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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.

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GENERAL COMMENT: The paper is rather long, the instrumental sections are somewhat laborious and detractfrom 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.

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Interactive Discussion

Discussion Paper

REPLY: We thank the reviewer for the kind words and very helpful suggestions. The instrumental sections might seem laborious, but needed in our view because of the fact that RSP in not a well-known instrument and because some additional level-1 processing was required owing to a lack of aircraft attitude information. Also we need to elaborate on the possible shattering issues affecting the CIN measurements, as also pointed out by the reviewer below. The details of the flights are short and give information about, e.g., cloud system history and age (as also requested by the reviewer below). These things considered, we did not find any way to shorten the paper without removing crucial information.

COMMENT 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 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.

REPLY: More information about the distortion parameter was provided in Part 1 of the paper. For completeness we included this information in this paper too and elaborate further on the interpretation of the distortion parameter in the Macke et al. (1996) model. Furthermore, we agree that actual surface roughness could theoretically produce additional optical features through the generation of ray caustics for particular forms of distortion for single ice crystals. However, ice crystal roughness implies some stochastic roughening process, such as riming. For a collection of randomly oriented roughened particles, any such optical features are therefore not expected to be observed. The following was added to section 2.1:

"The ice crystal optical properties are calculated using the standard geometric optics code developed by Macke et al. (1996). This ray tracing code takes distortion of ice crystals into account in a statistical manner by perturbing, for each interaction with a ray, the normal of the crystal surface from its nominal orientation by an angle varied randomly with uniform distribution between 0degree and delta X 90 degree, where delta is referred to as the distortion parameter. Thus, this approach represents the stochastic large-scale distortion of a collection of ice crystals. However, Yang et al. (2008) found that this approach is also an efficient, yet accurate treatment of microscale surface roughness. For a large collection of ice crystals microscale surface roughness and large-scale particle distortion both lead to a similar randomization of the angles between crystal facets, which in turn leads to the diminishing of features in the scattering phase matrix. Increasing the number of impurities within ice crystals also has a similar effect (Hess et al., 1998). Thus, we consider the distortion used here a proxy of randomization of the angles between crystal facets possibly caused by any of these effects."

Consistent with this discussion we state that the retrievals indicate "roughened or distorted ice crystals" in the conclusions and abstract. Furthermore, we refer to delta as “distortion” rather than “microscale distortion” throughout the paper.

Furthermore, the following was added to the conclusions: “Furthermore, in order to provide a more rigorous evaluation of the capabilities of the approach presented here, in situ instruments are required that that are not contaminated by shattering artifacts and possibly sample the scattering matrix elements across a wide range of scattering angles.”

COMMENT 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.

REPLY: This assumption is tested in Part 1 on a large number of complex ice crystals and their mixtures and asymmetry parameters were retrieved within 5%. In section 2.1 of the revised paper we elaborate somewhat more on this: “The retrieval technique was evaluated in Part 1 using simulated measurements based on state-of-the-art optical properties of smooth, moderately roughened and severely roughened solid plates, solid and hollow columns, solid and hollow bullet rosettes, droxtals, aggregates of columns and aggregates of plates, as well as several mixtures of these habits (Baum et al., 2005a, 2011; Yang et al., 2013). The evaluation showed that the ice crystal asymmetry parameters are generally retrieved to within 5%, or about 0.04 in absolute terms, largely independent of calibration errors, range and sampling density of scattering angles and random noise in the measurements. Moreover, plate-like, column-like, smooth and rough particles were generally found to be correctly identified."

COMMENT 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. (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.

REPLY: The range of asymmetry parameters in our LUT is indeed 0.71 to 0.94, as now stated in section 2.1 of the revised paper. We agree that the scattering properties of small ice crystals are highly uncertain. Small particles are already discussed in section 3.1: "ice crystal effective radii retrieved using GOES-8 (Minnis et al., 1995) that are collocated with the Citation aircraft available from ESPO range from 25 micron to 40 micron (not shown), which is expected to be large enough for the geometrics optics approximation to be valid." We have added the following to the discussion in section 3.1: "Furthermore, the scattering properties of small ice crystals are highly uncertain (e.g., Ulanowski et al., 2006; Um and McFarquhar, 2009)"

COMMENT 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

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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?

REPLY: 3D effects may be expected to cause variability in the retrievals over an inhomogeneous cloud deck. However the primary 3D effects on retrievals that we have identified in water cloud retrievals are cloud top height variations that at low sun-angles change the brightness of the cloud substantially and cloud-side illumination of cumulus clouds that can also enhance, or reduce polarized features. For most of the case studies presented, the sun is high in the sky, the variability of the retrievals is rather small and the clouds are extremely extended horizontally. This suggests that the side illumination and cloud top height variation effects are not significant. The variability in the 11 July case is larger than for the other cases, but whether this results from 3D effects or actual variability in asymmetry parameter is not known. Averaging the data over time would diminish this variability, but that would not answer the question of whether the variability results from 3D effects or natural variability of the ice. A more detailed study using simulated measurements calculated with a 3D vector radiative transfer code is planned for the future. We now list in the conclusions that possible 3D effects are one of the remaining issues to be studied in future work.

COMMENT 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.

REPLY: The following sentences appeared in the original paper in section 3.1: "Evidence of shattering in the data of the Cloud Particle Imager probe on the Citation during CRYSTAL-FACE has been seen (Carl Schmitt, private communication), although the severity and effect of shattering is unique to each instrument design (Korolev et al., 2011). For example, the CIN has a much larger aperture than the CPI."

To better highlight this point, we moved these sentences to section 2.4 where the CIN data is discussed. Also we added statements about the possibility of shattering affecting the CIN to the conclusions and abstract. The potential effect of shattering on the CIN measurements is discussed in the paper and taken into consideration as a source of uncertainty in the CIN measurements. However, as noted in section 2.4 and 3.1, the potential effect of shattering on asymmetry parameters measured by the CIN is difficult to estimate.

Minor Point 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.

REPLY: The possibility of the bias occurring because of shattering has been added to the abstract and the conclusion.

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

REPLY: In all cases the anvils sampled were roughly 1.5-2 hours aged and active cells remained in some contiguous region of the sampled cloud. This information has been added to the introduction of section 3. Also ages of the clouds sampled are added in the individual descriptions of the case studies.

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

REPLY: The effect of multiple scattering on our retrievals is expected to be minimal

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since it is fully taken into account in the look-up-table. We added the following additional information to the paper in section 2.1: “The LUT is calculated using the doubling-adding method (Hansen and Travis, 1974; De Haan et al., 1987) assuming a single uniform plane-parallel cloud layer.”

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

REPLY: See our reply to the previous comment.

Minor Point 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.

REPLY: The RSP instrument noise does not vary with scattering angle. The data at scattering angles larger than 165 degree is not used because there polarized relfectance over ice clouds are usually near zero, as now noted in the revised paper. We further explain the data filtering in section 2.1 by adding the following: “Since the RSP noise and calibration uncertainties in polarized reflectances above clouds are expected to be well below 0.002, independent of measurement geometry (Knobelspiesse et al., 2012), this filtering can be considered conservative.”

Minor Point 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?

REPLY: Note that Beer’s law is only used in the sunglint filter and that the threshold of 0.5% of sunglint contribution is a conservative one. In the revised version we clarify this in section 2.1 by stating the following: “Note that this estimate of contamination of sunglint is approximate and that the threshold of 0.5% is a conservative one based on visual inspection of the polarized reflectances measured over clouds at sunglint geometries.”

Minor Point 7. Comparing retrieved aspect ratio against CPI images is meaningless

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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.

REPLY: The fact that comparison between CPI and our retrievals are only qualitatively was already stated in the conclusions and abstract of the paper. To clarify more, we added the word "qualitatively" in all paragraphs that discuss a comparison between the CPI and RSP retrievals. Also we added the caveat that CPI images are 2-dimensional projections in section 3.1.

Minor Point 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?

REPLY: The more rigorous calculation using the Improved Geometric Optics Method (IGOM) by Yang et al. are currently only available for particles that have predetermined aspect ratios, which only vary with size. Moreover, they are only available for three particle distortion levels. In order to obtain optical properties for ice with a virtually continuous selection of aspect ratios and distortion values, we calculated them with the geometric optics code available to us. In part 1, we tested our approach on simulated measurements that were calculated with optical properties from IGOM, as noted by the reviewer. For simulation assuming single plates and columns, as well as those assuming complex habits, the ice asymmetry parameters were all retrieved with the stated accuracy of 5% as noted in the manuscript. To clarify, we now explicitly state in section 3.1 that the LUT is based on optical properties calculated with standard geometric optics and that the approach is tested on simulated measurements using state-of-the-art optical properties.

We also note that in the paper it was already stated that the effective radii retrieved by GOES-8 are generally sufficiently large to imply that the geometric optics approximation is justified (see our reply to comment 3).

Minor Point 9. A number of g-values taken from climate model parameterizations of g

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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 0.75, at a non-absorbing band.

REPLY: We now mention the fact that some parameterizations yield lower asymmetry parameters and now cite Edwards et al. (2007) in the conclusions. The statement that most climate models and satellite retrievals use greater asymmetry parameter values than found here has been retained, and we note that the reviewer's overview agrees with that statement.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 32063, 2012.