/\text{km}$) that are unlikely to be from proper cirrus clouds (see the extinction PDF in Wang and Sassen 2002), let alone cloud top. Assuming an average cirrus cloud physical depth of 2 km and a maximum cirrus optical depth of 3.0, for example, maybe half of the data in Fig. 2, if they are characteristic of the entire cloud layer, may be from altostratus or dense anvils. It should be stated where the in situ data were collected, and from what types of clouds. The term “cirrus” in the title may not be appropriate.

3. The discussion of the microphysical conditions associated with halo formation is selective. They basically only consider pristine, solid column crystals, and rely on an aspect ratio relationship that is derived from surface snowfall measurements at much warmer temperatures. They gloss over the usual suspect for 22 degree halos- the bullet rosette crystal, which is essentially always randomly arrayed with respect to the individual bullets (Sassen et al. 1994). Such crystals can easily be on the order of 100

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microns, which could explain the bimodal peak in Fig. 4 (that is indeed larger for 46 degree halo clouds, another inconsistency). You need not necessarily have many small crystals to form a halo- a few good big ones will probably work. Their “upper bound” size discussion does not give proper attention to such particles, because they seem fixed on showing the importance of “small” cirrus crystals.

4. The concept of effective radius for nonspherical ice crystals is bothersome, considering the infinite variation in crystal shape, density, and propensity for spatial crystal clusters. They use two different definitions, and there are more. What is the effective radius of a bullet rosette with eight hollow bullets?

5. It has long been known that ice crystal shattering on airborne instrument inlets can create a stream of high concentrations of small crystal bits. There is another possibility that can elevate concentrations, which the authors can check. Because the CAS is designed for counting and sizing high concentrations of small aerosols and cloud droplets, it has a narrow inlet and laser beam to try to sample only one drop at a time. It also must sample electronically at a high frequency. So, if a large spatial particle (say a 500 micron long bullet rosette) is sampled, I think it likely that it would be sampled as a number of smaller bits (bullets). I suggest they check the temporal record of counts and look for clusters of particles that could result from shattering or this other effect. This used to work for identifying questionable FSSP data. Oddly, dividing a rosette into single bullets may yield a similar extinction coefficient that direct measurements would yield. Are bullet rosettes or aggregates present in the corresponding CIP images?

6. Much is made of the theoretical halo phase function predictions in Figs. 1 and 6. While I respect the theoreticians they cite, it must be recognized that for the size parameters involved, any approximate theory is questionable when it comes to the details of halo predictions. The particle sizes they are promoting are, unfortunately, too small for accurate ray tracing, the standard method for ice crystal scattering simulations.

7. It is stated in the Conclusions that “from a climatic standpoint, it is the crystal proper-

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ties nearer cloud top that matter most.” I disagree. In both the visible and infrared, it is the properties of integrated cloud optical depth up to approximately 3.0 that is sensed by satellites and of importance to radiative transfer.

I refer to the other reviewers comments addressing more minor points that would have to be corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1295, 2007.

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