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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "A spectral method for retrieving cloud optical thickness and effective radius from surface-based transmittance measurements" *by* P. J. McBride et al.

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1 General remarks

The paper introduces a new retrieval method for cloud optical thickness and effective radius. Employing spectral transmissivity measurements a new proxy for the effective radius was found. The authors suggest to use the spectral slope between 1565 nm and 1634 nm wavelength instead of the transmissivity at a single wavelength (1628 nm). Several benefits of the new method are highlighted. Especially for clouds of low optical thickness the new method significantly improves the separation of simulated cloud transmissivities for different effective radii. The uncertainty of the new retrieval is discussed theoretically and by analyzing spectral radiation measurements. A comparison of retrieved cloud properties for two different case studies shows an improved agreement with a microwave radiometer and the MODIS retrieval compared to a common two wavelength retrieval.

The new retrieval method is worth to be published as it might be groundbreaking for new applications of ground-based spectral radiation measurements. However, the paper is poorly written and thus hard to understand for the reader. Some passages are redundant and some put in the wrong section of the paper, which has been frustrating for me as a reader. Furthermore, discussions on the new retrieval method have to be expanded and comparisons with other retrieval methods have to be more meaningful. I am sure that changes in the structure will help to point out the main conclusions of the paper more clearly.

As this paper might become a reference for the proposed new retrieval approach, I strongly suggest to address the following major issues before publication.

2 Major comments

P1056, Introduction: In the introduction you are jumping from topic to topic. Improve your argumentation by resorting the passages or moving them into the main sections. **P1056 1-16)** detailed description of retrieval techniques...move to section 3. **P1058 1-26)** detailed description of data processing...move to Section 3 and just briefly outline the content of your paper.

P1066, Section 3: When introducing the retrieval techniques you compare retrievals based on transmissivity measurements with retrievals based on reflectivity measurements. This is comparing apples and oranges. There is no instrument which is capable to measure both transmissivity and reflectivity at the same time. So you have no choice

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between both retrieval methods. Furthermore, you are about to introduce a method for ground-based measurements of transmissivity. So why explaining a retrieval for reflectivity. This is confusing the reader. I suggest to remove all passages dealing with the reflectivity retrieval including figure 3a, 6.

What should be done instead is to focus more on the comparison between the two-wavelength ground-based retrieval and the new spectral ground-based retrieval. This is comparable and differences have to be pointed out more clearly.

P1066, Section 3: The titles of the subsections are inadequate. The content is not ordered well. I recommend to rearrange section 3 as follows: 3.1. Two-Wavelength Retrieval... 3.2. Spectral Retrieval... 3.3. Uncertainties. **P1066, 14:** this passage explains how you have calculated your model input. Move this into section 2.7 radiative transfer model. **P1074, 6 - P1075, 18:** This passage describes the data-processing of your measurements. I recommend to move this discussion into a separate section (or include in 4.) In 3. only the theory of your retrievals should be discussed. **P1070, Section 3.3.:** The content of this section has nothing to do with the section header. Remove this section or move only the text to the begin of section 3.

P1069, 10: You interpolated your original transmissivity simulations to a grid of higher resolution $\Delta\tau = 0.1$, $\Delta R_{\text{eff}} = 0.1\,\mu$m (Sec. 2.7). You used this new grid as a lookup table and applied a least square fit to find the best τ and R_{eff}. I think you can do better. As you already interpolated you simulations, why don't you interpolate the original grid for exactly the given(measured) transmittance values? Then you don't have to apply a least squares fit. Your result will be more exact than sticking to the predefined values of the fixed grid. Except for the equations, the uncertainty estimation will be almost unchanged. You still can compute the range of τ and R_{eff} by varying the measured values.

P1072, 9: The idea to use a spectral slope comes out of the blue. Please explain, which physical processes gave you the idea. Just that the co-albedo has a spectral slope is no convincing excuse. Furthermore, with regard to Figure 4, I would state that the spectral slope of the co-albedo doesn't vary with particle size. Only the magnitude varies. It is the asymmetry parameter, where the spectral slope varies! So explain, how the co-albedo causes the different spectral slopes in the transmissivity.

P1073, 13: Figure 7 is the central plot of the entire paper. I could not find any discussion of Figure 7 in the text. Without a detailed discussion of this plot, the reader will not understand the new spectral method and how it improves the retrieved cloud properties. So please explain what we can see in Figure 7 and compare it in detail to Figure 3b (two-wavelength method).

P1075, Section 4: Don't separate the two campaigns with individual sections. This complicates the comparison of both cases. I suggest the following outline: 4.1. Retrieval Results 4.2. Uncertainties 4.3. Time Series.

P1076, 3: Why do you assume the air in this area to be heavily polluted? If there is wind from the Atlantic there might be relatively clean air. This assumption has to be justified or removed. Furthermore, in line 15, you argue that the retrieved R_{eff} fits to you assumption of polluted air. This can not seriously be a validation of your retrieved R_{eff}. Are there any aerosol concentration measurements which would support this vague assumption? I suggest to remove this hypothesis unless you present any supporting data, trajectories, etc.

P1076, 12: Do you have any explanation, why the slope method retrieved smaller average effective radii than the dual-wavelength retrieval? In Figure 10, it looks as if the dual-wavelength method generally cannot produce an Reff small than that retrieved by the slope method. Is this because of the general differences in the two methods and can be explained in principle? Or may this be the result of calibration uncertainties, offsets? The slope-method is independent of calibration, while the dual-wavelength method is not?

P1077, Section 4.3: What was done here is in general a repetition of the uncertainty

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analysis from Section 3. What is the additional value of this analysis compared to the uncertainties determined theoretically in Section 3? What does this study tells us about the real uncertainty in the retrieval? Why do the uncertainty values derived here do not agree with the theoretical values from section 3? I suggest to remove this analysis. Instead, it would be more interesting to plot the differences between two-wavelength retrieval and the spectral slope retrieval. Plotting the difference in dependence to R_{eff} and τ may help to identify the discrepancies as described in my last point.

P1079, 21, Figure 16 and 17: There is only one MODIS data point. Plotting a time series is pointless. Remove both figures and instead show a table with the corresponding values. This is absolutely sufficient.

3 Minor comments

P1056, 3: Equation holds only for spherical particles. Use the general definition $R_{\text{eff}} = 3/2 \cdot V/A$ (see Mitchell, JAS, 2002 or McFarquhar and Heymsfield, JAS, 1998)

P1056, 7: specify the wavelength range

P1056, 11: for which τ is the radiance maximum?

P1057, 27: "continuous" spectrum is misleading. You still measure on a discrete wavelength grid.

P1061, 7: -10 °C

P1064, 10: Your retrieval is based on 1650 nm wavelength. I suggest to use 1640 nm for MODIS too. OK, on AQUA 1640 nm does not work properly. But at least show how the MODIS results for 1640 nm would differ.

P1065, 1: Why don't you simulate from 0 to 1? 0.95 corresponds to 18°. A lot of solar zenith angles are missing. Is this adapted to your case studies or should it represent a

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general lookup table for all solar zenith angles? Then please add 1.0.

P1065, 2: "any cloud properties". Please specify.

P1065, 13: This seems to be a crude assumption to derive the surface albedo. Considering the retrieval method presented by Chiu et al., JGR, 2010, where surface albedo spectral variety is used to retrieve cloud properties, please add a discussion, on how surface albedo uncertainties may effect your retrieval.

P1066, 7: "reflectance" and "transmittance". Both are defined as non-dimensional quantities. As such, they should be called reflectivity and transmissivity, because "reflectance" is "reflected irradiance" in units of $\mathrm{W\,m^{-2}}$.

P1067, 5: In the previous sections, the transmissivity was defined as quantity of interest. Why do you now show radiance in Figure 2? Please change to transmissivity.

P1067, 10: The sensitivity to the effective diameter is not discussed in the following sentences. Rather the dependence on cloud optical thickness at 512 nm wavelength is described.

P1068, 23: "largest optical thickness". Please specify.

P1069, 8: Please explain in more detail. A large range in the retrieval results corresponds to a low sensitivity of the retrieval.

P1070, 8, Table 1: There is no column for the new retrieval method in Table 1. Especially, the uncertainty of the new method is what should be highlighted.

P1070, 18: There is no discussion on the retrieval sensitivity with respect to the cloud optical thickness. Please add.

P1071, 20, Figure 6: There is no need to show this plot. You don't measure the cloud albedo with ground based instruments.

P1071, 6, Figure 5: Plots for $R_{\mathrm{eff}} = 5\,\mu$m and $R_{\mathrm{eff}} = 25\,\mu$m are hard to distinguish.

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Please remove the color filling, or use different colors for negative and positive differences between $R_{\text{eff}} = 5\,\mu$m and $R_{\text{eff}} = 25\,\mu$m.

P1072, 25: You first claimed that you normalize the transmittance and then you have again a problem with absolute values. I don't understand.

P1074, 6: The constraint to liquid water clouds only holds for your measurements and simulations. There is no general reason, why you can not repeat the calculations for ice clouds.

P1074, 15, Figure 8: Which data base is shown here? How you explain the sharp edges in the refractive index of liquid water? However, I suggest to show the differences in the slope using simulations of cloud transmissivity. Similar to Fig. 6. The refractive index alone does not tell me that you will see this slope in the measurements. Radiative transfer is a non-linear process!

P1075, 8, Figure 9: Use for each plot the same axis ranges (irradiance and time), otherwise the plots are not comparable. Mark the time ranges you finally used in your analysis.

P1075, 24: How do you calculate liquid water path from your measurements?

P1076, 16: "library". Earlier you called it look up table. Please stick to one expression. The same holds for the names of the retrieval methods. Two-wavelength, Dual-wavelength,... choose one.

P1079, 6: "quite different". 3° and 5.7°, this is not a big difference. The sampling rate is more crucial. Does the MWR average over the 16 sec?

P1081, 12: Add a discussion on the general differences between two-wavelength and slope-method.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1053, 2011.