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Interactive Comment

Interactive comment on "Dependence of the single-scattering properties of small ice crystals on idealized shape models" *by* J. Um and G. M. McFarquhar

Anonymous Referee #2

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General comments

The paper demonstrates that the single-scattering properties of small ice crystals, with a maximum dimension of 50 μm, are highly dependent on the assumed idealized shape. This dependence on shape becomes exacerbated when the authors introduce their own small idealized ice crystal model called the "budding Bucky ball", which is based on laboratory grown ice crystal analogues described in Ulanowski et al. (2004). The Ulanowski paper demonstrated, in part, that the then current generation of in situ microphysical probes may not be resolving the detailed structure of small ice crystals due to their limiting resolving paper. This lack of resolution causes the automatic

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classification algorithm, of say the CPI, to report back “quasi-spherical” ice crystals. This appearance of “quasi-spherical” ice crystals has led a number of authors to assume spheres, spheroids, and Chebyshevs to represent the light scattering properties of small ice crystals. More recently more complex models such as Gaussian random spheres and Droxtals have been assumed. None of the currently assumed idealized small ice crystal models contain protruding hexagons from a common center similar to that described in Ulanowski et al. (2004). The authors demonstrate substantial single-scattering (phase function and asymmetry parameter) differences between their Bucky ball model and previously developed small ice crystal models. For instance, for the most non-spherical particles the uncertainty in g can be as large as 14% which would imply an uncertainty in the instantaneous short-wave forcing of about $\pm$42 Wm-2, such an implied uncertainty is very large and g for small ice crystals must be further constrained. The question is how is it best to do this? The problem with such uncertainties is that they generate a cottage industry of papers which essentially just tweak one of the parameters, and this can go on ad infinitum, without adding anything of real significance, just a series of tweaks with endless sensitivity studies without constraint. What is the point of this? Is it not better to obtain further atmospheric state parameters coupled with ice crystal growth models so that small ice crystals can be predicted from first principles rather than end up with n small ice crystal models? The authors do elude to this in their conclusion, but why not just do it? The point of the Ulanowski paper was to demonstrate the potential deficiency of current imaging probes by showing for the same resolution, as the imaging probe, a more complex particle may appear “quasi-spherical”. The model adopted by the authors was not meant to characterise actual small ice crystals that might exist in cirrus, it was merely used to demonstrate the potential inadequacy of current imaging probes. Moreover, the Ulanowski paper does not mean that small ice crystals are not “quasi-spherical”. At the moment we simply do not know how non-spherical small ice crystals may or may not be. The present paper is found to be unconvincing and further analysis is required before the paper can be accepted for publication, the major concerns are listed below together with a number of more minor issues.

Major concerns

1. The authors do not present any quantitative evidence that the Bucky ball is representative of real small ice crystals that might exist in cirrus. All that is cited is a paper by Bailey and Hallet (2009) based on laboratory grown ice crystals, some of which appeared to have budding arms. Can the authors be more quantitative here, what fraction of ice crystals had budding arms and how many arms were observed? What were the assumed growth conditions for these ice crystals in terms of temperature and humidity? What were the activation nuclei or what was the doping property that was put onto the substrate? Are the resulting ice crystals a function of doping property? Laboratory experiments such as these may not be representative of actual cirrus conditions due to the nature of particle growth in such experiments.

2. The Bucky ball phase function is invariant with respect to area ratio as is expected since the aspect ratios of the columns are invariant. With the number of arms adopted this significantly increases side-scattering and hence g becomes quite low. Why was the number 20 assumed? Is this number of arms representative of laboratory experiments of Bailey and Hallet (2209) and/or Ulanowski et al. (2004)? A further model that has been used to study ice crystal scattering is the Polycrystal due to Macke et al. (1996)[J.Atmos.Sci.53,2813-2825]. This model at 0.55 um gave an asymmetry parameter of about 0.74, invariant with respect to size since the aspect ratio of the Polycrystal was near unity. Therefore, how does the side-scattering of the Polycrystal compare with the Bucky ball model? This must be shown in Figure 11 for the same size, since if the side-scattering of the Bucky ball exceeds the Polycrystal or is very similar then we can dismiss this new model as being representative of cirrus scattering since we know from POLDER measurements that the Polycrystal side-scattering is too high [Labonnote et al. 2001. JGR 106,12139-12153].

3. The authors do not test their model against actual satellite-based measurements

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or in situ estimates of the bulk extinction, which could be easily derived from the CPI instrument that they used. Is the Bucky ball predicted area compatible with measurements? If the test suggested in point 2 above is inconclusive then a further test using PARASOL data on a pixel-by-pixel basis for appropriate thin cirrus cases should be performed. It is believed at the moment that the paper is too premature since the authors present no tests of their model scattering predictions against measurements.

4. The behaviour of the model at absorbing wavelengths should also be investigated at for instance at 1.6 um, where obviously the single-scattering albedo will be less than 1. How does this single-scattering albedo compare with other models including the polycrystal? Or how does the volume-to-area ratio compare to other models used for small ice crystals, including the Polycrystal? It is also known that due to the compact nature of the polycrystal the single-scattering albedo was too low when compared against aircraft measurements described in Francis et al. (1999) [JGR 104,31685-31695]. The same could also be true of the Bucky ball model? The authors need to prove their model against observation.

5. Further analysis on the polarization properties of the Bucky ball would also be helpful, how do the other scattering matrix elements compare against other small ice crystal models? Especially the P12 element.

6. The present paper is also incomplete as it only considers non-absorbing wavelengths. The A-train samples cirrus across the electromagnetic spectrum simultaneously. It is important to develop ice crystal models that are consistent across the electromagnetic spectrum. How does their model compare against others at wavelengths in the 8-12 um region? It is suggested that the authors use the publicly available DDA method to complete this study. With the latest DDA method size parameters of 40 should be attainable.

It is believed that the present paper is incomplete for the reasons listed above; in essence the authors must provide the evidence that shows their model is at least

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representative of the scattering properties of optically thin cirrus. If they are able to prove their model against currently available observations and measurements then the contribution becomes significant.

Minor points.

1.Introduction: Line 14 page 28111. The parameterizations and retrieval methods also depend on the assumed randomization of the model ice crystals.

2.Introduction line 24 page 28111. The aggregate of columns proposed by Baran and Labonnote (2007) are highly randomized such that their scattering properties become independent of the initial monomers making up the ice aggregate. Therefore, from the scattering pattern it would not be known that the particles were aggregates of columns. Other models tend to retain the hexagonal symmetry such that the scattering properties of individual monomers making up the aggregate are retained.

3.The statement on page 28113 line 8 the single-scattering scattering properties of ice crystals also depend on size and assumed randomization.

4.In the discussion of shattering on page 28114 section 2 the paper by Field et al. (2003) [J. Atmos.Ocean.Tech. 20,249-261] should also be cited as the authors also find that for narrow PSDs shattering is not likely to be a major problem.

5.Section 4 page 28120 line 21 please give the reference for the value of the refractive index of ice used in the calculations.

6.Section 5.1 page 28122 line 22. The authors should also note that in the review paper of Baran (2009) [JQSRT 110,1239-1260] it is shown that the g value of aggregates tend to asymptote as a function of aggregation for columns and plates due to the spatial nature of the resulting ice aggregates.

7.Page 28125 line 7. The term multiple scattering is used. Since the paper is based on the method of ray-tracing the term multiple reflections should be more appropriate.

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Figures.

Fig 1. The maximum dimensions in each image are very unclear can these be made clearer?

Fig 7. What do the authors mean by irregular hexagonal columns? In what way are the columns irregular? It is difficult to see the irregularity in the figure.

Fig 10. The phase functions for the more irregular particle show some degree of noise at scattering angles between about 50 -180 degree. This suggests that there are insufficient random orientations assumed in the Monte-Carlo simulations.

Fig 11. Same as fig 10.

Fig 13. What does the g value for the 3B model with aspect ratio of 1 increase with decreasing area ratio?

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