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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Ice particle habit and surface roughness derived from PARASOL polarization measurements" *by* B. Cole et al.

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This paper aims to retrieve information about the global variation of ice crystal habits from polarized reflectances. As discussed in the introduction this is an important goal as the current knowledge on the variation of ice shapes and roughness is very poor, but can have a profound effect on the ice cloud radiation balance. Global polarization measurements are crucial to gain more information about the global ice crystal variation.

As acknowledged in the submitted paper, the retrieval approach adopted here is largely based on the approach that I recently developed with colleagues as described in the referenced papers van Diedenhoven et al. (2012) and van Diedenhoven et al. (2013).

I will refer to these papers as "Part 1" and "Part 2", respectively, in the remainder of this review. Co-authors of the submitted paper Ping Yang and Bryan Baum were also co-authors on Part 1.

There are some fundamental differences between our approach and the one adopted in this paper. The approach adopted here can be a useful addition to the previous POLDER studies and the paper would be suitable for ACP. The paper is well written and clearly structured. However, as discussed below this approach has some limitations that need to be realized and discussed. Also there are some fundamental issues with the application of the retrieval approach that I list below. If these issues can be adequately addressed and if the limitations of the adopted approach are adequately described in the paper, I can recommend the paper for publication in ACP.

Major comments:

1) The main fundamental difference between the approach adopted in the current paper and our approach described in Part 1 is that Cole et al. aim to find a fit to the measurement within a look-up-table based on a set of habits with pre-described geometries and variable roughness, whereas we use a large set of single columns and plates with a virtually continuous selection of aspect ratio and roughness. Since complex ice structures have scattering properties that strongly resemble those of their hexagonal components, these single columns and plates serve as radiative proxies for complex particles, as we demonstrated in Part 1. Our approach reduces the retrieval to finding a characteristic aspect ratio and roughness level that yields a match with the measured polarization. This allows retrieval of the asymmetry parameter of the complex habit in the observed cloud and also information about the aspect ratio and roughness of the crystal components, as we demonstrated in Part 1. This approach thereby circumvents the problem that a limited selection of habits always will be a rather arbitrary choice out of the virtually unlimited possible realizations of ice crystal shapes existing in nature.

The approach adopted by Cole et al. is certainly also a useful one to explore, but the limitations need to be realized and discussed. For example, let us consider a cloud of which the radiative properties are dominated by bullet rosettes that have rather compact arms with aspect ratios of 2. If the bullet rosettes that are included in the database have aspect ratios closer to, e.g., 10, their polarization features will not match the polarization features of the observed cloud and the retrieval will likely not recognize the cloud as being dominated by bullet rosettes. In fact, the polarization features of a simulated cloud using aggregates of columns will probably match better because of the assumed aspect ratio of their components that is close to 2. The assumed geometries for the habits in the used database are very unconstrained and geometries of habits in natural clouds are likely to vary substantially due to varying atmospheric conditions and crystal formation histories.

This limitation means that when a given measurement is consistent with the simulations assuming aggregates of columns, for instance, this does not mean that the dominant habit in the observed cloud is necessarily aggregates of columns, but rather it means that whatever habit dominates the scattering properties in the cloud matches the polarization signatures of aggregates of columns and likely has components with aspect ratios that are consistent with those assumed for aggregates of columns.

The results of this work should be presented in the light of these limitations. Please discuss these limitations and how to interpret the results in the paper.

2) Page 29491, line 1-2: Three different effective sizes are used. It is stated that the results are not expected to depend on size, since the non-absorbing wavelength is insensitive to size. It is true that the wavelength used is not sensitive to size. However, most of the habits included in the optical properties database have assumed dimensions (aspect ratios of crystal components) that change with size. As we showed (Part 1), the asymmetry parameter and the polarized reflectance of ice crystals significantly depend of the aspect ratio (of crystal components). Effectively, assuming different sizes in the study of the submitted paper adds more variation in crystal habits investigated. However, it is not the change in size that matters, but the changes in aspect ratios.

In the current manuscript, this is not clear. Related to this, it might not be surprising that the aggregate of column is not only found the best fit to the measurements for the assumed size of 60 micron, but also for the larger size assumptions (Page 29495, lines 7-15), since this aggregate is one of the few habits in the used database that is not assumed to change with size. The polarized reflectances simulated using aggregates of columns will be essentially the same for all sizes.

Furthermore, the observed small shifts in retrieved roughness when assuming different sizes (section 3.2) have nothing to do with particle size. Instead the results reveal the limitation of the approach using just a limited set of habits, since changing the crystal aspect ratios in this set (effectively by changing the selected size) yields different results.

In summary, the inclusion of results for different size assumptions is confusing. The different results have nothing to do with difference is crystal size, but are just a result of the assumed size-dependent crystal dimensions (i.e., aspect ratios). This confusion should be clarified. I suggest using one size only and discussing the limitations. Alternatively, the look-up-tables for the 3 sizes can be combined into one larger one, and the lowest RRMSE within this LUT can be determined. In any case, it should be explained clearly in the paper that the crystal size does not matter in this analysis, but only the particle shape, which happens to be assumed to change with size for most of the habits included.

3) Page 29493, Line 7: The optical thickness included in the POLDER level-2 product is used to select a LUT computed with the closest optical thickness. It is my understanding that the POLDER level-2 products assume the inhomogeneous hexagonal crystal (IHM) model with an asymmetry parameter of about 0.76 (C.-Labonnote et al., J. Geophys. Res., 106, 12139–12155, 2001). Since the retrieval of optical thickness largely depends on the asymmetry parameter assumed in the retrieval algorithm (e.g., Zhang et al., Atmos. Chem. Phys., 9, 7115–7129, 2009), the use of the level-2 product in the current study to select a LUT corresponding to the retrieved optical thickness

is fundamentally wrong. For example, let us consider a pixel with a retrieved optical thickness of 2. This optical thickness is consistent with the measured reflectance, assuming the IHM with an asymmetry parameter of 0.76. However, a pristine plate, for example, will have an asymmetry parameter of about 0.95. To produce the same reflectance, a cloud consisting of pristine plates needs to have an optical thickness about (1-0.76)/(1-0.95)=4.8 times larger than a cloud consisting of IHM crystals (Zhang et al., 2009). However, in the approach adopted by the authors, the LUT of pristine plates for an optical thickness of 2 would be selected for this case. Since polarized reflectances for cloud optical thickness lower than $\sim$5 depend on cloud optical thickness (Part 2), the LUT for pristine plates will most probably not fit the measurements and lead to high RRMSE values mainly because the selected optical thickness is wrong. This is why in Part 1 we perform separate optical thickness retrievals for any combination of shape and roughness (i.e., asymmetry parameter) before calculation the RRMSE.

This fundamental error might explain why the aggregate of columns so often leads to the best fit with the measurements, because this habit has an asymmetry parameter similar to the IHM model (Table 2). This error needs to be corrected in the revised version. Instead of the level-2 retrieved optical thickness, the optical thickness needs to be retrieved for any combination of shape and roughness in the database. An alternative, approximate approach could be to use the scaled optical thickness tau*(1-g) to select a LUT with the corresponding scaled optical thickness. Another option would be to omit any result for clouds with retrieved optical thickness lower than $\sim$5, in order to ensure saturated polarized reflectances.

4) The results are presented for all data over ocean. I would strongly suggest to present results for several optical thickness ranges. Presenting the results in optical thickness ranges would reveal the possible difference between thin cloud (COT<5) of which most could be assumed to be cirrus (either in situ or anvil blow-off) and the thicker clouds (COT$\sim$>20) that could be associated with convective activity. Since such cloud types have very different formation histories, such statistics may reveal differences in habits

and roughness values. Cirrus clouds dominate the global coverage statistics, so the current results are most likely dominated by these clouds. In case no systematic difference in retrievals for different cloud optical thicknesses is found that would also be interesting information that can be mentioned in the paper without showing all the plots. (Note that this segregation into optical thickness ranges would only be possible for optical thickness smaller than 5 if the error noted in my previous comment is resolved.)

5) Lines 13-24 on Page 29496 and Figure 13 raise two important questions.

a) If I understand correctly the dots are all simulated polarized reflectances corresponding to the best fit of the data for each selected pixel, while the colors represent the distribution of real polarized reflectances of the selected data. It is true that the simulations fit the bulk of the data, but they do not fit the outliers at all. This suggests that the multi-directional polarized reflectances at many data points cannot be sufficiently fit by the simulations. The simulations should be able to span the whole space of measured polarized reflectances and again this plot suggests that the used database is too limited. It seems likely that in some cases, the RRMSE is very large and the retrieval result would be quite meaningless. Maybe I misinterpret Figure 13? Please clarify this.

b) The statistics of measured polarized reflectances in Fig. 13 are similar to previously presented results (e.g., Knap et al., Appl. Optics, 2005; Baran and Labonette, 2006; Cole et al. 2013). The results presented here, as well as previous results, puzzle me because they seem to only represent polarized reflectances that are saturated, which is only the case for relatively thick clouds with optical thicknesses larger than about 5 (Part 2). For instance, at an optical thickness of 1, the polarized reflectance at a given scattering angle of an ice cloud with a certain ice crystal habit is about 60% of the saturated value (Part 2). Previous papers, including Cole et al. (JAMC, 2013), also explicitly state that the observed statistics are being fit with simulations with saturated polarized reflections (COT>5). However, in none of these papers and neither in the paper under review the authors state that a selection of data points is made that ensures optical thicknesses larger than 5 to produce these statistics plots.

Since global coverage of ice clouds are probably dominated by clouds with optical thicknesses lower than 5, I would expect the bulk of the presented statistics to be more consistent with polarized reflectance significantly lower than those expected for saturated values. Moreover, the naturally present range of ice cloud optical thicknesses should lead to a spread of polarized reflectances at small scattering angles (e.g., at 90 degrees). The statistics presented in Fig. 13 (and previous POLDER studies) suggest that the selection criteria applied inadvertently exclude thin clouds. I suggest investigating the statistics of optical thickness of the included pixels. If pixels with low optical thickness are included I suggest separate figures such as Fig. 13 for thick and thin clouds.

Note that if indeed thin clouds are somehow excluded from the study due to the used selection criteria, my concerns raised in comment 3 are no longer relevant. Also, explicitly adding a selection criterion to select only relatively thick clouds clouds with a retrieved optical thickness larger than 5 (e.g., Chepfer et al. 2001; van Diedenhoven et al., JAS, DOI: 10.1175/JAS-D-11-0314.1) would remove the concerns raised in comments 3 and 5b.

Specific comments:

1. Introduction:

Page 29485, line 5 (and page 29488, line 6): The term "Top of atmosphere" is used only twice in the paper and both times the acronym TOA is given. I suggest to remove "(TOA)" in both cases.

Page 29485, line 11: Note that Magono and Lee (1966) only classified snow crystals warmer than the homeogeneous freezing level (> -40 C).

Page 29486, line 28: It is stated that ice roughness lowers asymmetry parameter. Please give references for completeness (e.g., Part 1).

Page 29487, line 8 and further: Since the used method is very much based on our

work (Part 1 and Part 2), I would appreciate if this basis is already explained in the introduction. Please discuss the differences between the current approach (using a selection of complex habits) and ours (using columns and plates with a virtually continuous selection of aspect ratios and roughness parameters as radiative proxies of complex particles), as detailed in my major comment 1.

2.2 PARASOL satellite data:

Page 29489, line 3: The selection of data over ocean only is an important limitation, since ice clouds over land could very well be different. This should be clearer stated in the conclusions and abstract.

Page 29489, line 3: In Part 1, we showed that scattering angles in the 120-150 degrees range are essential for a proper retrieval. This is not included in the selection criteria of the current paper. The criterion that the data should span 50 degrees of scattering angle does not ensure the 120-150 degrees range to be measured. Data in the 60-110 degrees scattering angle range does not include sufficient information to sufficiently distinguish different aspect ratios and roughness values. I suggest adding this extra selection criterion or checking how results vary with varying scattering angle ranges available.

Page 29489, Eq. 2: Please indicate how the sign is determined or give a reference.

Page 29489, line 23: I would suggest moving the sentence about exclusion of sunglint to line 6, so that all selection criteria are in one place.

2.3 Ice properties:

Given the concerns expressed above, it would be useful to show how the polarized phase functions (or degree of polarization or simulated polarized reflectances) vary between the included habits and included roughness values. An example is also shown in Fig. 3 of van Diedenhoven et al. (2012; Part 1).

Page 29490, line 20: I believe that the Baum et al. (2005; 2011) papers are good references for this averaging procedure.

Page 29491, Eq 3: Since P is a matrix, please use bold face.

2.4 Retrieval:

Page 29492, line 5: The method that we described in van Diedenhoven et al. (2012; Part 1) was presented as a general method to be applied to any instrument capable of making multi-directional polarization measurements, including POLDER. We explicitly discussed its applicability to POLDER measurements. Please remove “from RSP. . ..” or change the sentence to

“Van Diedenhoven et al. (2012, 2013) presented a method to infer the ice asymmetry parameter by using polarized reflectance measurements and applied it to measurements from the RSP (Research Scanning Polarimeter) aircraft instrument.”

Page 29492, line 10: the difference between our approach and the current one should be indicated. We use single columns and plates with a virtually continuous selection of aspect ratios and roughness parameters as radiative proxies for more complex particles, as discussed in my major comment 1.

Page 29493, line 17-19: I am confused about this sentence. POLDER has up to 16 viewing geometries per 6x6km pixel. Since 9 pixels are combined here, I would expect a maximum of 16*9=144 viewing geometries to be evaluated, all with slightly different azimuth angles. Are only 16 selected from these measurements? How is this selection made? Is this selection representative of the whole superpixel? Please clarify.

Page 29493, line 19-22: The way that the Cole et al. (2013) study is described here, it seems to be the same as the approach taken in the current paper, which is not the case. This is confusing and I suggest removing this sentence since the Cole et al. study and results were already in the introduction.

3. Results:

Page 29494, line 6 and captions of Figure 1 and Figure 7: Please include the important information that these statistics are limited to measurements over ocean.

Page 29494, line 11: I was a bit confused what is meant with “and most retrieved habits are within a few percent for each size”. I suggest to remove it.

Figure 6. This figure contains the same information as figure 5. Just noting that a similar independence with size assumption is found would be enough. I suggest removing figure 6.

Figure 12. This figure contains the same information as figure 11. Just noting that a similar independence with size assumption is found would be enough. I suggest removing figure 12.

4. Summary:

Since method is very much based on our work, I would appreciate if this basis is also mentioned in the summary.

Page 29497, line 17: Please include that the data is restricted to pixels over ocean.

Page 29498, line 13: I suggest including a reference to van Diedenhoven et al. (2012, JAS, DOI: 10.1175/JAS-D-11-0314.1), in which we also examined POLDER measurements above deep convection and found that the polarized reflectances of cold clouds were consistent with rough compact crystals with an asymmetry parameter of 0.74. Ice clouds at warmer temperature (>-40C) were found to be consistent with rough plate-like particles with larger asymmetry parameters.

Page 29498, line 28: The reference van Diedenhoven et al. (2012) should be van Diedenhoven et al. (2013). I would also suggest removing “very” from this sentence, since most retrieved values were around 0.78.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29483, 2013.