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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Remote sensing of water cloud droplet size distributions using the backscatter glory: a case study” *by* B. Mayer et al.

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Very good paper! I strongly recommend publication! The authors introduce a new remote sensing scheme that enables the indirect measurement of cloud optical thickness, as well as effective radius and width of the upper cloud layer drop size distributions. The important finding is that glory/near-glory measurements can be used to infer cloud microphysical properties (nearly) independently on the overall cloud optical thickness or cloud inhomogeneity. I understand that such measurements are difficult to perform, especially under low sun conditions. However, these findings may encourage future work along this direction. An interesting application may be the active remote sensing of clout bottom microphysical properties with off-beam lidar measurements. A promising extension of this approach may be the use of polarized radiances. I hope the suggestions - most urgent is Major 3) - below will be helpfull for improving the paper.

Major suggestions:

1) page 5: "The background can be used to determine the optical thickness...": .. for a virtual homogeneous plane parallel cloud. The TRUE optical thickness should be systematically larger.

2) page 5 "... surpisingly little noise.", Fig. 6: The variations in optical thickness and effective radius may be quite natural. Why should this be noise? I suggest to make a correlation between the two timeseries (tau, r_eff) to see if the "noise" in these two nearly independent parameters is correlated. If so, the scatter can be attributed to background variability, indeed.

3) page 6: "This sorting by effective radius...": ... also averages out the potential different widths of the size distributions. Do the authors assume that width and effective radius of the SD are correlated, i.e. that SDs with similar effective radii have similar widths? In this case, why should we bother to measure the width? If SDs with same effective radius but different width are possible, than the averaging procedure used by the authors reduces the width information in the glory reflectance pattern! The authors state in the Conclusion that "averaging scan lines with similar effective radii is essential...". However, this statement makes the whole approach questionable if effective radius and width are independent quantities.

Minor:

1) page 2: "A gamma distribution was assumed...": Please specify the range of radii used in eq. (2).

2) Fig. 2, left: The inner dark band is the shadow of the aircraft? If so, please add this to the text as the reader may be puzzled about the missing shadow.

3) e^{-3} -> e^{-3/cos(theta_0)}, where theta_0 is the solar zenith angle.

4) page 4. left column: "Figure 4 shows" -> "Figure 4, bottom, shows"

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5) page 5, left column, bottom: "...expected range." based on what informations?

6) page 6: "Visual comparison of the side maxima...": Blind as I am I can hardly detect significant differences between the 0.8, 1.0 and 1.2 micron curves in Fig. 7 (top). May I suggest to perform a more quantitative approach for selecting the best fitting sigma-curve? Why are the curves in Fig. 7 vertically seperated? I (miss-)understood that sigma and r_eff only change the shape of the glory pattern, not the absolute value.

7) page 7: The whole paragraph "Special features ... (see Figure 5)." seems better suited for the Introduction.

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