

Interactive comment on “Comparison between the first Odin-SMR, Aura MLS and CloudSat retrievals of cloud ice mass in the upper tropical troposphere” by P. Eriksson et al.

Anonymous Referee #1

Received and published: 12 September 2007

Refereed review of “Comparison between the first Odin-SMR, Aura MLS, and CloudSat retrievals of cloud ice mass in the upper tropical troposphere, by P. Eriksson et al., published in ACPD on 14 Aug 2007.

General comments:

This paper describes inter-comparisons of a set of initial ice water path retrievals between three prominent microwave satellite instruments: the sub-millimeter microwave radiometer (SMR) on Odin, the Microwave Limb Sounder (MLS) on Aura, and CloudSat, with an additional comparison to Atmospheric Radiation Measurement (ARM) program millimeter-wave cloud radars in the tropical western Pacific Ocean. Not only com-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

parisons are shown, there is significant discussion devoted to the investigation of why the different observational platforms retrieve different amounts of ice mass. The vertical response of the SMR is shown, along with the relative sensitivity of the microwave instruments (and a comparable IR measurement) as a function of particle size, and more refined estimates for corrections due to beam filling effects are discussed. Although the data presented is limited in extent, global maps within the tropical latitudes are shown, and probability density functions for the different platforms are compared and explained in detail.

Overall, the paper is fairly well organized, the figures are concise, new information on ice cloud retrievals in the upper tropical troposphere is presented, thus it deserves to be published and is relevant to the subject matter of Atmos. Chem. Phys. However, some additional clarification must be made in regards to the methodology of comparison as well as the sensitivity studies. In particular, the reviewer was unsatisfied with the authors ascribing differences between the retrievals to assumed particle size distributions, but no sensitivity study was shown or referred to. Also, there are several ambiguous statements due to the improper use of English that affect the meaning, interpretation, and readability of the manuscript; these are highlighted in the “technical corrections” section at the end of this review. The editing suggestions are not complete and the authors are advised to thoroughly edit the manuscript. All relevant technical, scientific, and methodological comments are found below in the “specific comments” section.

Specific comments:

- p. 12036 (abstract), line 5: Is it “11” or “12” km? At least two other places in the manuscript say “11 km” (pp. 12040 and 12051)
- p. 12039: You cite the Davis et al. (2006) paper. May want to consider adding as text the main results that are relevant to this work.
- p. 12040, lines 24-25 and afterwards to end of Section 2.1: There is no discussion

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

of the particular causes of why the atmosphere is opaque near the tangent point. Are atmospheric $T(z)$ and $RH(z)$ profiles needed to infer dT_{sub_b} like with MLS? If so, where do they come from? What additional factors control its sensitivity? Some additional clarification on the retrieval would be useful. What about the vertical resolution of the SMR? Why is only one particle size distribution used even though later on in the paper it is demonstrated this quantity controls some of the sensitivity? There is no error estimate due to the assumption of one PSD.

p. 12045, lines 20-21: This is true among the microwave measurements, but are the authors claiming better retrievals over IR measurements?

p. 12046: Why not calculate the sensitivity of dT_{sub_b} to different assumed PSDs? This parameter is argued to be important and control a significant (but un-quantified) portion of brightness temperature variability and should be demonstrated by the authors. This could potentially be done on a small sub-sample of representative SMR observations.

p. 12046-12047: The use of CloudSat data to construct Fig. 3 could use some more clarification. Is the antenna response, r , a “spatial” response? Is the rectangular response an approximation or fairly precise, and if not, what is the potential error? Why 30 CloudSat profiles per SMR field of view? If the horizontal resolution is 45 km, and CloudSat samples ~ 1 km resolution, why not 45 CloudSat profiles?

p. 12047, lines 8-9: What are the specific details regarding the “non-linearity” from the PSD? Is this separate from the non-linearities due to radiative effects, mentioned below?

p. 12047, lines 22-24: Regarding the sensitivity of the IWP to corrections to dT_{sub_b} , should really stress the point that the overall magnitude of IWP is very sensitive to this correction (over a factor 2 difference from 0-20% correction). Does the red line (revised correction) represent the mean value of the scatter?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p. 12048: What about adding variability statistics? Given the large scatter in Figure 5, the cited values of IWP in the abstract and conclusions should be accompanied by +/- variability of IWP

p. 12050, line 8: For the PDF, any values $< 4 \text{ g/m}^2$ are not included from the SMR because of noise. Yet, in the abstract and conclusions, the mean value of the SMR is on the order of 4 g/m^2 or less, depending on other factors. How is this justified? What is the mean if all of these noisy points are removed? Perhaps the reviewer misunderstood the arguments here (if that is the case, the authors need to re-work this section carefully), but the reviewer had difficulty understanding that the detection threshold is on the order of, or slightly greater than the mean value of IWP. If this is correct, how can we trust the results of the SMR IWP retrievals?

p. 12050, line 16: What precisely is the “perspective given” to Section 3.2?

p. 12050, lines 23-25: This is a very important point, in that a slight change in the correction factor could cause a false “mode” in IWP (e.g., a secondary peak at large IWP). Good point about CloudSat and preliminary IWP retrievals, these results will change with successive re-processing efforts.

p. 12051, line 4: Does a 40s average correspond to a 200 km MLS limb horizontal dimension?

p. 12051, line 11: Please add the particular ARM sites used (Nauru? Manus? Darwin?). Should add Ackerman and Stokes (2003), Physics Today reference or other prominent ARM reference. Which ARM data products did the authors use? Should cite all relevant works. Also, need to discuss sampling biases. How do the authors handle the ARM vertical IWP profile, is it limited to above 11 (or 12?) km? Clearly this will be more problematic in precipitating systems because of beam attenuation, although in these cases SMR and CloudSat could sense the tenuous cirrus/anvil clouds that the MMCR will miss. Need to discuss these caveats and reference appropriate works. It is remarkable that the IWP values from ARM falls in between these two measurement

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

platforms. How many ARM profiles were used and do they represent the full range of geophysical variability?

p. 12051, line 26: Why does CloudSat have the least complicated error budget?

p. 12052, line 17 and onwards: Can't really visualize spatial patterns with low IWP because color scale fades to white, need more iterations, more color variability.; line23: What is meant by "Comparable mean values"? this has to do with data availability or something else?

p. 12052, line 29: The reviewer is not convinced of anything regarding the PSD because no appropriate sensitivity study was shown or cited (to the reviewer's knowledge), as discussed earlier. This is a very important claim and more can be shown to support it.

p. 12053, line 13: The 1.2 g/m^2 figure represents the scatter due to spatial heterogeneity/ bean filling, or other factors? What about PSD effects? Need to elaborate on this in the Results section, and possibly extend to other platforms.

p. 12053, line 17: Should add Li et al. (2005) reference along with John and Soden (2006). This inference is entirely correct and important to stress.

p. 12057, Figure 1: Which channel is this vertical response valid for? Can multiple responses be shown for different channels?

p. 12060, Figure 4: The color scale is not sufficient to see variability for low IWP boxes/pixelation. Same goes for Figs 6 and 7.

p. 12061, Figure 5: The scatter suggests to the reviewer that the cited numbers of mean IWP in the abstract and conclusions should be accompanied by variability values as well. There is lots of scatter between the data sets.

Technical corrections:

p. 12036 (abstract), lines 13 and onward: The use of "compensation" is awkward. Is

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

this the bias “correction”? Consider re-wording for clarity.

p. 12037, line 11: “wavelengths” should be “wavelength”; line 15: “This as the scattering” is awkward and unclear; line 26: “so far” is unclear; lines 28-29: Could use a re-write (“observation” should be “observing”)

p. 12038, line 6: What do you mean by “remaining”? Same line: what about adding a reference for Odin-SMR? Is there anything in the refereed literature you can cite?; lines 27-28: should be “Retrievals have been released”; line 29: should be “ice mass is now”

p. 12039, line 3: should be “between the first”; line 7: should “main” be “the primary”?; line 10: “masses” should be “mass” (singular); lines 16-17: should be “Different values of cloud ice mass”; line 22: suggested re-wording “This section describes the datasets involved in the inter-comparison”

p. 12040, lines 22-23: suggested re-wording “Scattering and the data used here are taken from”

p. 12044, line 14: wording is unclear; line 24: suggested wording change: “This aspect is summarized in Fig. 2, for the satellite observations considered here, and a”

p. 12045, line 1: “for considered data” not clear. Which data is considered?; line 19: should be “within a factor of 2”; line 20: change “choice here” to “choice”; line 28: delete “accordingly”; line 29: “this particularly” is ambiguous

p. 12046, line 28: suggested change to “the results are given in Fig. 3”

p. 12047, line 20: delete “too”; line 27: change “observation” to “work”

p. 12049, line 2: change “on that” to “that”

p. 12050, line 7: PDF should be PDFs; line 14: very unclear sentence; line 21: should be “SMR indicates false”; line 25: change to “an indication that a”; line 28: change “Cloudsat PDF for the provisional R03 data product is relatively”

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

p. 12051, line 16: re-word to “The main differences”; line 17: “is consistently above CloudSat.”; line 22: “Data from the first”. Also, there will be more Odin satellites? If so, should discuss in introduction.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12035, 2007.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper