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Atmospheric Chemistry and Physics
Discussions

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

Anonymous Referee #2

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Review of "Degree of ice particle surface roughness inferred from polarimetric observations" submitted for possible publication to the Atmospheric Chemistry and Physics by S. Hioki et al.

This paper describes a method to interpret polarized reflectance measurements over high and cold clouds in terms of microphysical properties of ice particles. The method is applied to measurements from the PARASOL spaceborne mission. The results indicate that the method is theoretically powerful and much better than the traditional "best fit" approach. In practice, the results are not in agreement with the forward simulations.

There is a lot of different material of this paper from an evaluation of the noise in the PARASOL polarization data to the analysis of two cases studies in the tropics and extra-tropics, and through the development of an EOF-based inversion method. This

results in a rather large number (16) of figures. On the other hand, little explanation and detail is provided for each item of the paper.

The authors argue that their approach, based on the EOF analysis of the spectral and directional properties of the polarized reflectance, is better than the traditional "best fit" approach because it is much more resilient to the measurement noise. Yet, the result appear inconsistent with the theoretical development : In the tropics, the chi-square is much larger than expected. In the extra-tropics, although the chi-square are in line with expectations, the roughness parameters that are found are way outside the range of values used for the forward simulations (the median of the retrieved values is 2.93 for a theoretical range of [0;0.7]. The discrepancies are not properly analysed. There is no information whether the inconsistency between the theoretical simulations and observations results from the amplitude of the polarization, its spectral variations, or its directional properties. As a consequence, the paper is of little use for the community.

In addition to this major criticism, I find that the presentation of the paper results is not appropriate. First, it should be very clear that the "surface roughness" of the ice particles (in the title) is only an effective parameter attempting to reproduce the polarization properties of ice clouds with a very simple mono-dispersive crystal shape (a very strong assumption). In addition, the abstract does not mention that the results are rather inconsistent with the theoretical assumptions, which raises doubt on the method appropriateness to the problem.

For these reasons, I cannot recommend publication of the manuscript in its present state.

At the very least, the authors should extend the range of the roughness parameter used in their theoretical computation to the values that are found by extrapolations in the real data analysis. Does the conclusions of the paper remain the same ? Also, there is a need for a better interpretation of the results: In the tropics, the measurements cannot explain the measured polarized reflectances. What is the characteristics of these measurements that cannot be explained by the theoretical simulations ?

Also, according to the authors, the results are significantly different in the tropics and extra-tropics. Is it really because the clouds are different or because the viewing geometry characteristics vary with the sun angle and are therefore different in the tropics and extra-tropics ?

The part of the paper that estimates the noise in the PARASOL polarization measurements si based on the very strong assumption that the polarisation is zero for a scattering angle of 170°. This assumption, and its potential impact on the results is insufficiently described. I also note that the figure that show –P12 for a number of ice crystal models (Figures 7 and 9) do not show the results of the polarized phase function (and its variability) for such scattering angle. As a consequence, one may question the hypothesis. Also, the authors mention a study by Fougnie that estimates the polarized reflectance noise. Why not take the value from this analysis. At the very least, the alternative result should be provided and discussed

Other comments It should be more clear that Abstract, line 13-14: "The present theoretical results are in close agreement with observations in the extratropics but". This is a rather surprising statement as the results in the extra-tropics are clearly outside the range of the theoretical simulations P34286, l 25: It appears that the present study is less advanced than that of Diedenhoven. The study is mentioned in the introduction but the results are not compared. Why ? P34286, l 25: "... it is not suitable for analyzing local variability". This criticism is surprising as the present paper does not analyze the local variability P34289 l 11. I do not quite understand the use of the eta parameter in the equation. Indeed, a signed version of the polarized reflectance is never used in the present paper. Besides, it leads to a rather strange behaviour of the modified polarized reflectivity in the vicinity of 170° (with positive and negative values, but nothing close to zero). P 34290 l 1. "where random variables". X_i are not random variables but measurements ! P 34290 l 2. "because the average polarization". The value of importance here is not the averaged polarization but the actual value. P34291 l1-2: We apply the

same variance to all three POLDER channels used in the analysis (0.865, 0.67, and 0.49 μm). Why not provide the results of this analysis. Are similar values found ?

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