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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Remote sensing of ice crystal asymmetry parameter using multi-directional polarization measurements – Part 2: Application to the Research Scanning Polarimeter" *by* B. van Diedenhoven et al.

Anonymous Referee #2

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General comments

This is an interesting and useful paper that applies a newly developed multi-view multi-wavelength polarized remote sensing technique to retrieve the asymmetry parameter (g), ice crystal surface roughness, and aspect ratio of naturally-occurring tropical cirrus. The technique is tested against in situ estimates of g using four cases of cirrus which occurred during the CRYSTAL-FACE campaign of 2002. The authors show that for two cases the median values of retrieved and in situ g were within about 1.5% of each other, whilst the other two cases were within about 6% of each other. The paper demonstrates the usefulness of using polarization to add information to the retrieval of cloud properties and the necessity of including full information about the scattering matrix apart from just using the P11 element alone. The retrieval of g and aspect ratio are important to obtain as the two are closely related and g is one of the important parameters in climate models that determines how much incident solar irradiance is reflected back to space. Therefore, any information on g is welcome in order to constrain the asymmetry parameter values assumed in climate models. As the authors point out, most climate model parameterizations of g predict values significantly greater than the values that they estimate and retrieve, as to whether these retrieved and estimated values are correct is another matter and is why further research on this topic is sorely needed. The other useful point about this paper is that the range and number of scattering angles used in the retrievals far exceed what is currently available from space-based observations. It is hoped that this paper will, in-part, encourage deployment of instruments similar to the GLORY concept.

The paper is rather long, the instrumental sections are somewhat laborious and detract from the results that the paper wishes to communicate and details of the flights are not really necessary, the list of references is adequate. However, the paper is full of physical assumptions which may or may not be true, the authors need to state a number of caveats after each of the statements, examples are given below.

1. One of the major assumptions that the authors make is that their chosen large-scale approximation to ice crystal surface roughness is true. The authors are merely using a gross approximation (based on the Cox-Munk approach to mantle tilting) applied to ray-tracing to predict featureless scattering matrix elements. This is not the same as actual surface roughness, Macke et al. (1996) referred to this approximation as "distortion" as it is a method used to change the direction of ray-paths within the crystal after each reflection-refraction event. No evidence is shown that this technique represents actual surface roughness or any evidence cited. To do this properly requires a more rigorous treatment of surface roughness, based on electromagnetic theory or some

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improvement to physical optics, which can capture the full interaction, unfortunately ray-tracing cannot do this. The authors are actually randomizing the ice crystal on the large-scale, so rather than using the term retrieval of surface roughness, they should perhaps refer to their retrievals as retrievals of "distortion", the retrieved distortion values are still very important as they are a departure from the pristine case. The authors find that their retrievals indicate highly randomized ice crystals, pristine ice crystals are not found, a finding that is in agreement with a large body of the literature. There might be some cases of actual ice crystal surface roughness which might by itself produce optical features on the scattering matrix elements at scattering angles not observed by the instruments and as such a completely different g might result. This is why instruments are required that sample the scattering matrix elements across a much larger range of scattering angle than presented by the authors, this point needs to be made.

2. A further major assumption of the authors is that the individual monomers that make-up the ice crystal aggregate are so far apart from each other that there is no interaction between them and as such the scattering phase function of the aggregate is the same as the monomer. They quote some examples, it is true that the scattering phase functions look similar, but they are not exactly the same, and hence g will vary as the ice aggregate grows. This growth of the ice aggregate and the impact of such growth on g is shown in figures 5 and 6 of Baran (2009), in those figures it is shown that the resulting g values of the aggregate ice crystals are not the same as the initial monomer and that the change in g as the ice crystal grows is dependent on the initial shape of the monomer. The authors need to show that their assumption holds for distortion parameters greater than some value, if not, then the change in g depends on initialization.

3. Most of the ice crystals sampled by the CPI appear to be indeterminate, in such cases it might be possible, that these indeterminate ice crystals are compact single entities which might not be roughened. Such compact single ice crystals may also have very low g values as shown by Um and McFarquhar (2009). Moreover, Ulanowski et al.

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(2006), figure 8 of that paper, estimate g experimentally and show that small roughened rosette ice crystals may have g values much less than what is retrieved by the authors. Such ice crystals would be indeterminate with regard to the CPI, the authors need to demonstrate that they can retrieve such small values. They seem to be limited to g values > 0.71, or is a saturation limit reached, in which case it is no longer possible to retrieve g lower than a certain value? Note also, that the small smooth ice analogues have no backscattering features, although there is some difference between the DLP shown in Figure 10 at backscattering angles. However, integrated over a full PSD these would become smoothed, convolved with multiple scattering effects differences may not be so discernible between rough and smooth small ice crystals even using polarization.

4. Another major assumption of the authors is that 3D RT effects can be ignored in their modelling of the radiative transfer (RT). The Polarimeter used has a horizontal resolution of a few hundred metres. Therefore, do the authors average the Polarimeter measurements to larger 1 km domains, or do they use the full horizontal resolution? If the full horizontal resolution is used how can they justify not taking into account 3D RT effects? If they used the full horizontal resolution, then if averaged up to 1 km, do they obtain the same results?

5. The authors touch upon the issue of ice crystal shattering. This issue might be particularly important for the 29th July case. To ascertain as to whether ice crystal shattering was present the authors should examine each CPI image to see if more than one crystal was present in each image. If they find this occurrence, then this indicates that ice crystals arrived in bunches which implies shattering. On the other hand the CPI processing might have removed the shattered ice crystals from one image so a large ice crystal image might appear alongside images of smaller ice crystals in the same frame. These occurrences in the CPI images should be checked for and commented on in the paper.

Minor Points

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1. The bias that is reported in the abstract may also be due to shattering of ice crystals, this possibility should also be stated, unless they can prove otherwise.

2. Please state whether the anvils sampled were young or mature? and which of the cases were young and mature anvils?

3. The authors retrieve g-values for optical thickness > 5, what is the effect of multiple scattering on their results?

4. The RT discussion, please state assumptions, plane-parallel? Homogeneous cloud layer?

5. Please show a figure of the instrument noise as a function of scattering angle, and indicate in that figure the range of scattering angle that is actually used in the retrievals.

6. Is Beer's law assumed due to issues of speed using full RT? If so, please state, and what is the error in assuming Beer's law?

7. Comparing retrieved aspect ratio against CPI images is meaningless as CPI images are just projections. Comparison can only be qualitative, although this does not mean that the retrieved aspect ratios are correct. This caveat should be stated in the paper.

8. In the previous paper the authors used scattering results from a more rigorous physical optics model why are these not used in this paper?

9. A number of g-values taken from climate model parameterizations of g are quoted; the authors should also note that in the paper by Edwards et al. (2007), from Figure 2, top- panel of that paper, the g value from that parameterization can be as low as $\sim$0.75, at a non-absorbing band.

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