Atmos. Chem. Phys. Discuss., 15, C11074–C11080, 2016
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Atmospheric Chemistry and Physics Discussions

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15, C11074–C11080, 2016

Interactive Comment

Interactive comment on "Degree of ice particle surface roughness inferred from polarimetric observations" *by* S. Hioki et al.

Anonymous Referee #1

Received and published: 4 January 2016

General comments

This is a novel and original paper, which for the first time attempts to apply a more rigorous principal component-based polarimetric retrieval method to linearly polarised measurements to obtain quantitative global information on the surface roughness properties of atmospheric ice. A good analysis of error is presented and the choice of error distribution in the measured linear polarisation is shown to be correct. The application of principal component analysis to the retrieval of surface roughness is novel, and they find that almost 100% of the variance in the –P12 (sigma) element is sufficiently captured by the first two components. This approach leads to a much simplified forward model, comprising of just a linear combination of the first two principal components. This is a very powerful technique and the importance of applying these PC techniques to retrievals is exemplified by the fact that the basic underlying physical model (being the eight-branched hexagonal ice aggregate) on which the forward model is based appears to fail often. This paper is publishable as it presents a novel retrieval methodology and very interesting results worthy of wider dissemination. The tropical results are very interesting, as it is in the tropics where climate models are most frequently in error in terms of cloudy short-wave and heating biases. However, there are a number of issues that the authors should address before the paper is finally published and these are discussed below.

Major concerns

1. The authors ignore aerosols above the cloud. It is well known that aerosols can contaminate polarisation measurements above the cloud, and these may result in spurious retrievals noted by the authors. Such contamination may frequently occur in the tropics due to increased anthropogenic aerosol over the Indian Ocean. The authors need to show that aerosols do not bias their results. This can be shown by using standard aerosol profiles in their radiative transfer calculations and perform sensitivity studies or use CALIOP data to show that aerosol was not above the cloud during the period under study.

2. Determination of cloud-top height. Figure 9 demonstrates the importance of constraining cloud-top height to retrieve surface roughness. Is the cloud-top sufficiently well constrained? It would be useful to show by using an independent dataset provided by the DARDAR product (i.e. combines active radar and lidar to retrieve cloud profiles) as to how well their cloud-top is constrained? The use of passive radiometric measurements provides only a weak constraint as the cloud-top determined using IR or solar measurements depends on the profile of IWC. Thus, over regions of convection, the IWC profiles vary considerably and IWC increases as a function of distance from the cloud-top. With such profiles, the passive IR measurements might appear warmer due to emission from below the cloud-top, which in turn might result in a significant error in the assignment of cloud-top altitude and hence retrieved surface roughness. Might

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the error in the cloud-top altitude account for some of the non-retrievals found in the tropics?

3. The authors find estimates of surface roughness well beyond the range of their theoretical results. There could be other reasons for this not discussed by the authors. Other reasons could be as follows: (1) Accuracy of the light scattering computations. The authors make use of a physical optics approximation but do not show or cite results that confirm the approximation is sufficiently accurate in calculating the –P12 element. Please show or cite results and quantify errors in the backscattering direction? (2) The method of tilted facets to represent surface roughness may not be sufficiently accurate to represent naturally occurring deep surface roughness? A paper by Liu et al. (2013) [JQSRT 129, 169-185] show that the scattering matrix elements calculated using the tilted facet method becomes inaccurate at backscattering angles when compared against an electromagnetic treatment of idealised surface roughness when sigma=0.2. Is it possible to quantify the inaccuracy of the TF method used to calculate the matrix element when sigma»0.2? They might be able to account for this inaccuracy in their retrievals if there is a systematic bias in the TF results? Please comment and show results.

Minor points

Please could the authors proofread their manuscript again to remove errors such as typos, incomplete sentences, etc., etc.?

Citations. Page 34285 line 6. When discussing microscopic morphology the authors should also consider citing Ulanowski et al. (2006; 2014) [Ulanowski, Z., Hesse, E., Kaye, P. H., and Baran, A. J.: Light scattering by complex ice- analogue crystals, J. Quant. Spectrosc. Radiat. Transfer., 100, 382–392, doi:10.1016/j.jqsrt.2005.11.052, 2006; Ulanowski, Z., Kaye, P. H., Hirst, E., Greenaway, R. S., Cotton, R. J., Hesse, E., and Collier, C. T.: Incidence of rough and irregular atmospheric ice particles from Small Ice Detector 3 measurements, Atmos. Chem. Phys., 14, 1649–1662, doi:10.5194/acp-

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14-1649-2014, 2014.

Page 34285 line 18. When discussing biases in global retrievals by inappropriate application of a phase function perhaps they should also cite Macke and Mishchenko 1996 [Macke, A., and M.I. Mishchenko, 1996: Applicability of regular particle shapes in light scattering calculations for atmospheric ice particles. Appl. Opt., 35, 4291-4296, doi:10.1364/AO.35.004291].

Page 34285 line 14. When discussing constraints on IWP, they should cite Sourdeval et al. (2015). In that paper, a technique for directly retrieving IWP from global solar and IR measurements is demonstrated in the presence of multi-layer cloud [Sourdeval, O., C.-Labonnote, L., Baran, A. J. and Brogniez, G. (2015), A methodology for simultaneous retrieval of ice and liquid water cloud properties. Part I: Information content and case study. Q.J.R. Meteorol. Soc., 141: 870–882. doi: 10.1002/qj.2405].

Page 34286 line 7. Two papers published in 2015 are cited to support the application of surface roughness to improve solar, near-ir, and ir retrievals. I agree with this statement, but the application of this in addition to ice crystal complexity in the form of ice aggregates also leads to more consistent retrievals as demonstrated by Baran and Francis (2004) using very high-resolution solar and infrared measurements [Baran, A. J. and Francis, P. N. (2004), On the radiative properties of cirrus cloud at solar and thermal wavelengths: A test of model consistency using high-resolution airborne radiance measurements. Q.J.R. Meteorol. Soc., 130: 763–778. doi: 10.1256/qj.03.151].

The authors need to state before page 34299 that their analysis is based on a pixel-by-pixel approach. Perhaps in section 1 or when they first start to use POLDER data?

The previous best-fit approaches were mostly based on super-pixels derived from the POLDER product. Could the use of super pixels reduce the problem shown in Figure 1?

The authors use the term “satisfactory” several times throughout the paper when com-

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paring model results. Please quantify this statement? What do they mean? Please provide a quantitative statistical measure to these statements.

Section 2.2.1 page 34292. Over which size parameter ranges were the IGOM and ADDA methods applied? In the case of ADDA, how was surface roughness represented? The eight-branched hexagonal ice aggregate, is this the same as modelled by Yang and Liou (1998)? If so, please cite the reference as follows [Single-scattering properties of complex ice crystals in terrestrial atmosphere. Contrib. Atmos. Phys.,71,223–248].

Section 3. The authors estimate surface roughness parameters well beyond their theoretical limit. Given the techniques employed to represent surface roughness, retrieving sigma values »1 demonstrates a failure in the model, which is pointed out. They do not find any solutions beyond their theoretical limit and must rely on extrapolation to obtain an unphysical surface roughness estimate of 2.82, tending to an upper value of 13.6! Relying on extrapolation to obtain these gross values is very unsatisfying. Surely after a certain sigma value the –P12 element converges until their is no longer information on sigma? They can demonstrate this theoretically by simply showing a figure of the –P12 element as a function of sigma. It could be that they are retrieving unphysical values due to their being no information on sigma? As the –P12 pattern becomes asymptotic at the most extreme values of sigma. Please comment and show results?

It is difficult to see from Figure 13 the proportion of the sample that contains retrieved sigma values > 0.7. Please state this proportion? Is this proportion location specific? Cloud-top height specific? Aerosol above cloud? Please comment and show results?

Apart from the above, a further possibility is that the particles could be hollow as well as surface roughened. This is mentioned in the case of rosettes, but another form of hollowness is stepped cavities and these appear to be frequently occurring as shown in laboratory studies conducted by Smith et al. (2015) [Helen R. Smith, Paul J. Connolly, Anthony J. Baran, Evelyn Hesse, Andrew R.D. Smedley, Ann R. Webb, Cloud cham-

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ber laboratory investigations into scattering properties of hollow ice particles, Journal of Quantitative Spectroscopy and Radiative Transfer, Volume 157, May 2015, Pages 106-118, ISSN 0022 4073,http://dx.doi.org/10.1016/j.jqsrt.2015.02.015]. This is an interesting form of cavity as multiple scattering increases due to the stepped nature of the cavity, and consequently, the asymmetry parameter values decrease relative to the more conventional cavity types. This behaviour is similar to spherical air bubble inclusions and will affect the –P12 element in a similar way to surface roughness without having to over prescribe surface roughness values.

Another possibility is that the ice aggregate model might be too compact and as a result the multiple scattering between the individual monomers that make up the aggregate might well over estimate the side scattering and so incorrectly decrease the linear polarization. Natural ice aggregation due to gravitational sedimentation tends to more spatial ice aggregates (by spatial, I mean multiple interactions between monomer particles can be neglected). In this way, the identity of linear polarization is retained, which is eventually removed by hollowness, surface roughness or a combination of both. The authors should also consider the inclusion of more spatial ice aggregates in their future studies.

A further reason for the number of failed retrievals could be due to lack of information. This number might be reduced if other independent measurements were made available to the retrieval through the greater use of radiometric measurements at different wavelengths as well as active measurements to constrain the cloudy profiles of IWC or IWP and cloud-top height through lidar backscatter and linear depolarisation measurements. All these techniques as the authors are well aware will make use of other scattering matrix elements which they so far neglect. They might like to point out the wealth of information that is now available and the relative ease with which it can be incorporated into a PC-based retrieval scheme.

The tropical results are very interesting. I agree that the failure in the tropics is more likely due to errors in the prescribed scattering model. A paper by Baran et al. (2012)

[Baran, A. J., Gayet, J.-F., and Shcherbakov, V.: On the interpretation of an unusual in situ measured ice crystal scattering phase function, Atmos. Chem. Phys., 12, 9355–9364, doi:10.5194/acp-12-9355-2012, 2012] shows that averaged in situ Polar Nephelometer measurements obtained in a convective cloud could only be explained by the inclusion of quasi-spherical particles, in that case represented by Chebyshev particles. Could this be the reason for the retrieval failures in the tropics? This is a further model or variant thereof that is worthy of future investigation by the authors.

Figures 12-14. The heights of the histograms exceed the values along the y-axis. Please re-plot so that the histogram heights do not exceed the y-axis values. Otherwise, it is difficult to estimate probabilities from the graphs alone.

Along with the retrieved sigma values that are within their theoretical range of sigma, could they provide the corresponding values estimated for the asymmetry parameter? Or is this too much of an extrapolation at the moment?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34283, 2015.

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