Atmos. Chem. Phys. Discuss., 14, C3843–C3849, 2014
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the relationship between the scattering phase function of cirrus and the atmospheric state" *by* A. J. Baran et al.

Anonymous Referee #1

Received and published: 19 June 2014

This paper aims to explore "the relationship between the scattering properties of atmospheric ice and the physical state in which the ice resides", which is indeed "important to explore, as this may lead to an improvement in the parameterization of ice optical properties in climate models." The general style of paper is adequate, although in my opinion some paragraphs and figures can be omitted. Unfortunately, in my opinion the science in this paper has several flaws, which make the conclusions not justifiable. Only when the method is adequately demonstrated and more statistics are included and when all comments below are adequately addressed in the text, I would recommend the paper to be published in ACP. However, the current version of the manuscript is insufficient for publication. General and specific comments are listed below.

General comments:

1) Although the technique used here is based on previous analysis of POLDER data, these previous applications analyzed statistics of many cloudy pixels to arrive general, qualitative conclusions. The current application to derive distortion levels from individual pixels should first be demonstrated adequately before being applied to real data. Several important questions remain unresolved:

a) How does the capability for PARASOL to discriminate different crystal distortion (or habit in general) depend on the cloud optical thickness? As discussed below in more detail, directionality of the reflection should be reduced by increasing contributions of multiple scattering, i.e., increasing optical thickness. I would expect results to depend on optical thickness.

b) How does this capability vary with the angular range and sampling? The phase functions in Figure 4 show only minimal differences at the sampled scattering angle range of 80-130 degrees. I would expect the technique to be better suited for pixels sampling between, e.g., 120-170 degrees.

c) Is the use of a single habit mixture with only varying distortion sufficient? The scattering phase function not only depends on distortion, but also on habit (Um and McFarquhar, 2007, 2009; Macke et al., 1996a; Yang and Liou 1998). Here only a single selection of habits and a single PSD is used. The ensemble model might generally fit in situ volume extinction measurements, but that does not constrain the scattering phase function. Furthermore, a very large variation of habits is possible in natural clouds. The dependence of the scattering phase function on crystal shape and the implications for the method needs to be discussed. For example, the ensemble model consists of columns, bullet rosettes and aggregates of columns, but what if a real cloud contains mainly thin plates or columns with very different aspect ratios than used in the model?

References:

- Um, Junshik, Greg M. McFarquhar, 2007: Single-Scattering Properties of Aggregates of Bullet Rosettes in Cirrus. J. Appl. Meteor. Climatol., 46, 757–775.

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- Um, J. and McFarquhar, G. M. (2009), Single-scattering properties of aggregates of plates. Q.J.R. Meteorol. Soc., 135: 291–304. doi: 10.1002/qj.378

2) As stated in the paper, "this paper reports a positive correlation between the scattering phase function and RHi." This conclusion is based on 12 pixels in total, which in my opinion is a too small number to justify any conclusions. Furthermore, low RH values are also associated with pixels indicating severely distorted crystals. The fact that the 12 pixels with less distorted crystals are 1) on the edge of the area were the data was within the selection criteria and 2) their adjacency of null-results to the 12 pixels with less distorted crystals also raises concerns. For example, could contamination of lower lying liquid water clouds be excluded? Finally, as further argued below, the criteria for the null-results are not given, while these remove the majority of the field with low RH from the analysis. Only by inclusion of more data and more robust statistics the present conclusions could be reached.

Specific comments:

Page 14111, second paragraph: I suggest also to include the new results by Magee et al. (ACPD, 2014) in the discussion:
Magee et al., "Mesoscopic surface roughness of ice crystals pervasive across a wide range of ice crystal conditions", Atmos. Chem. Phys. Discuss., 14, 8393-8418, 2014

Page 14112, line 29: The reduction of the halo features by increasing roughness was recently shown by Van Diedenhoven (2014). That paper also explores the presence of halo features in mixtures of rough and pristine ice crystals, which may be relevant for the discussion on page 14113.
Reference:
Van Diedenhoven, B., 2014: The prevalence of the 22° halo in cirrus clouds. J. Quant. Spectrosc. Radiat. Transfer, in press, doi:10.1016/j.jqsrt.2014.01.012.

Page 14113, line 28: Please also include the recent paper by Cole et al.:
Cole, B. H., Yang, P., Baum, B. A., Riedi, J., and C.-Labonnote, L.: Ice particle habit and

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surface roughness derived from PARASOL polarization measurements, Atmos. Chem. Phys., 14, 3739-3750, doi:10.5194/acp-14-3739-2014, 2014.

Page 14117, line 1: Please mark the location of the used data (i.e. where the aircraft was above cloud) on the map in figure 6. Were there lidar measurements at the location at which the retrievals suggest pristine crystals?

Page 14119: paragraph 2 and further: In my opinion, the discussion about the comparison of the area ratio of the used ice model and in situ measurements is out of scope of this paper. The paper aims to relate the scattering phase function with variation of RH. The area ratio is not uniquely related to the scattering phase function, which is mainly determined by the overall shape of the crystals, the aspect ratios of their components and the level of distortion. As the authors already noted, the capability of the ensemble model to replicate in situ estimates of volume extinction and other cirrus properties is already demonstrated in several papers (Baran et al., 2009, 2011a, 2014a). I suggest removing this part of the paper, including figure 3.

Page 14122, line 27: Please indicate here which definition of distortion parameter is used. Is this using the uniform distribution of Macke et al. (1996)?

Page 14125, line 12: I believe “total reflectance” should be “spectral albedo” here.

Page 14125, line 15: What are the assumptions for the aerosol and are they realistic for this particular dataset?

Page 14125, line 19: I assume a Cox and Munck model is used. Please add the reference. Is the reflectance value of 0.000612 an addition to the reflectance predicted by the Cox and Munck model? These details are not given in the Buriez et al. (2001) paper.

Page 14124, line 4: Please note that aerosol scattering, Rayleigh scattering and glint on the ocean surface also add to directional variation of measured reflectance.

Page 14126, line 15 and line 27: Please note that the directional dependence can also

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be caused by inhomogeneity in the cloud, as discussed by Buriez et al. (2001). Strictly speaking, the assumption of a perfectly homogeneous cloud is unphysical in itself, so please rephrase these sentences.

Page 14127, line 3: I would expect that the level of directional variability of cloud reflectance depends also on the cloud optical thickness. For optically thin clouds, single scattering is contributing significantly to the total reflectance and I would expect the shape of the phase function to be of greater importance here. Directionality should be reduced by increasing contributions of multiple scattering. The importance of the optical thickness of the cloud on the analysis should be discussed here.

Page 14130, line 15: The figure shows retrievals over land, although one of the selection conditions was for the measurements to be over ocean, which is also consistent with the radiative transfer model. Please clarify and remove the land pixels.

Page 14130, line 15: It would be very illustrative to add a plot to the paper showing the retrieved optical depth. How do the retrieval results correlate with optical thickness?

Page 14130, line 16: It is stated that 190 pixels contained no discrimination between ice models. What criteria are used here to define “no discrimination” and what is the basis for these criteria? The method seeks the lowest rmse (Eq. 5) produced by the different models and theoretically one model should lead to the lowest rsme. It is extremely unlikely that two or more models yield exactly the same rsme. Please clarify.

Page 14130, line 21: What are the indications for multi-layered clouds? Please show that the results are not affected by multi-layered clouds. The pixels with more pristine crystals are adjacent to pixels with null-results, which raises concerns about possible contamination.

Page 14131, line 8: Please show differences in PARASOL measurements in the region with pristine particles and those in regions with distorted crystals to illustrate the difference in backscattering features.

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Page 14131, line 16: If I understand correctly, the PN measures over nearly the full scattering angle range, so please clarify the remark about the need “in situ observations to sample the scattered angular intensities over a more complete range of scattering angle than is currently possible”.

Page 14132, line 15 and figure 10: In my opinion, the arbitrary x-axis is not very illustrative. Why not use the distortion value itself?

Page 14133, line 1 and further, figure 11, and appendix A: I am puzzled by these figures and their explanation. At 865 nm, where ice is essentially non-absorbing, most light will penetrate through the whole cloud for cloud optical depths below about 8-12, where the reflectance is below 50

References:
- Liou, 2002, An Introduction to Atmospheric Radiation, book, ISBN: 978-0-12-451451-5
- V.V. Rozanov, A.A. Kokhanovsky, The average number of photon scattering events in vertically inhomogeneous atmospheres, Journal of Quantitative Spectroscopy and Radiative Transfer, Volume 96, Issue 1, 15 November 2005, Pages 11-33, ISSN 0022-4073, http://dx.doi.org/10.1016/j.jqsrt.2004.12.026.

Page 14134, line 7: Figures 10 and 12 seem almost identical. I suggest noting that the weighting of RH does not make a difference instead of showing a new figure. However, the weighting of RH over the cloud depth needs to be corrected.

Page 14134, line 11: The correlation between RH and crystal distortion is far from convincing. The sample size for the low distortion pixels is very low (12 out of 297).

Pages 14135-14136: Please adapt the conclusions to reflect all the changes made accordingly.

Technical corrections:

Page 14111, line 18: “Aspect ratios” should be “Area ratios” here.

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Page 14111, line 20: I believe “make-up” should not contain the hyphen.

Page 14112, line 2: Please remove brackets around the citations.

Page 14113, line 7 and page 14136, line 26: Please change “Van de Diedenhoven” to “Van Diedenhoven”

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 14109, 2014.