We thank the reviewer for taking the time to read and comment on the manuscript. The suggestions regarding the discussion of the sensitivity to the effective radius were most helpful and have strengthened the presentation of the data. My future research will benefit as well.

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

Reviewer #3

1. p. 1054, lines 25-28. I don't think that for the thicker clouds, the difference in uncertainty levels between 12.8% and 8.9% is significant enough to be mentioned in the abstract. For thinner clouds, at least for the dual wavelength method, the uncertainty level is misleading. The average retrieved effective radius of 17 um ±21% means that most of the retrieved effective radii are between 13 and 20 μm; this is incorrect if we look at Figs. 10c and 10d.

This is a good point. To address this and some of the points that follow, we defined a 'valid' retrieval as one that results in an effective radius uncertainty less than 2 μm. When considering this limit, the averages change from 17 μm ±21% to 9.2 μm ± 16.0% for the dual-wavelength method. Though this does not appear to be more representative of figure 10 in the Discussion paper, we changed these plots to include the histogram of the valid retrievals of effective radius. This also changes the average uncertainties from 12.8% and 8.9% to 11.7% and 8.6% for the thicker cloud cases at the SGP. The effect of choosing 2 μm as a threshold is seen in Figure R1d where the dashed lines represent these 'valid' retrievals and the solid lines show all the retrievals. The shape of the distribution of the dual-wavelength and slope methods is very similar when choosing 2 μm as a threshold.

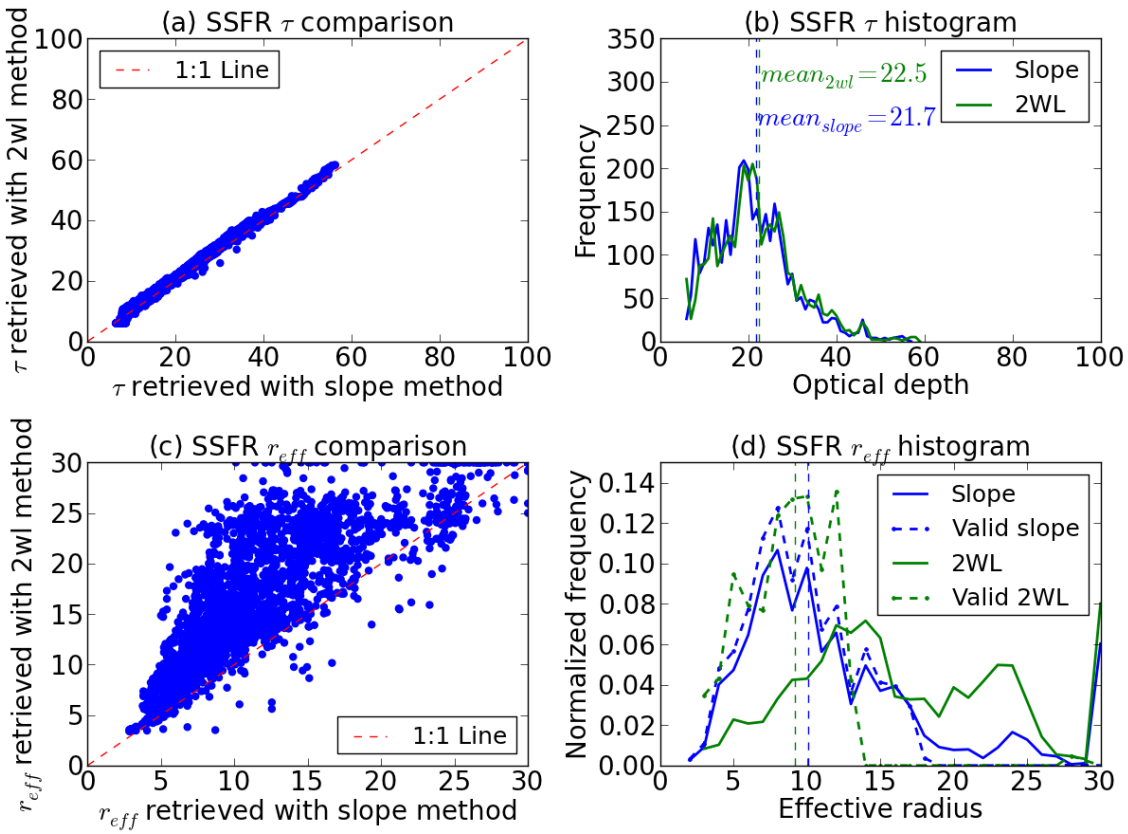

Figure R1: Cloud parameter retrieval results comparing the slope method to the dual-wavelength retrieval method. Shown are (a) scatter plot of the retrieved optical thickness for the two methods, (b) a histogram of the retrieved optical thickness for both methods, (c) scatter plot of the retrieved effective radius for the two methods, and (d) a normalized histogram of the retrieved effective radii for both methods showing all retrievals (solid) and valid retrievals (dashed).

2. p. 1056, lines 12-13. It is not quite true anymore. The AERONET cloud mode (see, http://aeronet.gsfc.nasa.gov/cgi-bin/type_piece_of_map_cloud and Chiu et al., 2010) can be mentioned here.

We added the phrase “at a single wavelength” to clarify what we are referring to. Chiu et al. present a solution to this and we cite that paper later in the text (page 1057, line 16) and point out that a solution to the non-unique behavior is presented therein.

3. p. 1057, lines 20-23. Rather than 3 wavelengths, it would be more accurately saying that, Kikuchi et al. (2006) used two dual-wavelengths: 1.02 and 1.6 um and 1.02 and 2.2 um. The authors of this paper didn't find much difference between them for water clouds. (With respect to Kikuchi et al., see also my comments to pages 1068, 1070 and 1071 below.)

That is correct. The Kikuchi et al. paper is a two, dual-channel approach so we changed the wording to reflect this.

4. p. 1063, lines 24-25. Needs a reference. I would suggest: Turner D. D., A. M. Vogelmann, R. T. Austin, J. C. Barnard, K. Cady-Pereira, J. C. Chiu, S. A. Clough, C. Flynn, M. M. Khaiyer, J. Liljegren, K. Johnson, B. Lin, C. Long, A. Marshak, S. Y. Matrosov, S. A. McFarlane, M. Miler, Q. Min, P. Minnis, W. O'Hirok, Z. Wang, and W. Wiscombe, 2007. Thin liquid water clouds: Their importance and our challenge. Bulletin Amer. Meteor. Soc. (BAMS), 88, 177-190.

True. This was neglected. We added this reference as suggested.

5. p. 1065, lines 17-18 and Fig. 1. USGS grass albedo is unrealistically large. I doubt that grass can reflect 70% in NIR. Please check!

The albedo in this range does appear too high. However, we do not use any of the data in the spectral region that reaches 70%. We've updated this plot (Figure R2) and excluded the parts of the albedo that I'm not using. We also added a short section about how the surface albedo might affect the retrieval as suggested by Reviewer #2 (page 1065, line 23).

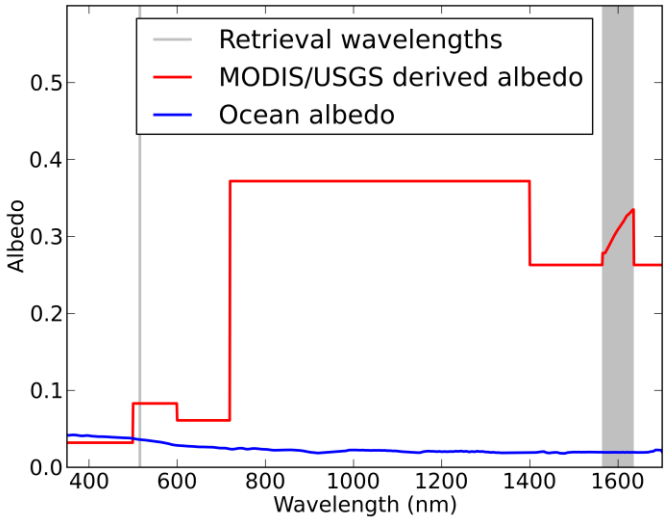

Figure R2: The surface albedo values used in the radiative transfer model. The 'vegetated' albedo derived from MODIS and the USGS grass albedo is in red and the ocean albedo from Coddington et al. (2010).

6. p. 1066, lines 10-11. Since a viewing angle was never mentioned, I assumed that it was nadir and zenith measurements. If appropriate and to avoid confusions, I'd call them zenith and nadir radiance; otherwise, indicate viewing angles.

This is true. We added nadir and zenith to the description.

7. p. 1067, lines 3-9. Plots in Fig. 2 depend on SZA and surface albedo. They were never mentioned.

All of the figures presented in section 3 were made with the same surface albedo and SZA; we added that statement to the first paragraph of section 3.

8. p. 1068, lines 4-7. Need a reference here. I can suggest Rawlins and Foot, 1990 and/or Platnick, 2000.

We added the Rawlins and Foot reference as suggested.

-9.p. 1068, lines 25-27. The manuscript says that there is "no sensitivity to effective radius for optical thickness less than 10, with some sensitivity to effective radius for optical thickness between 20 and 40." This is the key statement describing the retrieval method. If this statement is true (and I believe, it is), how can one retrieve droplet effective radius using this method and report about the uncertainty of the retrievals? Also, I guess, this statement is not consistent with the results of Kikuchi et al. (2006). Instead of 515 nm they used 1020 nm. Does it help? I recommend discussing this issue more thoroughly. As a minor comment, please indicate SZA and surface albedo used for Fig. 3. To make Fig. 3b more informative, you might want to change the aspect ratio of the plot.

10. p. 1070, lines 11-12. Again the manuscript says that "there is virtually no effective radius information under a cloud with optical thickness less than 10." See the previous comment.

11. p. 1071, lines 25-28. The same. See the previous comment. Also, please indicate SZA and surface albedo used for Fig. 5.

We address comments 9, 10, and 11 here. The statement "*no sensitivity to effective radius for optical thickness less than 10, with some sensitivity to effective radius for optical thickness between 20 and 40*" was made as a way to characterize the retrieval as highly uncertain for values of optical thickness less than 10 with decreasing uncertainty in the optical thickness range between 20 and 40. To quantify and discuss this issue further we applied a 2 μm threshold uncertainty in the retrievals. That is, retrievals with an effective radius uncertainty higher than 2 μm were considered invalid (that is, the retrieval failed) for the purposes of this work. We updated the text to refer to this definition in place of the phrasing highlighted above. With regards to Kikuchi et al., there is, unfortunately, no uncertainty analysis provided with their results which makes a comparison difficult because this is one of the central messages of this work.

12. p. 1072, lines 17-28. To really understand and appreciate your proposed slope transmittance retrieval algorithm, at least, a 2D plot of T(tau, reff) for the spectral region between 1565 and 1634 nm is desperately needed. This is the most innovative part of the paper but it is not sufficiently well explained and illustrated.

We added a plot (Figure R3) that shows the change in spectral shape for the range 1565 nm to 1634 nm for a range of effective radii in addition to the text that goes along with explaining it. This will hopefully explain in more detail the reasoning behind the use of the slope and its effective radius dependence. In addition, the dependence of the slope on optical thickness and effective radius is included in the lookup table plot in figure 7 of the Discussion paper. Though we do not feel the 2D plot of the slope adds more information to the paper, we have included a contour plot here of the slope versus optical thickness and effective radius below in figure R4. Unless the referee recommends publication in the final version, we will not add this figure.

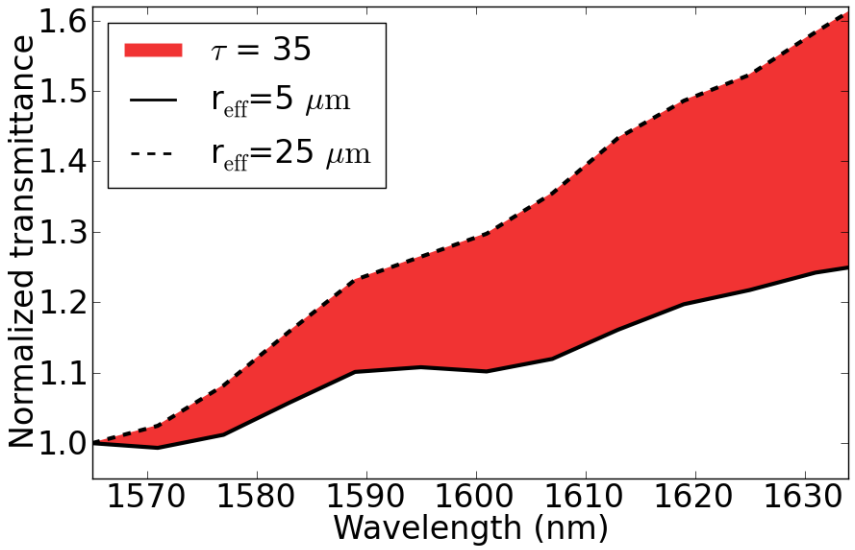

Figure R3: The spectral shape of the normalized transmittance for two effective radii and one optical thickness over the range where the spectral slope is fit.

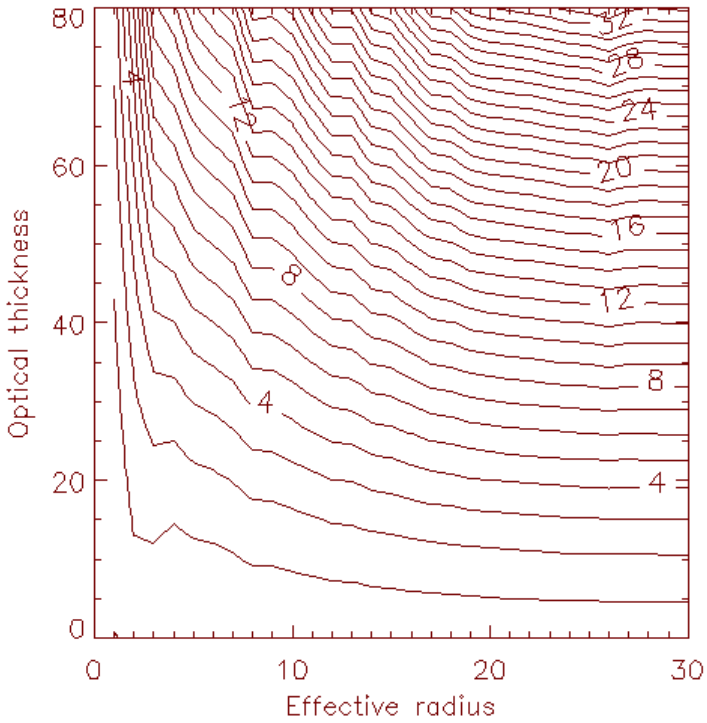

Figure R4: Contour plot of the slope in the spectral region between 1565 nm and 1634 nm versus optical thickness and effective radius. The slope is shown in units of μm^{-1}

13. p. 1073. Equation (9) is confusing. Explain it better or delete.

We added the reference of the text book that it came from and added a further explanation.

14. p. 1075, lines 2-5. A reference is needed here. I can suggest Dutton et al. (2004JGR).

We added this reference as suggested.

15. p. 1076 and Fig. 10a. I didn't get why the retrievals for 5≤tau≤10 are so good. How does it consistent with your previous statement that there is "no sensitivity to effective radius for optical thickness less than 10".

Another reviewer (#2) pointed out that we neglected to include a discussion of sensitivity to optical thickness. This discussion was added to the end of section 3.2. In that discussion we point out that the dual-wavelength method is, in fact, sensitive to the optical thickness as demonstrated by the separation of the optical thickness lines in figure 3b and by the relatively low optical thickness uncertainties in Table 1. Thus, it should be expected that the optical thickness retrievals of the two methods should match.

16. p. 1077, lines 22-24. It's unclear to me. What is special in reff=16 um?

We replaced the plots in this section with time series plots of the uncertainty retrievals (see Figure R5). We feel that they get the point across in a more straightforward manner. That said, the point about r_{eff}=16 µm was simply caused by a statistical artifact. The small amount of data that we are using did not make all bins in the histogram statistically significant. This was another reason for removing these plots and replacing them with time series plots.

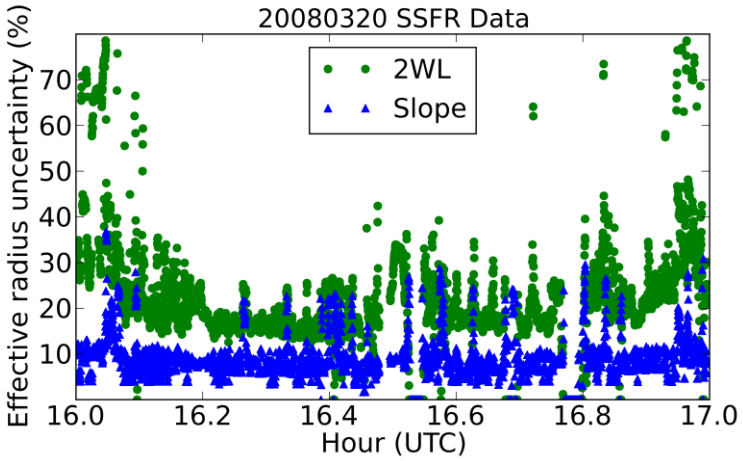

Figure R5: Time series plot of the relative effective radius uncertainties for the ICEALOT case.

17. p. 1078, lines 24-26. I don't think that the large uncertainties of the retrieved effective radius allow us to compare the validity of the assumptions behind Egs. (2) and (3).

We agree. The goal was not to draw any conclusions about the vertical structure of the observed cloud, but to compare the LWP with these different assumptions.
We replaced this sentence:

"These results also show that the LWP calculations assuming a WH06 cloud compared better with the MWR retrievals than the constant effective radius LWP."

with this:

"The average LWP assuming the WH06 cloud was closer to the average of the MWR retrieved value, but the study of vertical (and horizontal) cloud inhomogeneities requires more analysis"

18. p. 1079, lines 6-7. What was the cloud base height for this case? It's a crucial information for comparing the effects of different FOVs.

Thanks for pointing this out. We added the cloud base height (~1.2 km) to the FOV information.

19. p. 1081, lines 16-17. 12.8% and 8.9% are obtained from standard deviations of non-Gaussian distributions as the ones in Fig. 10b. If this is true, they do not accurately characterize the uncertainty of the retrievals and I wouldn't emphasize that one of them is higher than another.

As pointed out in addressing previous comments we added a threshold of the effective radius uncertainty. The average uncertainties of the valid retrievals were 11.7% and 8.6%.

20 p. 1081, lines 18- 19. The statement is too mild. At least, replace 'higher' with 'much higher'.

We added this as suggested.

21. p. 1081, lines 24-27. This statement is not convincing for me.

We agree that this passage added little to the paper and was confusing so we deleted it and added:

"and the effects of cloud inhomogeneity"

to the sentence following the deleted passage (page 1081, line 27 of the Discussion paper).

22. p. 1087. I would replace table 2 with just two values tau=46 for case (a) and 31-38 for case (b).

We agree that this simplifies the discussion so we removed the table and added the values to the text per the reviewer's suggestion.

23. p. 1093. Two green colors for tau=50 and 10. It is hard to distinguish them.

We removed this plot from the discussion as suggested by Reviewer #2.

24. p. 1098. What are those points that give reff>25 um for the 2wvl method in Fig 11c?

I do not have an explanation for these points. One possible explanation is the presence of mixed phase/ice clouds that are not being caught by the somewhat simple phase flag. The absorption properties of ice in the spectral region of the slope are stronger than liquid water and can look like large water drops. They don't fail the phase check as described in Section 3.5 of the Discussion paper, but the expectation was not that this would catch all cases.

25. p. 1101 and 1102. Figures 14 and 15 are too busy. It is hard to get any information from them.

This is true. There is a lot of information on these plots and much of it is redundant. We changed these plots to show shorter, representative time periods that show more detail but will still illustrate the same points in the text. See Figure R6 below which will replace Figure 14 from the Discussion paper.

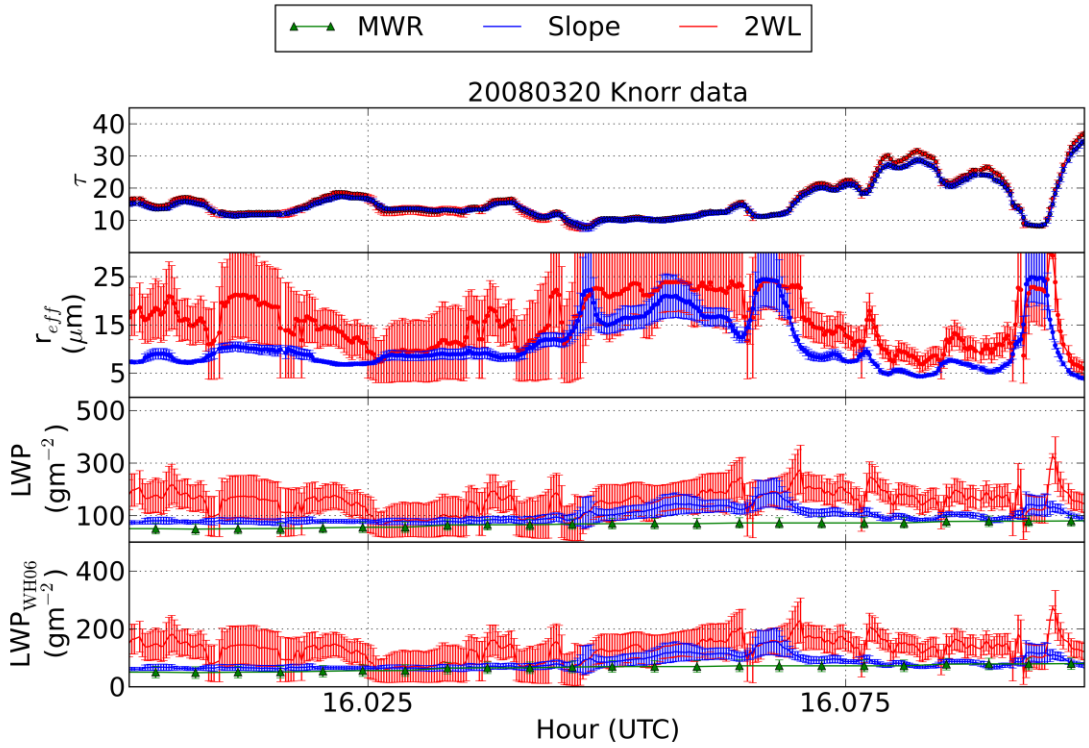

Figure R6: Time series plots of SSFR retrievals of optical thickness and elective radius. LWP retrievals from the NOAA MWR along with calculated LWP from SSFR retrievals. Each point is shown with its estimated uncertainty.

26. p. 1103 and 1104. To better see the variability, I would recommend to change the aspect ratios of these plots.

We took the suggestion of Reviewer #2 and replaced these plots with a table.