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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the interpretation of an unusual in-situ measured ice crystal scattering phase function" *by* A. J. Baran et al.

Anonymous Referee #2

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Review of "On the interpretation of an unusual in-situ measured ice crystal scattering phase function" by Baran et al.

Overall recommendation: Accept with minor revision.

Overview comments:

This manuscript interprets observations of an unusual scattering phase function measured by Gayet et al. (2012) in terms of a weighted habit mixture model of ten element hexagaonal ice aggregates and smaller ice crystals represented by Chebyshev ice particles. The authors argue that it is mainly the smaller ice particles that are responsible for the bow-like feature observed and dominate the scattered intensify measured by the PN. The findings of this manuscript are significant in that it attempts to interpret mea-

surements of the scattering phase function of distributions of ice crystals in terms of the scattering properties of some of the ice crystals that are found in the distributions. As such, I definitely feel that the article should be published. Nevertheless, there are a few aspects of the study and presentation that should be clarified before the paper is published.

Detailed Comments:

Page 3, line 24. I think a sentence should be added that there is also a need to constrain the asymmetry parameter of cirrus, since this is a parameter that is frequently used in the radiative transfer codes that are implemented in GCMs. This would increase the broader impacts of the study.

Page 4, line 23: "was" should be "were"

Page 5, line 12: "The PN measured the scattering phase function for each of the ice crystals shown in Fig. 1, . . ." Is this true? This sounds like the PN is measuring the phase function of each of the crystals measured by the CPI. This is clearly not the case. The CPI images a few of the ice crystals in the cloud that passes through the sample volume, and the PN will measure the scattering phase function of the crystals that pass through its sample volume. Although the crystals come from the same cloud, they are not the same crystals I believe. Rather, a statistical comparison is being made over the same population of crystals.

Page 6, line 3: Is it possible to have Figure 1 presented with higher resolution (even if two figures are needed in order to make it bigger)? With the resolution currently presented, it is very difficult to discern that the components of the chain like aggregates are quasi-spherical crystals. Can the authors also give some idea about the sizes of these quasi-spherical crystals. Both would seem to be important points given the subject of the paper.

Page 6, line 17. Ultimately, in our study of cloud physics radiative interactions we want to be able to obtain closure where we can take observations of ice crystal size and shape distributions, and calculate the scattering parameters (i.e., scattering phase function and asymmetry parameter) and have them agree with direct radiation measurements (i.e., from polar nephelometer). When applying the method of distortion to ray tracing, we are ultimately adding an element that is not directly based on observations: we suspect that there is roughness to ice crystals that cause such distortion but do not have the capability of actually observing that or testing the basis of the formulation of distortion with direct in-situ observations. It should be specifically noted that this is a limitation even though such a limitation is unavoidable right now given our current state of knowledge. This is essentially acknowledged on lines 1-3 on page 7, but I think the statement should be stronger (along with a call stating that we really need to be able to better characterize ice crystal roughness from an observational perspective).

Page 7, line 8. The study of McFarquhar et al. (2002, JAS), on which the lead author is a co-author, where Chebyshev shapes were used to characterize small ice crystals should also be referenced as the findings from that paper could be highly relevant to this current study.

Page 7, line 9: The authors state that the properties of the quasi-spherical particles are selected so that the ice bow feature is retained. Later, (paragraph starting line 18) the authors simulate the averaged scattering phase function using weighted mixture model, including the use of Chebyshev particles who were selected to retain the observed scattering phase function. There is subsequent comparison with the observed scattering phase functions. Is it then not surprising that you can get good agreement with the measured scattering phase function, since the in-situ properties were chosen to match these phase functions? A more convincing closure would be obtained if the measured ice crystal properties themselves were used to produce a scattering phase function independent with any information from the measured scattering phase function. I understand why the authors do this and have no objection to the paper being published using such an approach. But I think that they should more emphasize that

this is done and should state that more first principal information from the in-situ microphysics is needed to obtain a true closure. Also, can some more information about the actual aspect ratios of the ice crystals be included in the study to compare against the observed properties?

Page 8. Again, it is not surprising that there is agreement between measured and observed scattering phase function, since the weights are determined in order to minimize the disagreement. This should be noted at the beginning of Section 4.

Page 8, line 14: Why are phase functions normalized to unity at 15 degrees? Why that angle in particular? Why is not the integral of the phase function normalized to the same value?

Page 9, line 8: It could be of interest to compare against the merged scattering phase function of McFarquhar et al. (2002) that also included contributions from Chebyshev particles.

General Comment: The authors refer to the quasi-spherical ice crystals, both in their role as being present in the aggregates and through their role as 12 micrometer particles as used in the simulations. Thus, the question is are the authors saying that small quasi-spherical particles exist on their own, or is their existence on the aggregates sufficient to get the observed scattering phase function. As there is still considerable controversy in the cloud physics community on whether such small quasi-spherical particles actually exist given uncertainty in measurements, some discussion and clarification on this issue would be helpful. Alternatively, if I have misinterpreted something in the manuscript, please clarify.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12485, 2012.