

## REPLIES TO THE REVIEW COMMENTS

Firstly, we would like to thank the two reviewers for taking time out of their busy schedules to complete these two reviews and for the constructive comments they have supplied. We feel their comments have improved the paper.

### Anonymous Referee #1

Received and published: 15 February 2012

Review of "Satellite retrieval of the liquid water fraction in tropical clouds between -20 and -38\_C" by D.L. Mitchell and R.P. d'Entremont Paper # AMTD 4-7657-2011

Specific comments:

1. The method for deriving cloud liquid fraction is based on the fact that  $\beta_{eff}$  is constant for  $T < -40_C$  (i.e. for all-ice clouds) while  $\beta_{eff}$  increases for  $T > -40_C$ . The author state that the increasing of  $\beta_{eff}$  is due to the presence of liquid particles. It seems to the reviewer that this statement is mainly based on the Giraud et al. (2001) study in which only one day of coincident POLDER-ATSR2 data is considered. The Giraud et al. results should be used with caution because (i) the POLDER instrument is not well-adapted for very thin cirrus clouds and (ii) the warm clouds analyzed in their study are probably altostratus clouds.

*Reply: In Section 3 we note that one day of coincident POLDER data was used in Giraud et al. (2001) but also note that the dataset of Giraud et al. (1997) was based on 21 AVHRR images of high clouds over the northern Atlantic and Europe.*

2. The authors dedicate subsection 5.1 to the question "liquid water or ice?" They note that it is conceivable that the increase in  $\beta_{eff}$  is due to small ice crystals but they show that it is very unlikely for the 5 August 2007 case study. What gives the "small ice crystals" hypothesis for the other case study ? Is it unconceivable that a small fraction of IWC is associated to very small near-spherical ice particles ? I think the authors should moderate their conclusions.

*Reply: We added text in Section 5.1 stating that "Very similar results were obtained for the 22 July case study." The 22 July case was not included in this particular analysis for brevity. Regarding quasi-spherical ice particles, ice cloud PSD characteristic of the clouds we sampled are shown in Fig. 11 and when we assume that all ice particles are quasi-spherical (i.e. droxtals) as shown in Fig. 12, the  $\beta_{eff}$  calculated from these observed PSD are still much lower than the retrieved  $\beta_{eff}$  for  $T > -40_C$ . Some portion of the IWC is probably associated with small quasi-spherical ice particles and this is accounted for in the retrieval algorithm by the threshold value for  $\beta_{eff}$ .*

3. The retrieval algorithm presented in this study assumes that MODIS pixels are clear or overcast. What's happened if pixels are partially cloudy? The authors should briefly discuss that in section 5.

*Reply: The last paragraph in Section 2.3 now fully explains this.*

4. Section 4 relative to the algorithm description is not very clear, in particular bottom of p. 7669 and top of p. 7670. At first reading it is difficult to understand if there is one or two  $\beta_t$  values. Maybe an additional figure showing the different threshold values should be informative.

*Reply: We fully agree with this observation. This paragraph has been extensively rewritten and broken into two paragraphs in Section 4.*

Minor comments and technical corrections :

p.7661, l.14 : "The second term is the upwelling surface and atmospheric energy that: : :?"

*Reply: Now corrected as reviewer suggested: "The second term is the upwelling surface and atmospheric energy that is absorbed by the atmosphere and re-emitted to space at its own temperature."*

p.7664, l.19-22: Please describe the dataset more deeply (year, where) here and not on page 7666.

*Reply: Data locations placed in the Figure 3 caption.*

p.7671, l.10-15 : Why the authors do not evaluate their retrieval using mean diameter less than 9 microns ?

*Reply: This was an oversight; thank you for mentioning this. We have added to Sec. 5, line 681-682, the sentence "Moreover, in situ observations of mixed phase Arctic clouds (Lawson et al. 1998) during FIRE ACE did not find  $\bar{d} < 9 \mu\text{m}$ ."*

p. 7674, l.19-21 : it seems to the reviewer that the sentence beginning "Yang et al. optical: : :." is redundant with the previous one.

*Reply: The previous sentence indicates that optical properties can be calculated using either MADA or via the Yang et al. database, so here in this sentence we need to specify which one was used.*

p. 7677, l.7-8: Not clear. Is it a result from the present study ? If not a reference would be useful.

*Reply: Good point; this was not clear. Text has been added to lines 853-856: "Given that 2/3 of the retrievals are contained in the  $\text{mean} \pm \sigma$  range, the pink curve in Fig. 14 (corresponding to  $\text{mean } \beta_{\text{eff}} + \sigma$ ) suggests that 16% of the clouds experience a change in  $D_e$  from  $\sim 86 \mu\text{m}$  to  $\sim 20 \mu\text{m}$ , which is over a four-fold increase in  $\beta_{\text{ext}}$ ."*

p.7679, l.24 :Add Hu et al (2010) paper is the reference list.

*Reply: Thank you for catching this oversight. Both Hu references have now been added.*

## Anonymous Referee #2

Received and published: 23 February 2012

The article describes a technique for retrieving the liquid water fraction of clouds with a low liquid water fraction. The technique focuses on 4 MODIS channels: 11  $\mu\text{m}$  and 12  $\mu\text{m}$  for retrieving phase fraction and particle size, and 2  $\text{CO}_2$  channels for retrieving cloud top temperature and 13.3  $\mu\text{m}$  absorption optical thickness.

**The article suffers several major flaws which are outlined below.**

### 1. Incomplete description of the cloud retrieval algorithm

I feel that one mark of an acceptable scientific paper is that the results should be reproducible. Too many details are missing in this paper for that to be the case. For example, the authors state that they are using observations at 11

and 12  $\mu\text{m}$  to retrieve cloud emissivities and 13.3 and 14.2  $\mu\text{m}$  to retrieve cloud temperature. They never explain exactly how they retrieve cloud temperature.

My guess is that a  $\text{CO}_2$ -slicing approach was used, but no discussion of quality control or algorithm assumptions is given. There are numerous papers on the topic that make clear that while the retrieval is simple theoretically, it requires care in practice. In addition, more than one  $\text{CO}_2$  channel pair is typically used. In addition, while it's implied that the errors in cloud temperature retrieval are less than 0.14 K (page 7663, lines 26-30), that error is in fact due only to potentially errors in spectral cloud emissivity. In fact, there is no real error analysis of the cloud top temperature retrieval, which could have large impacts on retrieved IR cloud properties.

In general, the retrieval methodology lacks sufficient details for someone to reproduce the results. Relationships between mid-layer temperature and cloud (top?) temperature are not explained, important details about microphysical property assumptions are not summarized.

Even more importantly, the authors show that the retrieved fraction of liquid water is incredibly sensitive to small changes in assumed liquid effective particle size, but the impacts of this and other retrieval errors is not explored. Nor is the possibility of a small ice crystal mode is not explored.

*Reply: The emissivity and cloud temperature retrieval technique that we use is not  $\text{CO}_2$  slicing, as the reviewer suggests. We have extended our description of the retrieval model in sections 2.1 and 2.2 and parts of Section 4 have been rewritten.*

*Regarding retrieval errors, we would like to emphasize that we are not retrieving at the individual pixel level; our retrievals are in an average sense. As shown in Figure 5 by the pink lines, we are reporting on the average trend in the  $\beta_{\text{eff}}$  ratio with temperature between 205 and 253 K. Each mean value in the plot was computed using thousands of pixels.*

*More details on microphysical treatment have been added; such as ice particle shape calculations are based on area- and mass-dimension power laws and size distribution assumptions (e.g. lines 221-222, Sec. 4, 721-722, 764-765). An appendix describing the cloud mask has been added. The impact of droplet size on the retrieval is described in Fig. 7 and 9, and the possibility of a small ice crystal mode is explored in Sec. 5 and Figs. 10-12.*

### 2. Incomplete description of the data set.

The authors state that they are working with MODIS observations of cloud fields off of Costa Rica on 22 July and 5 August 2007. They state that a cirrus mask

is applied so that cirrus clouds are isolated and evaluated and that all retrievals are over ocean and correspond to single-layer clouds (page 7667, lines 1-6). In addition, only cases with  $\varepsilon < 0.7$  are included. This description of the data sampling is not sufficient. How does the cirrus cloud detection work? How is it ensured that the non-unity retrieved cloud emissivity is due to cloud transparency rather than cloud coverage within the pixel? How is it determined that the clouds are single-layered? The answers to these questions are crucial to interpreting the results of the study. In addition, comparisons with existing MODIS MYD06 retrieval products including cloud top temperature would have been very useful in assessing the quality of the data sampling and cloud top temperature assumptions. The comparisons with CALIPSO outlined on page 7665 lines 16-22 are not very detailed and the impacts of the differences between retrieved heights is not discussed. In addition, it would have been very easy to get estimated cloud top and midlayer temperatures from the CALIPSO products for comparison with these retrievals.

*Reply: An appendix has been added to describe how the cloud mask works and how single layer clouds are selected. A discussion of partial cloud coverage of pixels is now included in the last paragraph of Sec. 2.3. Note that this retrieval is only intended for clouds whose radiances contain microphysical information; hence  $\varepsilon < 0.7$ . In the last paragraph in Sec. 2.2, cloud vertical extent observations from CALIPSO are compared with those from our MODIS retrieval.*

3. Uncommon and/or inconsistent language that may introduce confusion. A major issue is the use of the phrase cirrus cloud. While the abstract clearly states that the paper is about retrieving the liquid fraction in mixed-phase clouds, the paper discusses retrieving the liquid fraction of cirrus clouds. Since cirrus cloud has the connotation of a cloud completely composed of ice crystals, the phrasing is extremely problematic and raises the obvious question "Why are water retrievals being done on ice clouds" The easy solution would be to change cirrus cloud to mixed-phase cloud in the text. But, I do not feel that would be either sufficient or appropriate due to the assumptions the authors appear to have made in selecting their data set and in how the retrieval technique is based on the authors' previous work on cirrus cloud retrievals using data from the same field campaign. For example, the authors are willing to call scenes indicating cloud conditions are almost all liquid as cirrus (Page 7671, lines 5-6). If the authors are implying that traditional cirrus clouds typically contain a detectable amount of liquid water, they need to make that clear from the start. Another example is the use of the phrase single-layer. Page 7676 lines 6-8 reads "the lidar analysis reveals the presence of liquid-dominated cloud under the CALIOP flight path corresponding to the two MODIS scenes, mostly between 7 and 9.5 km." The presence of the lower-level cloud conflicts with the statement that the study was screened to include only single-layer cirrus clouds (page 7667, line 6). Another more minor point is the fact that the author does not make clear that photon tunneling contributions are an artifact of using the ADT approximation to calculate absorption efficiencies.

*Reply: We agree that the cirrus cloud terminology used throughout the paper is a potential source of confusion since people often understand cirrus to be pure ice clouds, not mixed*

phase. Therefore we have replaced “cirrus” or “cirrus cloud” with merely “cloud” or “high cloud” in most cases. Most of these changes occurred in Sec. 2.

Note that a portion of the CALIOP lidar results in Fig. 13 represents a narrow swath through the TC4 region we sampled. The fact that the lidar detected some multilayer clouds does not mean all clouds in our sampled region were multilayered or that single-layer clouds cannot be selected by the cloud mask.

The fact that the cirrus retrieval described in Mitchell et al. (2010) and this mixed phase cloud retrieval are applied to the same TC4 cloud field should not create confusion since the cirrus retrieval was developed for ice-only conditions ( $T < -40$  C). The cirrus retrieval and this mixed phase retrieval share some common ground but in many respects are very different using different physics (see last paragraph of Sec. 4).

Lastly, photon tunneling (i.e. wave resonance) contributions to absorption and extinction were parameterized based on Mie theory to produce the modified anomalous diffraction approximation (MADA). The close agreement between MADA and other more theoretically rigorous scattering/absorption calculations indicates the parameterization carries certain physical relationships key to understanding and predicting wave resonances. In fact, these relationships led to the physical insights exploited in the Mitchell et al. (2010) cirrus PSD retrieval as well as this mixed phase retrieval.

#### 4. Insufficient motivation and mediocre organization.

I feel that a major issue with the paper’s organization is that the retrieval of the water fraction of cirrus clouds should have been presented instead as a hypothesis that undetected liquid water was necessary to explain the observed radiances. If this were the case, the authors would probably have done a better job of tying this work to previous work (including the Mitchell et al paper referenced frequently in this work). As presented, it is difficult to see the motivation of the work since a full range of the liquid fractions cannot be retrieved using the technique. As more minor notes, the paper would have been easier to read if it had followed a more traditional style of motivation, data, model, algorithm, results, and conclusion. Also the figure captions did not always define all of the data on the plots.

*Reply: We agree this would be an interesting angle to take, but all said and done, it may also open the Pandora’s Box of debate on defining cirrus clouds. In addition, our cirrus clouds are defined by a cloud mask, which is no guarantee of pure ice conditions. Figure captions have been improved.*

**In addition, I’ve listed a number of technical comments below.**

1. Page 7663, line 1 “For ice clouds, the real refractive index” should read “For bulk ice, the real refractive index”

*Reply: Done*

2. Page 7663, line 13: The microphysical assumptions reported in Mitchell et al. (2010) should be summarized given their importance in the retrieval algorithm and results.

*Reply: Done*

3. Page 7666, lines 8-10: regarding the g-based scattering correction to Q and  $\beta$ . I believe that the g-parameterization in Yang et al 2005 was based on an assumed set of particle size and ice crystal shape distributions. Since I'm assuming this study does not use the same crystal shape and size distributions, is the g-parameterization still applicable?

*Reply: Yes. Text has been added to Sec. 2.4: "In the retrieval algorithm,  $\beta_{\text{eff}}$  is calculated from (6) using the g parameterization given in Yang et al. (2005), which is parameterized in terms of  $D_e$  and assumes a certain combination of ice particle shapes. While this shape assumption affects the wave resonance contribution to absorption, ice particle shape for a given  $D_e$  has a very weak impact on g at thermal wavelengths where g depends primarily on forward scattering as determined by the particle's area cross-section (van de Hulst 1981)."*

4. Page 7667, Line 1-3 and Page 7687 (Figure 3): Please mention which MODIS channels are mapped to R, G, and B to create the false color image.

*Reply: This is now explained in the Appendix.*

5. Page 7667, Line 12. Please list the temperature intervals used in Fig. 5.

*Reply: These intervals are evident in Fig. 5 and are not so scientifically important.*

6. Page 7668 line 1: "This is only possible due to this curious relationship that Mother Nature has provided" is not the best sentence for a scientific paper.

*Reply: Sentence has been reworded.*

7. Page 7668, Line 22: "Otherwise the cloud is assumed glaciated." Given the use of the term cirrus in this paper, I feel it is necessary to define what is meant by glaciated here.

*Reply: Glaciated is now defined (line 546).*

8. Page 7670, Lines 13-14: It would be good to make clear that the retrieval is also sensitive to the shape of the water droplet size distribution by rewriting "Changing the droplet dispersion parameter from 5 to 15 changed the liquid fraction by \_26%" as "The retrieval is also sensitive to the water droplet size distribution; changing the droplet dispersion parameter from 5 to 15 changed the liquid fraction by \_26%."

*Reply: Done*

9. Page 7670, Lines 16-18: "For ice crystals this sensitivity is due to the photon tunneling phenomena and for water droplets this is due to Beer's Law absorption effects between wavelengths (Mitchell et al., 2010)." is out of place here.

*Reply: Sentence removed and replaced with new paragraph at end of Sec. 4.*

10. Page 7673, Lines 11-12: Note that as plotted, these size distributions do not show particles with maximum dimension less than 10 microns.

*Reply: The first (smallest size) bin of the 2D-S probe is centered on 10  $\mu\text{m}$ . A reference is given for details.*

11. Page 7673, Line 24: "However, this may be a consequence of low levels of liquid water." I feel that this statement should be qualified to make clear that this is not the conclusion of Cooper and Garrett (2010). Something like "Based on the results of this study, we feel that the results of Cooper and Garrett (2010) may be a consequence of low levels of liquid water". Otherwise, the Cooper and Garrett results do not support the hypothesis of this paper.

*Reply: Done*

12. Page 7674, Line 19-20: "For the Yang et al. optical properties, tunneling is greatest for quasi-spherical ice particles (droxtals) and less for bullet rosettes" It would be more appropriate to say here that "for the Yang et al., optical properties, the differences between calculated  $Q_a$  and ADT  $Q_a$  is greatest for quasi-spherical ice crystals and less for bullet rosettes" since tunneling is not an assumption made in the Yang calculations.

*Reply: A reference is now given that backs up this statement, showing estimates of the tunneling efficiency for droxtals and bullet rosettes.*

13. Page 7675, Line 13-14, "Thus all cloud levels contribute significantly to the radiance observed by the satellite sensor". I disagree with the authors. Not all cloud layers contribute equally, therefore I am not certain that the retrieval will not be affected by the vertical partitioning. Retrieval techniques that make use of absorption (such as this one, and size retrievals at 2.1 or 1.6 microns) are much more sensitive to vertical partitioning than scattering-based techniques. The possibility that the sensitivity to liquid fraction reaches a saturation point at some optical depth into a cloud bears study.

*Reply: We agree that some vertical partitioning is likely. A sentence has been added here: "The semi-transparent nature of such clouds ensures that at least some of the upwelling energy flux originated from the lowest cloud levels."*