Atmos. Chem. Phys. Discuss., 4, S1317–S1321, 2004
www.atmos-chem-phys.org/acpd/4/S1317/
© European Geosciences Union 2004

Atmospheric Chemistry and Physics Discussions

ACPD

4, S1317–S1321, 2004

Interactive Comment

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

B. Mayer et al.

Received and published: 22 July 2004

The referee made a number of constructive comments which will certainly help to improve the paper and for which we would like to thank him. Below are our detailed replies to the points the referee raised. The original referee comments are printed in italic.

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.

True. This is a problem of any cloud retrieval algorithm to date. Radiative smoothing was already mentioned in the conclusions and we added the following statement and reference: “The plane-parallel assumption, made by the retrieval, may also cause an underestimation of the optical thickness (Davis et al. 1997) which affects any cloud

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

© EGU 2004

retrieval to date."

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.

Yes, this is basically what we wanted to say, but obviously we formulated it not very clear. With "surpisingly little noise" we intended to say that the retrieval is robust and doesn't introduce extra noise. The new formulation is "The effective radius shows surprisingly little variation, except for regions where large inhomogeneities occur around the 180° backscatter direction. This demonstrates the robustness of the retrieval."

As a side remark: The suggested correlation between τ and r_{eff} is tricky because (as formulated in the Conclusions) the optical thickness is derived from multiply-scattered radiation and hence subject to the usual 3D effects (for this solar elevation basically radiative smoothing) while the effective radius is derived from a single-scattered radiation and hence not affected by smoothing. Probably one could learn something by carefully interpreting a larger data set.

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.

It is certainly true that averaging reduces the spatial resolution of the width information. In the Conclusions we added the following statement: "However, the averaging procedure implies some loss of information. While effective radius and optical thick-

ACPD

4, S1317–S1321, 2004

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

© EGU 2004

ness are determined at a very high resolution, the natural variability of clouds prohibits the determination of the width of the size distribution at the same spatial scale. This prevents studies e.g. of the variability of the size distribution within a single cloud cell but still provides very useful information, in particular, the width of the size distribution averaged over the field of view of typical satellite instruments."

Averaging is a necessary but possibly acceptable procedure in this case. It is certainly correct that the averaging of scan lines affects the determined widths. However, the averaging area in our case is done over a stripe with 1km width and 10km length (the left part of the scene shown in Figure 2). This averaging area could probably be reduced 5km length or less. The effective radius is retrieved in really high spatial resolution, higher than anything available from satellite observations. The width cannot be retrieved at this very high resolution but still at a useful resolution, comparable to what is available from satellites.

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

This is specified a little later, where the integration over size distribution is actually done: "For the calculation of the optical properties by integrating over the size distribution, Mie calculations were carried out with increments of 0.001 μm in the radius range 0.001 to 30 μm."

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.

Done.

3) $e^{-3} \rightarrow e^{-3/cos(theta\_0)}$*, where theta_0 is the solar zenith angle.*

Done.

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

ACPD
4, S1317–S1321, 2004

Interactive Comment

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

© EGU 2004

Done.

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

Probably an unlucky formulation again. Replaced it by "The optical thickness is also typical for marine stratocumulus." There is also some more discussion added in the Conclusions section comparing the results of the new retrieval with independent data.

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.

We agree that the differences are somewhat hard to detect, but that is probably mostly due to the unlucky choice of line styles and scale. The figure has been changed and should now clearly illustrate what we are talking about. Concerning a more quantitive approach: This is actually the weakest point of the manuscript in our eyes. Finding an objective way for the determination of the width is not at all a straightforward task and we will work on this topic because we want to apply the method to a larger data set and for this purpose we need of course an automatic algorithm, but that is beyond the scope of the paper. In the present manuscript we draw the conclusion that the width of the size distribution is between 0.8 micron and 1.2 micron which is a strong statement considering the 3 micron which are usually assumed (in the C1 phase function). Finally, the curves are separated because we shifted them artificially to improve visibility. This was in principle mentioned in the caption ("shifted by a constant amount") but we changed the formulation, to make this more clear. The referee understood of course correctly that only the shape of the glory depends on r_{eff} and σ, not the absolute value.

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

ACPD

4, S1317–S1321, 2004

Interactive Comment

© EGU 2004

Yes and no. The references to other work could be moved to the introduction, but it forms the basis for the discussion of the optimum flight altitude which again is based on information from the Results section. Hence we decided to leave the section where it is because moving it to the Introduction would probably increase the length of the manuscript unneccessarily because some of the information would need to be repeated.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2239, 2004.

ACPD

4, S1317–S1321, 2004

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

© EGU 2004