

Authors' answers to referee comments on the manuscript:

## Comparison of MODIS and VIIRS cloud properties with ARM ground-based observations over Finland

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We would like to thank the reviewers for their helpful comments which we believe will greatly improve this manuscript. We present our replies to the reviewers' comments below. The reviewers' comments are presented in normal font and our responses are then presented in italic.

During the processing of the data due to the reviewers' comments we came across a minor error in the programming code which increased the number of cases in the VIIRS CTH analysis. The number of cases for the nighttime data changed from 38 (17) to 56 (24) for all (single layer) cases. The medians difference between the satellite and ground-based measurements for the same datasets changed from -880 (360) to -150 (309). The changes to the daytime data were smaller than this. We have changed the numbers on two of the rows in Tables 1 and 2 as well as a few instances in the text. The overall changes to the results were minor and no text in the manuscript needed to be rewritten because of programming error.

### **Anonymous Referee #1 comments**

Line 50: Please mention that there are also fewer bands available for CTH generation on the VIIRS, which has no 15  $\mu\text{m}$  CO<sub>2</sub> absorption bands used in the MODIS CO<sub>2</sub> slicing method.

*We have added the following sentence: "However, VIIRS have fewer bands available for CTH retrievals."*

Line 80: Awkward sentence – I would say "Therefore, care must be taken: : :".

*We have changed the sentence according to the reviewers' suggestion.*

Line 89: Aqua's equator crossing time is 13:30, not 10:30. Line 97: I would add that the 5-km CTP data is still available for C6, but the 1-km has been added. I would also state that the 1-km data has been used for the study as I don't recall that it is stated explicitly.

*We have changed the crossing time for Aqua and added the following text: "but are also still available at 5 km resolution. The 1 km resolution CTH will be used in this study."*

Line 101: "altitude" should be "altitudes". I would change the next sentence to read something like this: "Clear sky radiances are subtracted from observed radiances and ratios of these differences are used to retrieve CTP." And the next sentence: ": : : where the ratio of the bands sensitive to clouds at the highest altitudes are tested first."

*We have changed the text according to the reviewer's suggestions.*

Line 105: NCEP is "National Centers for Environmental Prediction".

*We have corrected this.*

Line 114: Mention that the apparent lapse rates are only used over ocean scenes. Land scene processing has not changed in C6.

*We now mention in the text that the apparent lapse rates are used over ocean areas.*

Lines 121-122: “uncertainty” should be “uncertainties” and “is:” should be “are:”.

*We have corrected this.*

Line 130: Please define NWC and SAF.

*We have defined NWC SAF.*

Line 133: Please use and define BTM (brightness temperature difference).

*We have changed this according to the reviewer’s suggestion.*

Line 142: Use “only” instead of “merely”.

*We have changed the text according to the reviewer’s suggestion.*

Line 191: I would write, “overpasses, of which 127 passed the : : :”.

*We have changed the text according to the reviewer’s suggestion.*

Line 201: Reference needed for 72° SZA value.

*We have added a reference.*

Line 204: Please state what “dominant” means (> 50% ?).

*We have added “(> 50% of the pixels)” after dominant.*

Lines 214-217: These sentences are confusing and possibly incorrect. Could you clarify these?

*We have changed the sentences.*

Lines 247-249: As both MODIS CTH methods involve IR data only, I suspect this artifact is really a function of viewing zenith angle (VZA), not SZA. SZA and VZA can be correlated at high latitudes. Perhaps these data come from higher VZAs where the cloud amounts and CTHs can be inflated with respect to near-nadir values.

*This is a very good point by the reviewer. We re-created Figure 2 a and b from the manuscript but colored the markers according to VZA (see below). However, the VZA results (c and d) did not look similar to those of the SZA (a and b). Instead, the high values of VZA are found all over the figure and some of the cases with high SZA placed above the 1:1 line actually have low VZA angles. A correlation between VZA angle and SZA does hence not seem to be responsible for this artifact. We have added a sentence regarding this in the manuscript.*

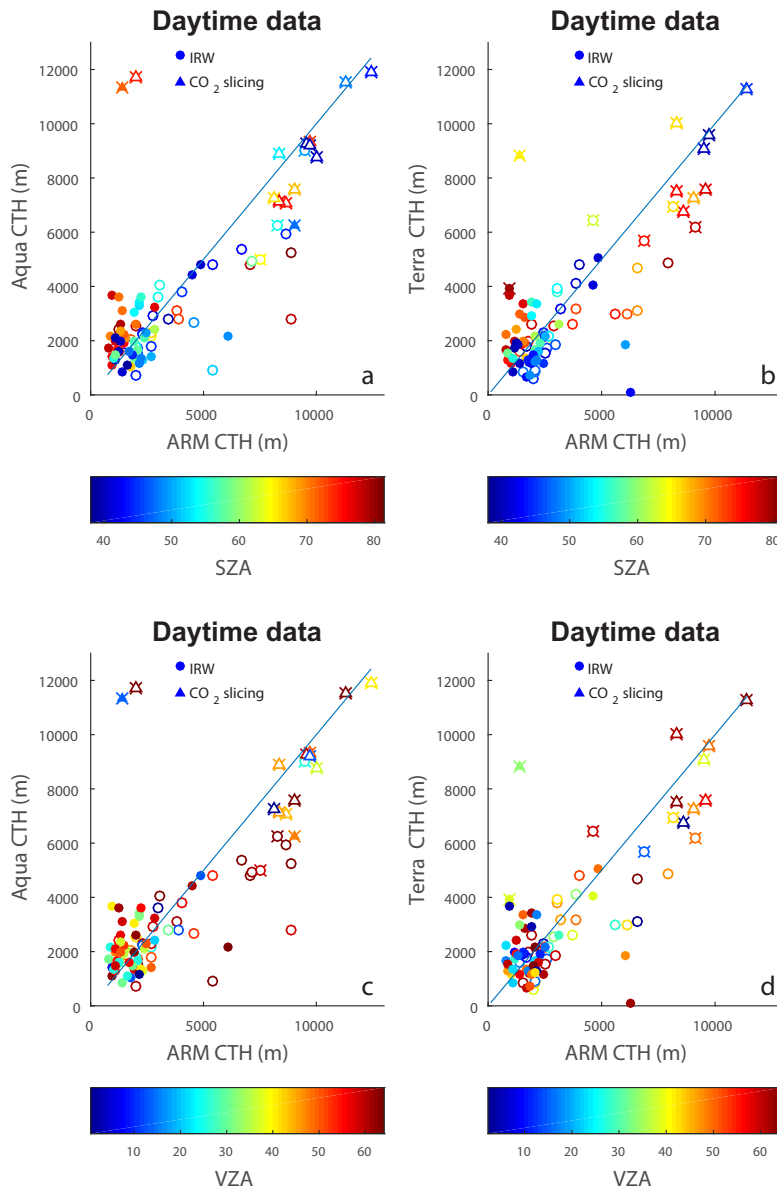


Figure 1. CTH results from figure 2 of the manuscript (a and b). c and d, the same figure but colored according to VSA instead of SZA.

Line 315: I think you mean overestimated CTHs for low-level clouds and not high-level clouds. Or is it underestimating high-level clouds? Please correct.

We have changed “overestimates” to “underestimates”.

References There are three missing references: IPCC 2013, Petäjä 2013, and Liljegren 1999

We have added the missing references.

## P. Minnis comments

1. There is no discussion of the uncertainties or biases in the surface data set. Because the surface data set the standards, we need to know how good they are.

a. radar estimates of cloud top height might be pretty good for low water clouds, but how about cirrus or thick ice clouds with small crystals at cloud top. For thin cirrus, this could be answered if the surface lidar data are used to estimate cloud top. For thicker ice clouds, previous comparisons would be informative.

*Powerful lidars are in principle more sensitive to thin cirrus than cloud radar. However, due to attenuation, it is not always clear that cloud top measurements would be any more accurate. With a tropopause height of 10 km or lower, the sensitivity of the cloud radar instruments utilised in this location is such that only very optically thin clouds are not detected. After processing through the Cloudnet scheme the typical sensitivity is better than -27 dBZ at 10 km for either instrument. This corresponds to an optical depth of about  $0.01 \text{ m}^{-1}$  for a single 30 m range gate measurement, with uncertainties of about a factor of 2-3 when considering both potential biases in reflectivity and uncertainties in the IWC retrieval. We consider the Hogan et al 2006 retrieval suitable for mid-latitude ice cloud (it is unbiased). The manuscript has been updated as follows:*

*“The minimum reflectivity at 10 km is about -27 dBZ, which leads to a derived ice water content of about  $5 \times 10^{-6} \text{ kg m}^{-3}$  when using a reflectivity-temperature based relationship (e.g. Hogan et al., 2006). This corresponds to an optical depth of about  $0.01 \text{ m}^{-1}$  (e.g. Heymsfield et al., 2003) with an uncertainty of about a factor of 3 when considering both potential biases in reflectivity and uncertainties in the IWC retrieval.”*

b. are there limitations to MWR retrievals such as effects of precipitation or thick clouds? any differences for supercooled clouds?

*The manuscript has been modified as follows:*

*“When properly calibrated, BTs are obtained with an absolute accuracy better than 0.5 K (Maschwitz et al., 2013), which corresponds to an LWP uncertainty of about  $20 \text{ gm}^{-2}$ . MWR retrievals are limited to non-precipitating profiles (Crewell and Löhnert, 2003) but are not affected by ice cloud optical depth. LWP in supercooled clouds can be retrieved reliably, although LWP values may be small and close to the instrument uncertainty.”*

2. What is actually retrieved by the satellite? cloud top height or cloud radiating height? Might this influence the relationship between the radar and satellite data? Example reference: (Minnis et al. GRL, 2008)

*The satellite will retrieve a cloud top from the cloud radiating height. We have added the following 2 sentences regarding this to section 3.1.*

*“The satellite retrievals obtain CTH from the cloud radiating height which corresponds to a height below the CTH, at least for optically thin clouds. This is known and corrected for, but this procedure become more problematic when several cloud layers are present, which may have different optical thicknesses.”*

A 2000-m bias might be a big deal for a low cloud at 1500 m, and not such a big deal for a cloud at 10 km. The heights should be analyzed separately for water and ice clouds.

*We agree that is a large bias is more problematic for low clouds compared to high clouds. However, since some of the high clouds are classified as water clouds by the MODIS algorithms we have instead divided the clouds according to the cloud top height measured from the ground-based sensor. We have added 2 new tables (Tables 3 and 4) and a new paragraph in Section 3.1 that describe these results.*

4. The retrievals are highly sensitive to semi transparency of the clouds, especially cirrus. The results should be separated for optically thick and thin clouds ( $COD < 3$ ) to provide more insight into the analysis.

*There are none or very few cases (maximum 3) in the datasets we have investigated where the mean COD is lower than 3. This type of analysis is hence not possible.*

Specific comments

pg. 3, line 19: "data is" should be "data are"

*We have corrected this.*

pg. 4, line 29: "monthly averaged lapse rates" should be "zonal monthly mean lapse rates over ocean"

*We have changed the text according to the reviewer's suggestion.*

pg. 6, line 4: "less" should be "fewer"

*We have changed the text according to the reviewer's suggestion.*

pg. 6, line 17: "is" should be "are"

*We have corrected this.*

pg. 6, line 27: "become" should be "becomes"

*We have corrected this*

pg. 8, line 8: The VIIRS data still have some large underestimates. Need some qualification of what is meant.

*We have changed this sentence since, as P. Minnis points out, the VIIRS data also have some large uncertainties. It now reads "VIIRS data also displays significant underestimates of CTH for high clouds."*

pg. 8, line 17: First clause of the sentence is awkward, please rewrite.

*We have rewritten the sentence and it now reads "However, no general conclusions regarding the performance of the algorithm should be drawn from this comparison since the number of daytime cases for VIIRS is low (Sect. 3)."*

pg. 9, line 17: C6 accounted for the degradation of the Terra calibration, but not the Aqua degradation that occurred after 2008 (Doelling et al. IEEE TGRS 2015). The C6 calibrations did not account for a fundamental difference of  $\sim 1\%$  between Aqua and Terra that was present in C5 (Minnis et al. JAOT 2008; Dong et al. 2008). That difference will cause difference in optical depth between Terra and Aqua.

*We do not fully understand P. Minnis comment here. We have read the provided references and found that there is a difference of ~1% percent between Terra and Aqua due to a degradation in Aqua. We have however not compared optical depth and therefore do not see how this comment applies to our results. The Aqua LWP is slightly more overestimated than the Terra LWP for C6 so we do not see how this comment could apply to this result either.*

pg. 9, lines 19-20: The increase in maximum tau is unlikely to be an explanation for the difference. The maximum C6 LWP is  $450 \text{ gm}^{-2}$ . Assuming a relatively small Re of  $10 \text{ }\mu\text{m}$  would correspond to  $\text{COD} = 67.5$ . Perhaps, there are larger uncertainties in the MWR data or something else going on.

*We have removed the part of the sentence regarding the increase in the maximum tau.*

*The discrepancies occur for large LWP implying thick clouds The relative uncertainty in the ground-based MWR retrieval should be constant up until about  $700 \text{ g m}^{-2}$  so that MWR uncertainty is not likely to be responsible for the great increase in uncertainty seen for large LWP. The selection criteria should extract reasonably homogeneous stratocumulus (in terms of cloud coverage, cloud depth, LWP, etc.) so that this also unlikely to be a viewing angle issue. We suspect this may be a result of high variability in the entrainment that the deeper clouds are exposed to – the satellite is most sensitive to the upper part of the cloud deck, and the impact of entrainment variability also most likely to have an impact on the upper part of the cloud deck. However, investigating this effect requires accurate LWC profiles, which are not yet a straightforward retrieval from cloud radar.*

pg. 10, lines 1-3: The sentence suggests that the MODIS retrievals used by CERES are the same as those used in the present comparison. They are not. The CERES MODIS retrievals were done with different algorithms, those described in the reference, Minnis et al. (2011). Please clarify.

*We have added the following sentence “However, the CERES team uses different cloud retrieval algorithms to the MODIS team.”. We have also changed “cloud retrievals” to “data” in the original sentence.*

pg. 10, lines 6-7: First, the comparisons were performed over the Azores, not the Canaries. Second, the LWP difference is  $13.5 \text{ gm}^{-2}$  if the larger satellite area is used, but the difference is  $-3.3 \text{ gm}^{-2}$  if only pixels over the site are used due to island effects, which make the large area averages unrepresentative of site. Are there any systematic spatial variations over the Finland site (on coast, on a hill, etc.)?

*We have corrected the location from the Canaries to the Azores. At Hyttiälä there is no systematic spatial variation (orography, land use) that would impact clouds.*

Last paragraph, section 4: It appears that for the present LWP comparisons, the C6 results are not any better than C5, maybe even slightly worse. But they are better over this site compared to other sites. Why? Any thoughts on that?

*We believe that the homogeneous spatial conditions and relative clean air at the site provide good conditions for cloud retrievals.*

Tables: Why are median height differences used instead of mean heights? What are the means? If you report medians, then means should also be included.

*We have used the median height difference instead of mean since most of the distributions were not normally distributed and contained outliers that would significantly affect the means. We have added the means to the tables for completeness and added the following sentences to section 3.1: “Both the median and mean differences are reported in the tables for completeness. However, only the medians will be discussed since the differences were generally not normally distributed and often contained outliers which significantly affected the means”*