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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.

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The reviewer’s primary concern with this paper is that the 46° degree halo shown in Figure 5 is not in fact a 46° halo. We address the reviewer’s concerns here, showing why it is difficult to argue that it is anything other than what we describe, especially in any way that would have meaningful consequences for the conclusions of the paper.

Major points

We explicitly acknowledge on page 1301 that there is some ambiguity regarding the upper limit on possible particle size due to other possible combinations of oriented crystals producing random facets. We will include more detailed discussion of the paper by Fraser (1979) in the revised version. Fraser shows that, indeed 22° halos are possible for low column tilt angles, and high solar altitudes. However the same

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conditions do not favor the combination with 46° halos, as observed.

Contrary to what the reviewer describes, there are no parhelia in Figure 5. This itself argues against ice crystal orientation of either plates or columns. We will include this argument in the revised version.

The reason the 46° and 22° halos do not appear perfectly concentric is likely due to angular distortion associated with the halo radiating close to the camera but at differing physical distances; unlike with an observer on the ground where the halo is effectively at infinity, here the observer is within the cloud. In this case, the effective radiating optical depth varies with the inverse of the cosine of zenith angle, from larger values closer to the horizon to smaller values looking straight up. From inspection of the Schwarzchild equation, this implies that the effective radiating depth will be farther from the plane closer to the zenith. The angular sensitivity will be largest for the 46° halo compared to the 22° halo because this halo covers the greatest range of zenith angle (about 90 degrees). It will give the impression of the halo extending up and away at higher zenith angles, which is precisely what is observed. The effect is analogous to what is observed with a fisheye lens. In any case, only a low-quality consumer digital camera was used, certainly with a mediocre lens, and this may have produced further angular distortion of the image. We will include these clarifications in the revised version.

Also, on p. 1307 there is description of the possible alternative that the outer band is instead an infra-lateral arc (supra-lateral is wrong and will be corrected). As described, this is likely not the case because the halo was photographed at its lower extremes just 9 seconds earlier, and in any case, both the 46° halo and infra-lateral arcs follow the same ray path through a crystal. Also, as shown on p. 47 of Tape (1994), infra-lateral arcs are associated with the parhelic circle, which was not observed. We will include the mentioned photo in the revised version as an inset to Figure 5.

The paper in its current form does not exclude the possibility that concentric 22 and

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46 degree halos could form for large ice crystals. They could, and this possibility is acknowledged. It is however stated that halos are not favored by large crystals, and provides supporting evidence that small crystals dominated based on optical depth and diffraction considerations.

Minor points

The paper by Strapp (2001) refers to a different instrument than we are concerned with here regarding measurement of very small ice crystals.

To the extent resolution matters it is a very second order concern for the first order question (are ice crystals large or small) that this paper addresses.

Particles being out of focus affected the CIP imagery but not the CAS imagery. Since the CIP made a negligible contribution to total measured cross-sections, out of focus imagery would not affect the conclusions, so we did not go into detail. However, what we did is assume that, while out of focus images have "donut-holes" in their centers that correspond to reduced cross-sections, the blurring makes them correspondingly larger in maximum dimension. These are two compensating errors. More sophisticated techniques exist for dealing with this problem, but for the sake of this paper, the conclusions are not sensitive to the exact details of any reasonable correction.

The CAS sizes were calibrated for ice spheres. Indeed, the reviewer is correct that there is ambiguity in CAS sizing of irregularly shaped particles. However, we have confidence in the data because the CAS and CIP derived extinction and ice water content agreed closely with those obtained by the bulk probes. This will be described in more detail in the revised version, although it is partly alluded to already.

We omit discussion of the number concentration, because in some sense, that is what is being tested. Comparisons with the Harvard probe will be mentioned in the revised version.

The "more general conclusions" have to do with relevance of the studies conclusions

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to the cirrus cloud measured in general, not just for halo observation times. This will be clarified.

We are concerned in this study with a first order question "Do small ice crystals dominate the observed scattering?". We examine this by looking at the extremes. The sensitivity study suggested addresses second order physics, which we deliberately avoid because it is less well supported by the accuracy of the study methods.

As stated in the text on p. 1309, the effective radius r_e was measured with bulk probes, whereas the area equivalent radius r_{eq} was measured with the CAS and CIP. Therefore the comment does not apply.

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

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