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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.

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Anonymous Referee #1 raised a number of issues which we will address when preparing the revised manuscript. Some of his comments helped us to identify potential sources of mis-interpretation and we will try to remove those in the final version. Some of his points, however, are not comprehensible for us and require further clarification without which we are not able to address them. Finally, we disagree with some of his points and hence we take the chance of the public discussion provided by ACPD. Below are the detailed comments to the points the referee raised. The original referee comments are printed in italic.

An algorithm is reported for simultaneous retrieval of cloud optical thickness, particle effective size, and the width of the size distribution of cloud droplets from the backscattering at the 753 nm wavelength. The issue addressed in this paper is interesting. However, the manuscript in its present form needs to be substantially revised before it is formally accepted for publication, *because of many major flaws. Furthermore, the retrieval algorithm is not clearly explained. Even with repeated reading, I still could not understand how the retrieved results presented in this paper were obtained. Below are my specific comments for the authors' consideration in the revision process.*

The manuscript presents the potential of a new method, describing the idea and providing a detailed example of the application to actual experimental data. We are a bit surprised that the referee thinks the manuscript lacks detail. Probably the comment stems from a mis-understanding: the intention of the manuscript was never to present an operational retrieval. Rather, it is a case study which applies a new idea to a data set to demonstrate its potential. Details of the "retrieval method" are therefore provided in the "Results" section while the underlying methods (experimental data, radiative transfer model) are presented in "Methods". Those two sections contain everything required for the reader to reproduce the data presented later in the paper.

(1) The materials contained in the present manuscript need to be substantially reorganized and expanded. Sec.2 (i.e., "Methdos") is the core of this paper. However, the first part of Sec.2 just described how the data were acquired from an air-borne instrument. The second part of Sec.2 essentially describes the forward radiative transfer simulations involved in this study. The retrieval algorithm is not mentioned in Sec. 2! In Sec. 3 (i.e., "Results"), the authors vaguely explained the retrieval algorithm, which, I found, is not easy to understand if not at all. Suggestion: The author should include a new subsection, say, Sec. 2.3, in Sec.2 to explicitly explain the retrieval algorithm. Additionally, sensitivity studies should be carried out to understand the sensitivity of the algorithm to various assumptions/parameters in the forward radiative transfer simulation. Without the sensitivity studies, the results reported in this paper are essentially meaningless.

See above. "Methods" describes the data and algorithms required for the later retrieval study. This is clearly outlined in the introduction. Quote from the manuscript:

> Section 2 briefly describes the CASI instrument and the libRadtran model

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used in this study and outlines the method. In section 3, the technique is illustrated in detail by evaluating a specific observation. In section 4 the results are summarized and discussed.

Therefore the referee's comment is probably due to the misconception that the retrieval is described in "Methods" while it is actually presented in the "Results" section? We try to clarify this in the revised manuscript, maybe by renaming the "Results" section to something more adequate?

In addition, the referee suggests to do sensitivity studies concerning the assumptions/parameters in the forward simulations. One basic assumption is the gamma size distribution which we agree needs to be discussed, see below. Other than that, however, we cannot see further points which need to be studied. The model was run with best knowledge to simulate radiance for the given input conditions (Detailed Mie phase function, well-tested radiative transfer code, 256 streams in the model, etc, as described in the manuscript). The influence of the underlying surface and the background atmosphere (Rayleigh scattering and molecular absorption) was studied and discussed. We showed that at 753nm those can be neglected for our application. Heavy aerosol load above the cloud might influence our retrieval (as it would influence any cloud retrieval). To be able to properly address the referee's point we need to know which other assumptions would make the results "essentially meaningless", if not discussed properly.

(2) Fig.3 shows that the glory reflectivity is essentially independent of cloud optical thickness. This means that there is no sensitivity of the reflectivity to cloud thickness. From first principles, it is impossible to retrieve the optical thickness if the sensitivity does not exist! In short, the retrieval reported in this paper simply violates basic physical principles.

No, it doesn't. Figure 3 presents two graphs, the glory reflectivity (which obviously does not depend on optical thickness) and the average reflectivity which strongly depends on optical thickness, as expected. The optical thickness is determined from the latter

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(Figure 3, bottom) as indicated in the text and in Equation (8). As in any classical water cloud retrieval, the optical thickness is determined from the absolute value of the reflectivity in a visible channel. The only difference in our case is that we average over a certain angular range to avoid noise introduced by the glory (please note that both measurement and model are consistently averaged over the same angular data points). We think this is adequately described in the manuscript. Quote from the manuscript:

> The glory reflectivity $R_{\mathrm{glory}}(\theta)$ is therefore a direct measure of the droplet size distribution while the average reflectivity depends mostly on the optical thickness.

And later in the text, to explain how the optical thickness is obtained from the fit parameters:

> The second term is basically the average reflectivity and is a function of the optical thickness, see Figure 3.

> The background can be used to determine the optical thickness, according to Figure 3: Using the curve for $r_{\mathrm{eff}} = 11.8 \mu m$, an optical thickness of 13.2 is determined.

(3) Are there any in-situ validation for the retrieved showed in Fig. 6? The reported optical thickness seems too low for water clouds.

Our method produced an optical thickness between 8 and 13, see Figure 6. This is low but definitely not too low for marine stratocumulus. In fact, Schueller et al [2003] present histograms of optical thickness during the same (ACE-2 CLOUDYCOLUMN) campaign, using data from a different instrument (OVID) on the same airplane. These values were derived with a “classical retrieval scheme” and are even lower than our

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values on June 26 (this is the very same day). The data, however, cannot be directly compared because (1) OVID is a nadir looking instrument which does not provide the 10° off-nadir direction observed here, and (2) the glory direction should be excluded from the "classical" analysis anyway because the glory feature leads to errors if not explicitely considered.

In-situ data are presented by Brenguier et al., Radiative Properties of Boundary Layer Clouds: Droplet Effective Radius versus Number Concentration, JAS 57, 803-821, 2000. The profile of effective radii provided in Figure 1c of this paper shows encouraging agreement with our newly retrieved results, considering that the retrieved effective radius is representative of the upper part of the cloud. We will try to include more material into the revised manuscript. However, in particular optical thickness is hard to validate because the in-situ measurement requires averaging over aircraft ascents and descents. Considering the natural variability of a marine stratocumulus, in-situ and remote sensing observations might only be comparable when averaged over larger flight distances.

(4) Eq. (1) and associated discussions: Hansen and Travis (Hansen and Travis, 1974: Light scattering in planetary atmosphere. Space Sci. Rev., 16, 527-610) showed that the bulk optical properties of water clouds depend on the effective size and also on the effective variance. The latter was not discussed in this manuscript. The authors might want to cite Hansen and Travis' paper.

Hansen and Pollack (1971) is already referenced, but we will add a reference to Hansen and Travis (1974) although this does not add much extra information. The latter paper shows that, for a size parameter larger than about 40 (which corresponds to a droplet size of 5μm for wavelength 753 nm), the optical properties do not depend on the effective variance for all practical purposes.

Actually, Hansen and Pollack (1971) provide a nice motivation for the method presented in our manuscript. Quote from their paper:

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> We have made preliminary computations to find the effect of changing the shape of the size distribution; the results for several different distributions indicate that the volume extinction, the single scattering albedo, the asymmetry factor, and the shape of the phase function (outside the region of the glory) depend mainly on the mean particle radius for extinction.

The "mean particle radius for extinction" approaches the effective radius for large particles and this result has been confirmed by several authors afterwards. It is interesting to note that more than 30 years ago the authors already implicitely mention that the glory region contains information on the size distribution.

(5) The retrieval of the width of the size distribution is quite questionable. The author assumed the gamma size distribution for the forward radiative transfer computation. In realty, the size distribution in cloud is not an idealized gamma function. If the authors assume a different size distribution function, say, the power size distribution, the reorganized retrieval result may be different. Thus, sensitivity studies should be carried out to understand the effect of the form (e.g., the gamma distribution or the power law distribution) assumed for the size distributions.

This is in fact an interesting point and it led to some discussion among the authors. It is certainly true for our method (and for any other remote sensing method known to us) that the detailed shape of the size distribution cannot be retrieved and has therefore to be assumed a-priori. The simple reason behind this is that an adequate sampling of the size distribution (in the sense that one may distinguish if the shape is gamma, log-normal, or exponentatial) requires probably 10 or more independent data points. On the other hand, from an optical measurement, usually only two or three pieces of information is available: In a "classical" cloud retrieval only two indepedent pieces of information are available (e.g. reflectance in an absorbing and a non-absorbing channel) which allows determination of not more than two parameters, optical thickness and effective radius. In our case, we have three independent pieces of information, the averaged reflectivity, the distance of the glory maxima, and the shape of the glory

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(basically the number and amplitude of the side maxima). This allows us to retrieve one parameter more than in a classical retrieval, namely the width of the distribution. In consequence, the retrieved width will of course depend on the choice of the size distribution.

Each retrieval has to make a variety of assumptions. In the model and retrieval world clouds are usually assumed to be plane-parallel and vertically homogeneous. In reality, however, they are rather complex three-dimensional distributions of water and ice droplets and particles. A variety of assumptions is required to narrow the parameter space down because classical retrievals usually provide only two independent parameters (e.g. vertically integrated optical thickness and an effective radius which is usually representative of cloud top). These assumptions include the homogeneity of the cloud over the field-of-view of the instrument, an assumption of the particle shape in the case of ice particles, assumptions about the underlying surface and background atmosphere etc. A variety of papers has been published on cloud property retrievals, with most of those not even mentioning the underlying assumptions. In our case, we needed a reasonable assumption of the size distrution and choose the gamme distribution because it is probably the most common approximation for a droplet size distribution. As Figure 1 shows, the gamma function is rather flexible and should be a good approximation of natural droplet size distributions. The result of our retrieval is tied to the underlying gamma assumption, and in the unlikely case that the shape of the droplet size distribution cannot be reasonably approximated by a gamma distribution (e.g. for a bi-modal droplet size distribution which one would not expect for cloud droplets) the result is not valid. Or let's put it this way: Each cloud retrieval provides optical thickness which is only valid under the assumptions that the cloud (a) is horizontally homogeneous; (b) is single layer; (c) contains only water or ice but not both; (d) that the ice particles have a certain shape. These assumptions are almost certainly not strictly fullfilled. Nevertheless, the retrieved data are extremely useful if these assumptions are kept in mind during interpretation of the results. The only additional assumption (as one of many) of our retrieval is that the size distribution can be approximated by a gamma function which is highly likely.

We could possibly test other assumptions of the size distribution like e.g. the log-normal distribution. The suggested power-law is probably not a good choice because neither effective radius nor width are defined for the latter. In any case, we thank the referee for raising this issue and will discuss it in the paper.

(6) What is the physical basis for introducing the "glory reflectivity"? The quantity defined in Eq. (6) depends on theta (this quantity is not defined in the manuscript though I assume that it indicates the scattering angle). The quantity defined in Eq. (7) is independent of theta. So, why the authors call the difference in Eq. (7) the "glory reflectivity"?

Maybe the referee is confused by our notation of the average, $< R(\theta) >$? From our forward simulations we simply found that the “wiggles” in the glory region always had the same amplitude, indepedent of the optical thickness. Hence, we separated the “wiggles” from the total signal by subtracting the average reflectivity. As Figure 3 (top) shows, the “wiggles” are in fact completely independent of optical thickness if the latter is larger than 5. The reason for this is given in the text: The glory is a single scattering phenomenon. On the other hand, the averaged reflectivity is also not useless, see Figure 3 (bottom), because it carries the information on optical thickness. Separating the simulation into “glory reflectivity” and “average reflectivity” allows us to indepedently retrieve optical thickness (from the average reflectivity) and effective radius as well as width of the size distribution (from the glory reflectivity).

And yes, a constant number or offset (average reflectivity) is subtracted from an angular dependent quantity (reflectivity) to get another angular-dependent quantity, the “glory reflectivity”. In the revised manuscript we will try to make that even more clear. Maybe, a third plot in Figure 3 would help which shows the original reflectivity (= average reflectivity plus glory reflectivity)? We had this plot in an earlier version of the manuscript but removed it because it did not provide extra information.

(7) Many variables in the manuscript are not defined. This makes it quite difficult to read the

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manuscript.

Anonymous referee # 1 already mentioned this in his initial review. We tried to address this comment but the only undefined quantity which we could identify was the N in Equation (7) the definition of which we added. It is also true that θ was not explicitely defined in the text (see referee comment 6) because it should be quite clear from the context that θ is a polar angle (Figure 2 says "Scattering angle θ" and we will insert the symbol θ in Figures 3 and 4) and try to clarify the text. Apart from that, however, we could not identify any important parameter which we forgot to define. Could the referee please help us?

(8) Eqs. (8) and (9) and the associated discussions are not understandable. Are they the key equations for the retrieval algorithm? Why should sigma and c in Eq. (8) be unity?

Yes, Eqs. (8) and (9) are in fact the key Equations for the retrieval algorithm. Equation (8) is simply a definition of the function which we fit to the data. A constant b plus a linear term $a \cdot \theta$ plus a constant c times the glory reflectivity which has been pre-calculated for a set of effective radii and widths of the size distribution. This is explained in the text following Equation (8). Equation (9) is the cost function to be minimized in order to do the linear regression and I think most authors would not even bother to mention it. We try to make the text even more clear.

The one thing which might be confusing is the notation $R_{\text{glory}}[r_{\text{eff}}, \sigma](\theta)$. It is explained in the text; quote from the manuscript:

> From a pre-calculated set of functions R_{glory} for various combinations of r_{eff} and σ the one is chosen which minimizes the cost function ...

but we should probably have explained explicitely that we have simulated the glory (as a function of θ) for a variety of effective radii r_{eff} and σ, converted those into glory reflectivity according to Equation (6), and stored them. $R_{\text{glory}}[r_{\text{eff}}, \sigma](\theta)$ therefore simply means the model-derived glory reflectivity for given effective radius and width sigma, as

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a function of θ. For each of the tabulated functions, we determine the fit parameters a, b, and c (Equation 8) and then we select this combination of r_{eff} and σ which minimizes the cost function, Equation 9. Is it ok if we formulate the text in more detail, as outlined above?

Finally, the text does not state that sigma and c in Equation 8 should be unity, but only that c should be unity. Quote from the manuscript:

> The third term is the glory, and as the amplitude of the glory reflectivity should only depend on r_{eff} and σ, c would ideally equal 1.

This text already explains why c should equal 1: The glory reflectivity, as calculated by the model should equal the glory reflectivity provided by the model. In principle there is no need to introduce a fit parameter c. However, we introduced it to capture possible uncertainties, e.g. calibration uncertainties, deviations of the viewing directions from the exact backscatter direction etc which would affect the amplitude of the glory reflectivity. In the revised manuscript we will additionally state that c is a measure of the quality of the retrieval: If c would differ from 1 significantly, we would know that there is a problem either with the forward simulation or with the experimental data. This, however, is not the case.

(9) The manuscript should be carefully edited. There are some missing words in the manuscript. For example, the bottom line in the first page: "with give size" should be "with a given size".

The mentioned example is questionable but we will ask a native speaker to check the manuscript.

(10) Two lines below Eq. (1) "The reason for this": Actually, the effective size is the mean path-length of the incident rays inside the particles. Note that when the size parameter is large, geometrical optics is valid and the incident wave can be regarded as a bundle of rays. For a large size parameter, the extinction and absorption properties of the particles are largely determined by the mean path-length - the physical basis for van de Hulst' ADT theory.

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This is probably true but we don't think that this explanation is more enlightening for the reader than the one provided in the manuscript. It is not immediately evident that the effective size is the mean pathlength and an explanation would be required why this is the case.

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