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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 authors try to explain an observationally derived ice crystal phase function with a mixture of model phase functions from different ice particle habits. The choice of habits is motivated from ice crystal images and from the finding of ice bow scattering features between 120 and 160 degree scattering angle. In the end the authors find that a combination of distorted hexagonal columns (attached along a chain) and quasi-spherical particles (approximated by lower order Chebychev particles) fits best to the observations. The important conclusion is that there is a bit of sphericity in the scattering behavior of ice crystals which are usually assumed to be either hexagonal or highly irregular formed. This could have some implication for cirrus radiation budget and remote sensing, as the authors state correctly. What I find somewhat problematic is the

large number of geometrical choices that went into the work without verification or discussion. The Chebychev particles are assumed to be of order 3, not 2 or 4 or ... The authors use 2 deformation parameters for the Chebychev particles, 0.01 and 0.3. The chain of ice crystal consists of 10 elements. The Monte Carlo ray-tracing uses a distortion parameter of 0.8. Only the habit weights are clear as that are chosen to explain the observed phase function values best. Why not varying all the other parameters as well? Also, there should be a larger weight given to the smaller Chebychev particles to account for a size distribution effect. So, in principle, I think this is an important study worth publishing but the best fitting ice particle habits should not get over-emphasized or even generalized.

Specific comments:

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

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.

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

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

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.

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