

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

A. J. Baran et al.

anthony.baran@metoffice.gov.uk

Received and published: 17 August 2012

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.

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.

C5843

Response. Agreed. We have now added to the revised version of the manuscript a discussion on the asymmetry parameter. Specifically, on page 4 of the revision, the following is stated.

“Of importance to climate models is the asymmetry parameter, which is a parameterization of the scattering phase function. The asymmetry parameter, g , is formally defined as the average cosine of the polar (scattering) angle, and is therefore, a measure of the degree of asymmetry in the forward scattering part of the phase function. The asymmetry parameter can take on, at least mathematically, values between ± 1.0 . The reason why the asymmetry parameter is important for a climate model is because it is one of the scattering parameters that controls how much incident solar radiation is reflected back to space (Stephens and Webster, 1981; Liou and Takano, 1994; Baran, 2004; Ulanowski et al., 2006). Clearly, for the case of conservative scattering, high and low values of the asymmetry parameter will, respectively, reflect less and more incident solar radiation back to space. Therefore, constraining the asymmetry parameter is also just as important as constraining the scattering phase function.”

Page 4, line 23: “was” should be “were”

Response. Done.

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.

Response. The reviewer is corrected, in the revised version; the text has been amended to the following, on page 6,

C5844

“Due to the differing sampling volumes of the PN and CPI instruments, these instruments do not measure the same ice crystals. They do however, measure ice crystals that occurred in the same cloud. It is therefore, possible, to make statistical comparisons between the CPI and PN measurements, over the same population of ice 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.

Response. Agreed, we have added a Figure 1b, this figure shows a higher- resolution image of the aggregate-chains. The diameters of the quasi-spherical particles were reported in Gayet et al. (2012) as being between 15 and 20 μm , and this is stated in the revised version of the manuscript and elsewhere. As the reviewer will appreciate there is considerable uncertainty as to the actual sizes of these quasi-spherical particles. The theoretical computations were performed assuming diameters of 24 μm , which is the upper range of the in-situ measurements. In the figure below we show that T-matrix simulations for $t_3(0.03)$ particles of diameters 15 and 24 μm are not significantly different to change any of the conclusions reached in the paper. All other quasi-spherical particles were tested in the same way, and no significant differences were found.

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

C5845

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).

Response. Agreed. The results from this paper suggest that the current representation of surface roughness by light scattering theory is incomplete. Since, in geometrical optics, the surface roughness is approximated by distortion of the ray paths, this means that the phase function becomes completely featureless. However, surface roughness on naturally-occurring ice crystals may not necessarily re-produce featureless phase functions. We have extended this discussion along the lines the reviewer suggests in the revised version of the manuscript. Specifically, we state the following, in the conclusion's section on page 18-19; we believe that the statement below is sufficiently strong.

“Figures 7 and 8 demonstrate that PN instruments are required that measure the scattered intensity over the full range of scattering angle, which is at least technically possible. This is required so that discrimination between models and reliable estimates of g can be achieved. It has been argued that quasi-spherical particles are reasonable for the appearance of the ice bow-like feature on the in-situ measured scattering phase function. In this case, the surface roughness on the host ice crystal appears to be composed of quasi-spherical particles. However, geometric approximations to surface roughness, used in current light scattering models, predict that no backscattering features should be present. The results from this paper appear to question such approximations, and more exact methods should be developed to account for surface roughness. Indeed, the asymmetry parameter of the best model fit is 15% higher than the asymmetry parameter predicted by the highly randomised ice aggregate model. The surface roughness that occurs on naturally-occurring ice crystals may not neces-

C5846

sarily mean small asymmetry parameters. Moreover, to aid theoretical developments, microphysical instrumentation should be developed that can image the structures and scale of the surface roughness. It can no longer be generally assumed that phase functions with no halos are also featureless and relatively flat at backscattering angles. This may be particularly true for the tops of anvil cloud, and for this type of cloud g is particularly important. To this end, space-based instruments should be developed that are able to resolve sufficiently, the backscattering properties of cirrus. The findings of this paper have important implications for cloud physics, light scattering theory, climate modelling and for the remote sensing of cirrus. It is therefore, of necessity to understand whether the phase functions, and consequently, their g-values, shown throughout this paper are a common occurrence.”

The reviewer should also note that we have extended the simulations to cover the complete range of scattering angle, as the models, predict different scattering behaviour, beyond the measurement range of the PN. It is therefore, important, to develop PN instruments that can measure as much of the scattered intensity as technically feasible. Moreover, we also suggest that space-based instruments should be similarly designed, to meet the specifications of the GLORY mission, which unfortunately failed. Such a mission would prove invaluable.

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.

Response. Agreed. We have incorporated the suggested reference into the revised manuscript, and the merged phase function does not exhibit an ice bow, probably due to the high-order assumed for the Chebyshevs and the addition of “rough particles.” We have noted this, in the revised version of the paper. In this paper, we suggest that quasi-spherical particles could also occur as surface roughness rather than as individual small ice crystals. However, we do not exclude the possibility that quasi-

C5847

spherical particles may also exist, of course, as individual particles.

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?

Response. We do agree, in principle, with the reviewer. However, as the reviewer will well understand that true closure, would require shattering effects being removed from the measured PSDs, it is clear from Gayet et al. (2012), that the measured in-situ PSDs were contaminated by shattering. Although, the phase functions and Fig. 1b do not appear to be, this is stated in the revised paper. Although, the models have been pre-selected we have also included the sphere as an additional model. The models must not only retain the right back scattering features but also side-scattering. This is not clear, as to which, would match the side scattering measurements best. Information on the distribution of the quasi-spherical particles on the ice crystal structure and their turbidity is not known, and the overall aspect ratios of the ice crystals are not readily known at this time. The backscattering feature, might also, possibly be a

C5848

turbidity feature, due to electromagnetic coupling between the particles that constitute the surface roughness. This is a further possibility that is stated in the conclusions of the manuscript. On page 13 of the revised manuscript, the following is stated,

“To compare P11 directly to the in-situ measurements, i.e., the differential scattering cross section, would require knowledge of the scattering cross section and total number density of ice particles. However, due to the problem of ice crystal shattering on the inlets of closed-path microphysical instruments (Korolev et al. 2011), there is considerable uncertainty with regard to the actual values of the total number concentration and volume extinction coefficient. To achieve direct comparisons between theory and measurements would also require more detailed information on the distribution and shapes of the individual structures that compose the surface roughness as well as its turbidity. Therefore, in this paper, true closure is not claimed, since in-situ measurements are not currently available that can achieve true closure.”

On pages 9-12 of the revised manuscript, we do state the need to retain the ice bow-like feature more explicitly, and it is this feature that is clearly being modelled.

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.

Response. This has now been stated 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?

Response. Due to both reviewers not liking the normalization of the phase function at a particular scattering angle, this has now been removed from the revised version of the manuscript. The asymmetry parameters are now explicitly calculated from the re-normalized scattering phase function.

C5849

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.

Response. We have noted this in the response above and have included a discussion of the suggested reference in the revised manuscript.

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.

Response. We actually state that the quasi-spherical particles dominate the scattered intensity measured by the PN, so therefore, we are suggesting that from the simulations, the quasi-spherical particles, which is the surface roughness, essentially act as individual scatterers, but on the surface of the host particle. In our simulations, we therefore, assume, independent scattering. This point has been further clarified in the revised version of the paper.

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

C5850