We thank the reviewer for taking the time to read and comment on the manuscript. The suggestions regarding the structure and flow of the paper have resulted in better organization. Comments about the refractive index plot (Figure 8) resulted in a better and more relevant plot being presented in its place. Holes in the discussion were pointed out, such as the discussion of the sensitivity to optical thickness and the theoretical development behind the use of the slope.

Listed below are the reviewers comments (italicized) and our responses.

Reviewer #2

1. *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.*

We rearranged the introduction into the following order: (1) Importance of studying clouds (2) Cloud retrievals with reflectance (3) Cloud retrievals with transmittance. The section of the Discussion paper on page 1056, lines 1-8 were combined with page 1056, lines 17-25 and page 1057 lines 1-5 were combined to make up the new paragraph (2) about reflectance. Page 1056, lines 9-16 were combined with page 1057, lines 6-23 to form the new paragraph (3) about cloud transmittance. The passage on page 1056, lines1-16 discusses generally how cloud retrievals are done with cloud transmittance and reflectance. It is important to have this information in the introduction so that the issues that are covered in the literature can be discussed. We removed some of the details from page 1058 (lines 6-16) but have left the rest to serve as an introduction to the algorithm presented.

2. *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 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.*

We agree with the reviewer that the way in which this comparison was presented may have been confusing. However, we decided to keep the description of the reflectance-based retrieval, for the following reasons: (1) The comparison to the reflectance retrieval uncertainties provides some context for cloud retrievals by comparing to something the readers will be more familiar with. In the revised version, we do make it clear that retrievals based on transmittance and reflectance are apples and oranges, as Reviewer #2 points out. The concern would be that the reader would be confused without the comparison with reflectance, thinking that the problems are the same. This discussion motivates the need for a new algorithm (i.e. one cannot simply apply the standard reflectance method to transmittance). (2) The point is also to show that simply taking the reflectance-based algorithm and applying it to cloud transmittance, does not work. (3) We think it is very informative to see the differences of information content in spectral reflectance as opposed to spectral transmittance, and to explain why only little information about size is carried in the effective radius. This was clarified in the text by adding to and elaborating on the points in the first paragraph of section 3. We revised the general description of reflectance/transmittance retrieval in the hope to be less confusing to the reader and to reflect all of the above points (1-3).

3. 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.

We made the introductory paragraph (paragraph 1) of Section 3 more explicit about why this section is ordered as it was in the discussion paper. We also combined Sections 3.3 and 3.4 because the content should be linked together. Sections 3.1 and 3.2 are there to motivate the need for a retrieval algorithm unique from that of methods used with cloud reflectance. Section 3.1 discusses how retrievals using reflectance (two-wavelength method) are done and discuss without quantification why the same method is inadequate for use with cloud transmittance. Section 3.2 introduces the uncertainty calculations as a method to quantify the (in)sensitivity to optical thickness and effective radius in reflectance and transmittance. Section .3.3 introduces the physical properties used in the development of the slope retrieval and finally section 3.4 introduces the new algorithm. The subsections were renamed to better reflect their content.

4. 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.

These are both good points and I've moved the passage on page 1074, line 6 into a paragraph added to section 2.7 added at page 1065 line 23 of the Discussion paper. The data processing sections have been moved from Section 3 and into a new Section 4.1.

5. 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.

Section 3.3 was incorrectly titled. As we pointed out in a previous comment, we did combine Sections 3.3 and 3.4. The new combined section has a title of 'Spectral

Transmittance dependencies on optical thickness and effective radius'. The new section discusses the Mie calculations of asymmetry parameter and coalbedo and then how these affect the transmittance for a range of optical thickness and effective radius.

6. P1069, 10: You interpolated your original transmissivity simulations to a grid of higher resolution = 0:1, Re = 0:1 m (Sec. 2.7). You used this new grid as a lookup table and applied a least square fit to find the best and Re. 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 and Re by varying the measured values.

The results show that the uncertainties in the retrieved effective radius are between 6% and 12%. For an effective radius of 5 µm, the uncertainty is between 0.3 and µm and 0.6 µm. We don't believe that increasing the resolution through interpolating the observation would gain that much in terms of a more exact solution. Also, if we understand the suggestion correctly, the processing would then require interpolating each observation and would increase the processing time of the retrieval algorithm. By interpolating the lookup table, this part of the processing is a one-time hit. The processing time would be a minor point if the result were a more accurate answer, but we believe that when the uncertainty is taken into consideration that this is not the case. In the end some test of goodness of fit is needed and we believe that the least squared fit is sufficient.

7. 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.

Thank you for pointing this out. We agree with the reviewer's statements about the coalbedo and asymmetry parameter and their spectral dependence. In order to clarify the benefits of the spectral slope in the transmittance and elaborate on the origins of the idea, the plot below was added along with further discussion of the role of coalbedo and asymmetry parameter in producing this shape.

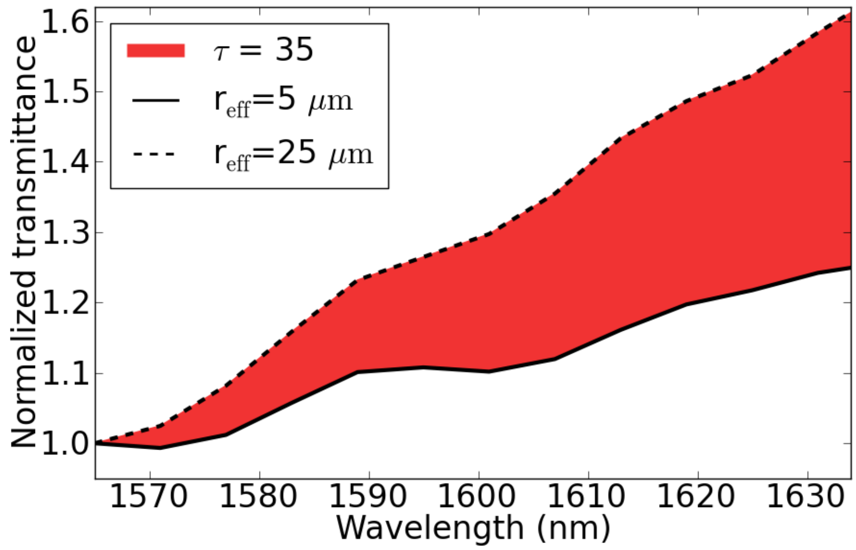

Figure R1: The spectral shape of the normalized transmittance for two effective radii and one optical thickness over the range where the spectral slope is fit.

8. 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

Discussion of this plot was brief and we added paragraph to Section 3.5 (page 1072, line 21 of the Discussion paper) to cover this in more detail.

9. 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.

This is a good suggestion as there are points that need to be repeated in each section that can be made just once. We created a new section 4.1 (per one of your previous suggestions), so the order is as you suggest, but the outline is 4.2 Retrieval results and so forth.

10. 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 Re fits to you assumption of polluted air. This can not seriously be a validation of your retrieved Re. 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.

This is speculative, as the reviewer points out, so we removed this from the text. For clarification: This statement was not intended as a validation for the retrieved effective radius.

11. 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?

This is an interesting question, but we do not presently have an explanation for this behavior. In the discussion of Table 1 and Figure 3(b) it was shown that the dual-wavelength method was not expected to be sensitive enough to retrieve the effective radius under all conditions. It is beyond the scope of this paper to explain all of the sensitivities of the dual-wavelength method other than to show that it is not sufficiently sensitive to effective radius for use in an operational retrieval. We will, however, address this question in the next paper, which we are currently working on. We will add a statement about this in the revised version, pointing out the referee's observation, and stating that there's currently no explanation for this behavior.

12. P1077, Section 4.3: What was done here is in general a repetition of the uncertainty 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 Re and may help to identify the discrepancies as described in my last point.

The uncertainty analysis presented in Section 3 is different from that in Section 4.3. In Section 3, we explored the uncertainties/sensitivities in the existing cloud retrieval techniques (current status) whereas in Section 4.3, we focus specifically on the new retrieval. We will clarify the reasons for this separation in the revised manuscript.

13. 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.

We agree. We removed figures 16 and 17 and added a table with the coincident data points.

14. P1056, 3: Equation holds only for spherical particles. Use the general definition Re = 3=2 ▯ V=A (see Mitchell, JAS, 2002 or McFarquhar and Heymsfield, JAS, 1998)

We agree. However, for now, the retrieval is strictly limited to the liquid phase. Using the microphyiscally defined formula by McFarquar and Heymsfield would work, but also be slightly confusing as we are in fact dealing with spherical droplets. In the discussion paper we mentioned Mie calculations but we did not mention explicitly that we assumed spherical particles so we added that in the discussion of the radiative transfer model (Section 2.7).

15. P1056, 7: specify the wavelength range

Done.

16. P1056, 11: for which is the radiance maximum?

We added this value to the text.

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

It is true that it is not truly continuous so we replaced this occurrence and one other (page 1072, line 8) with “contiguous and overlapping.”

18. P1061, 7: -10 C

We fixed this. Thanks for pointing that out.

19. 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.

The MODIS retrieval presented in the MOD06 product is a retrieval using the 1.6 µm channel and the 3.7 µm channel. This retrieval is presented as a difference from the standard MODIS retrieval which uses the 650 nm (over land) and the 2.1 µm. This is a slightly different retrieval which uses an absorbing channel and a more absorbing channel. This comparison is less relevant than if it were done with 650 nm and the 1.6 µm channel. That being said, the difference was reported for only one of the overpasses (~17.2 UTC, Figure 17 in the Discussion paper). For this case the effective radius was reported as 11.3 µm as opposed to 11.37 µm for the standard MODIS retrieval. With this limited data, we decided to forego a detailed MODIS/SSFR intercomparison. A more systematic comparison is under way in the next paper. We will add this in "outlook/future work" in the revised manuscript.

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

We calculated up to a value of 1 as suggested. The shorter range presented in the Discussion paper was motivated by the values of SZA encountered in the specific cases described in the paper.

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

We changed this to cloud optical thickness and effective radius.

21. 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.

It is true that the assumption of the surface albedo is not ideal as we have pointed out in Section 2.7. We added a paragraph discussing possible effects from any discrepancies between the observed and modeled surface albedo (page 1065, line 23). In response to another reviewer (#3), the plot of the surface albedo was changed to make it more clear what data was actually used in the simulations.

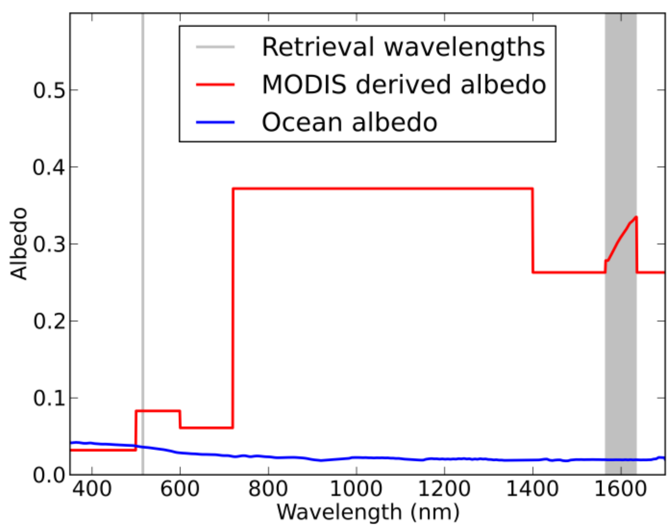

Figure R2: The surface albedo values used in the radiative transfer model. The 'vegetated' albedo derived from MODIS and the USGS grass albedo is in red and the ocean albedo from Coddington et al. (2010).

22. 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 Wm 2.

We were unable to find a definition of reflectance and transmittance with units of irradiance (W/m2); in fact, we found conflicting definitions for these terms. Since there is no unique definition for these terms, we defined them when they were introduced. Our definitions are now as follows: reflectance: dimensionless reflected radiance, normalized by incident irradiance at TOA, transmittance: dimensionless transmitted radiance, normalized by TOA incident irradiance at TOA, both specified in equation 4.

23. 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.

Although we agree with the reviewer that this might make the flow more consistent, transmitted radiance is not a commonly used observation for use with cloud retrievals and it is important to show its behavior without normalizing first. We added a line to the description stating that we are showing the transmitted radiance because it is what we use to calculate transmittance.

24. 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.

This paragraph was inadvertently separated into two and the following paragraph has most of the effective radius content. We corrected this mistake.

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

It begins to approach complete attenuation at an optical thickness of 80 (this can be seen in Figure 3). We added this to the text.

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

Good point. We added text explaining this more explicitly.

27. 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.

The uncertainty comparisons for the new method are included in the results (Section 4). The actual uncertainties are more important than the theoretical calculations used to motivate the need for a new algorithm.

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

Thanks for pointing this out. We added this discussion to the last paragraph of section 3.2.

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

We agree that this plot is unnecessary. This figure was used as another way to put the transmittance algorithm in context with the reflectance algorithm. This has been established so we removed this figure.

30. P1071, 6, Figure 5: Plots for Re = 5 μm and Re = 25 μm are hard to distinguish. Please remove the color filling, or use different colors for negative and positive differences between Re = 5 μm and Re = 25 μm.

We changed the colors to lighter shades so that the black (effective radius) lines are more visible.

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

The explanation in the discussion paper was attempting to explain one reason the slope was fit to the normalized transmittance as opposed to the transmittance itself. We don't think this explanation is necessary to the understanding of the technique and if it has caused some confusion in the way that we presented it, it seems worthwhile to remove it. In its place I've added a much simpler explanation of the benefits of the normalization.

32. 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.

We added a phrase to indicate that this constraint is just for this study.

33. 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!

This data was taken from Warren,S.G, Optical Constants of Ice from the Ultraviolet optical thickness the Microwave. Applied Optics,23,1206-1225, 1984 and we have added this reference to the manuscript. This data does rely on interpolation to meet the 1 nm spectral resolution and this is the origin of the sharpness edges. To check if this interpolation posed a problem, we plotted data from Wieliczka et al. 1989 which was taken at a much higher spectral resolution. We added this uninterpolated data to Figure 8 of the Discussion paper and included it here (the Wieliczka data will not appear in the paper). In addition we restricted the x-axis to the spectral region used for the phase discrimination (1667 nm to 1695 nm). We also changed the imaginary index of refraction to the bulk absorption coefficient which is the more pertinent parameter (See Pilewskie and Twomey, 1987). The Figure R3 shows two things: (1) The bulk absorption coefficient is flat through this region and (2) the interpolated values compare well with the uninterpolated values. We also calculated the slopes for liquid water clouds for the values of optical thickness (0.1 to 100) and effective radius (1 µm to 30 µm) in the lookup table and all were negative. Given this and the shape of the refractive index of ice, if the slope of the transmittance is positive there must be ice present. Figure 8 of the Discussion paper has been replaced with Figure R3 without the Wieliczka data and the text has been updated to reflect this.

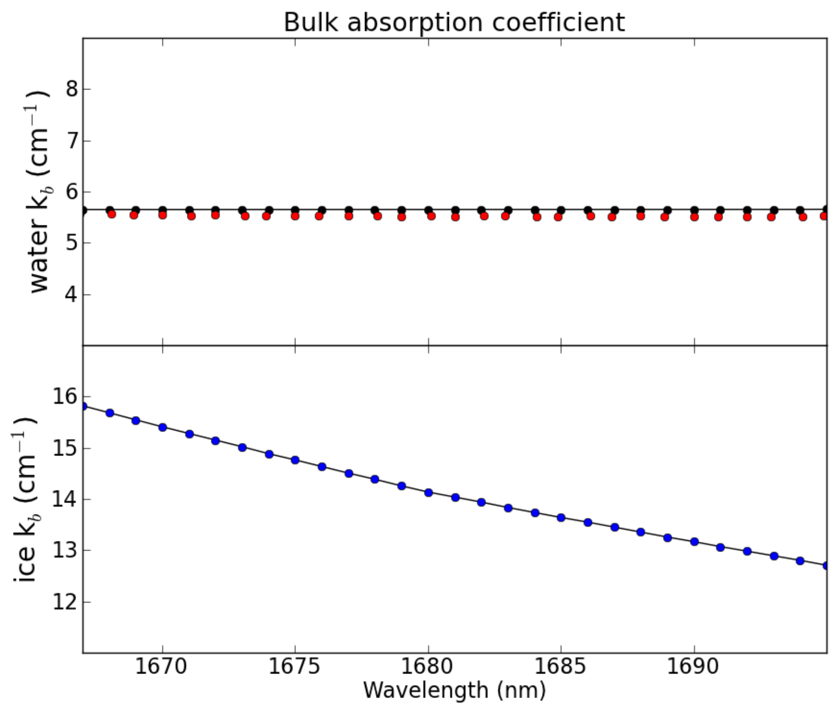

Figure R3: Bulk absorption coefficient of ice and water. Red points are uninterpolated data from Wieliczka et al. (1989) and black points are interpolated data from Warren (1984).

34. 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.

We agree. The y-axes (irradiance) for all three cases have been changed. The x-axes are the same for the two cases from the ARM site as they were intentionally chosen to be over the same range of time (and SZA). We are, however, leaving the axes from the ICEALOT case the same as the extended time shows the type of time periods that were avoided. We also added markers indicating the time periods as suggested.

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

We added a reference here back to Equations 2 and 3.

36. 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.

We changed all of these occurrences to lookup table.

37. 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?

I've removed the "quite" and added the length of the observation times to the text.

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

We added a short discussion of the two methods in the lines preceding the discussion of the results (page 1081 line 14 of the Discussion paper).