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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.

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The reviewer is thanked for his comments on the paper, which have helped to improve the content as well as presentation of the paper. We are also pleased that the implications of the paper for light scattering theory, remote sensing and climate modelling, have been understood by the reviewer.

The specific points raised by the reviewer are discussed and answered below.

The main problem that this reviewer had with the original submission was the lack of clarity with regard to the geometrical choices made to explain the ice-bow feature. This main problem has now been addressed in the revised version of the paper. In the revision, it is explained, why the geometrical choices have been made, specifically this

is discussed on pages 9 – 12 of the revised manuscript. Essentially, the choice of quasi-spherical particle is limited, as the ice-bow must be preserved, previous work (i.e., Mishchenko and Travis, 1998, amongst other authors) have shown that high or low order quasi-spherical particles tend to either smooth out or form the rainbow peak, respectively. The choice of diameter is limited by the measurements reported by Gayet et al. (2012), the quasi-spherical diameters were reported to be of order 15 – 20 μm, this size is stated in the revised version of the manuscript. In the paper, the size of model quasi-spherical particle is assumed to be 24 μm, at the upper end of the measurements. For reasons of brevity, we chose not to include the phase functions of every single quasi-spherical particle that was investigated, we have, however, included the scattering phase functions of quasi-spherical particles that preserve the ice-bow feature. To this end, we have also included a model composed of spheres, to demonstrate that the sphere assumption is invalid. The phase functions of the quasi-spherical particles are shown in Figures 2 and 4 of the revised manuscript. All figures are now in colour. We have also included a new Figure 3, which shows the shapes of the Chebyshev ice particles.

As can be seen from the new figures, the phase functions, as expected, predicted by the quasi-spherical particles, have low side-scattering, which is why a highly distorted ice crystal is favoured, as this high distortion is necessary to average out the observed side-scattering. Other distorted ice crystals were investigated, but these did not lead to better fits to the measurements. These investigations were left out for reasons of brevity, and this is stated on page 15 of the revised manuscript.

The reviewer also made the point concerning the PSD effect. In this paper, the effect of the PSD is not considered. This was not stated clearly in the original submission. On page 10 and 15 of the revised manuscript, the word monodispersive ice particle is now clearly stated. Since we are concerned with large ice crystals and a narrow range of quasi-spherical particles, whose PSD is unknown, the assumption of monodisperse particles is sufficiently accurate for the purposes of this paper. The problem with the in-situ derived PSD is that it is highly likely to be contaminated with shattered ice crystals; evidence for this can be seen in Gayet et al. (2012). However, the shattering does not affect the phase function measurements, as can be seen from the new figure 1 b, of the revised manuscript, which shows a more resolved image of the ice-aggregate chains, which does not show any evidence of shattering. Therefore, it is assumed, stated on page 13, that the measured phase functions are unaffected by shattering.

We do not over generalise the findings of this paper, but we still do, point out, the implications that the paper has for light scattering theory, climate modelling and remote sensing. The following statement is made on page 5, of the revised manuscript, and in the conclusions on pages 17-19.

"Although, this one case cannot be generalized, it does, however, demonstrate that naturally-occurring phase functions may not always be relatively flat at backscattering angles, even if halos are absent. Clearly, further measurements of naturally-occurring ice crystal scattering phase functions are needed, in different types of cirrus, in order to test whether the occurrence of structure in the back scattering direction is common or not."

We should also point out that in the revised manuscript, the comparisons between the phase functions for the habit weighted mixture models and in-situ measurements are now plotted over the full range of scattering angle. The models predict different backscattering properties, beyond the range of the PN. Therefore, new PN instruments are required that measure the scattering phase function at angles well beyond 160o.

We now answer each of the specific comments made by the reviewer.

page 12468, line 24: Please refer directly to a publication on cirrus microphysics, e.g Korolev.

Response. We have now included a paper by Korolev (Korolev et al., 2006, Ice particle habits in sratiform cloud, Q. J. R. Meteor. Soc., 126, 2873-2902, 2006), when

specifically discussing the microphysics on page 3 of the revised manuscript.

12488, 10-12: I don't think that one measurement campaign in May 2007 really brings into question the experimental evidence from Foot, 1988; Francis et al., 1999; Baran et al., 1999, 2001; Labonnote et al., 2001; Jourdan et al., 2003; Baran and Labonnote, 2006, 2007; Baum et al., 2011.

Response. We do agree with the reviewer, and we did not mean to give that impression in the original submission. We have revised this statement as stated above in the revised manuscript.

12489, 18-19: well, mathematically, it can, but physically, g can not get negative.

Response. In the atmospheric sciences, where we consider independent scattering, the reviewer's statement about the asymmetry parameter not being physically less than zero is true. However, in general it is not. The paper by Mishchenko (1994) [JQSRT, 52, 95-110], in Figure 2 and 3, of that paper, the g value can become negative for densely packed particles. However, this paper is about independent scattering, so we have incorporated the words 'at least mathematically', when defining the asymmetry parameter on page 4 of the revised manuscript.

12490, 25: in fact, most if not all paper that claim to account for surface roughness use the above described distortion method

Response. Indeed, they do, but the question is whether it is true. The original method of distortion proposed by the reviewer was never called 'surface roughness', others have, however, decided to call it that with no evidence. This is why it was separated out from those who do claim it to be surface roughness, with no current evidence. This is why we call it distortion. However, we now combine the two on page 8 of the revised version of the paper. We decided to take the opportunity presented by the reviewer to discuss this in further detail, in the revised paper on page 8, we state

"Since no halo features are noted on the averaged phase function reported by Gayet et al. (2012), the method of distortion is applied to the ray tracing (Macke et al., 1996a and Yang and Liou, 1998). In this method, at each refraction and reflection event, the ray-paths are randomly tilted, with respect to their original direction. This randomization process removes energy from the halo and ice bow regions and re-distributes it to side-scattering and backscattering angles. Therefore, for high distortion parameters, the halo and ice bow features are removed, creating featureless phase functions. The distortion parameter can have values ranging from 0 (i.e., no distortion) to 1.0 (i.e., maximum distortion).

This geometric method of distortion is commonly referred to, in the literature, as surface roughness, as it is supposed to mimic the scattering due to the smaller-scale structure that might occur on the surfaces of ice crystals. Since, neither Geometric nor Physical Optics can be applied to such small-scale structures, due to diffractive effects from the smaller-scale structures; a large-scale geometric method of distortion is, therefore, used to approximate the small-scale structure. How well such geometric methods represent scattering, due to actual surface roughness, has yet to be evaluated, since to date, there have been no comparisons between the approximations and electromagnetic theory. Due to this lack of evidence, in this paper, the term distortion is used rather than surface roughness."

12492, 13-14: phase functions are normalized with respect to the full directional integration, not to match values at a certain scattering angle. And even if so, why 15 degree? The asymmetry parameter of the experimentally derived phase functions has been reported in the manuscript, so there must exist a proper normalization of the that phase function as well.

Response. We agree with the reviewer. The method of computing the g value for each model has now been changed, from the weighted mean value, to the actual value of g calculated directly from the re-normalized model phase function. The problem with using the weighted mean was that the g value calculated was not accurate since ray-tracing results were being combined with T-matrix. The only accurate calculation is to

re-calculate g directly from the model phase function. This direct calculation of g from the model phase function has now been incorporated into the revised version of the paper. Moreover, in the revised manuscript, the phase function is no longer normalized at the scattering angle of 15o, rather the full phase function is scaled to match the in-situ measurement, so that, the overall shape of the phase function is fitted.

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