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Interactive Comment

Interactive comment on "On the relationship between the scattering phase function of cirrus and the atmospheric state" *by* A. J. Baran et al.

Anonymous Referee #2

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General comments:

The article presents a pioneering study of the relationship between atmospheric water vapour supersaturation and the departure of cirrus ice crystals from idealized shapes, as reflected in the the scattering properties retrieved from remote sensing measurements. This is very important, because if such a relationship exists as found in this work, then parameterizations connecting the supersaturation to scattering properties, such as the asymmetry parameter, will have to be included in climate models.

One flaw in this work is the limited character of the data: the conclusions are based entirely on one data set covering one co-incident flight. The study would have much greater value if other data sets were examined in the same way. If this is not possible or practicable, we should be told why, and the point that this is essentially a single case study should be emphasized and suggestions made for other future studies addressing this issue.

Another flaw concerns the single scattering model that is used, namely geometric optics (GO). While the GO includes facet distortion via facet tilt (the Cox and Munk 1954 model), it is only a surrogate for describing the scattering phenomena that are the expression of ice crystal surface distortion, as the GO model does not even include diffraction. One consequence may be that the discontinuity or nonlinearity observed in the RHi vs. distortion relationship, may be due to the fact that the facet tilt approach inadequately describes the scattering properties of ice, as it is essentially non-physical. That is not to say that the inclusion of air bubbles is physical - more likely, it is another ansatz that happens to produce results that match observations. Furthermore, the calculated asymmetry parameter values cannot be expected to be accurate. These points should be discussed.

While good use is made of the co-incident aircraft flight through the use of ARIES and the dropsondes, no advantage has been taken of in situ microphysical or humidity measurements. Why?

A further general comment is that the figures have been prepared somewhat carelessly, and both the graphics and the captions should be improved before the article is resubmitted for ACP.

Specific comments:

I do not like the statement in the Abstract that "This paper reports a positive correlation between the scattering phase function and RHi". Even though the statement is qualified in the next sentence, it still jars: something like "This paper reports a correlation between the shape of the scattering phase function and RHi" would be better.

Fig. 1: to aid a comparison between Fig. 1 and Fig. 9 that we are encouraged to do

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later, geographical coordinates should be shown in Fig. 1. Likewise in Fig. 6. Also, what does "composite" mean, briefly, in Fig. 1 caption?

Fig. 8 caption could be more descriptive: i.e. the retrievals are ARIES, and correspond to different runs. In (a) where was the sounding? Also I would drop the "percentage", what is meant is RHw expressed in %. In (a) RHi should ideally be shown, not RHw. Lastly, the units in (b) and (c) should be hPa, as in (a).

Figs. 10 and 12. These figures are nearly identical, and one has to struggle to see any differences. Either a different graphical representation should be used (possibly combining the two plots), or the difference should be quantified, or both. Quantitative descriptors could include a correlation coefficient; to remove a bias due to the large number of data points corresponding to large distortion, correlation could be calculated for mean RHi values for each of the four distortion levels. Also, it is not clear why the authors introduce a new variable, the "cirrus randomization parameter". Would the "distortion" variable not be better?

Technical corrections:

Page 14127, the index "j" has been omitted from the formulas.

Page 14141 line 20 and 14148 last line, change "Milosshevich" to "Miloshevich".

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 14109, 2014.