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Atmospheric Measurement Techniques Discussions
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Interactive comment on "CloudSat-constrained cloud ice water path and cloud top height retrievals from MHS 157 and 183.3 GHz radiances" *by* J. Gong and D. L. Wu

Anonymous Referee #1

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The core topic of the manuscript is retrieval of ice water path (IWP) from MHS-type sensors. The only (at least well-known) dataset of this type is MSPSS, but it has been shown to have a large bias and, accordingly, the manuscript treats a very important subject. It can here be mentioned that a similar dataset is also in the process of review. It is denoted as SPARE-ICE, see www.sat.ltu.se. This fact does not make the work of Gong and Wu less important. The opposite, it is a sign on that improvements of these retrievals is urgently needed, as well as that their basic methodology (building an empirical forward model by collocations with CloudSat) is a good idea.

Hence, the scientific importance of the manuscript is accordingly high, but there are shortcomings in both the presentation and efforts performed that require consideration. The main issues are:

1. No error analysis is presented. My opinion is that a retrieval is of little value until an useful error estimate is given. I understand that exact values not can be given for these retrievals, but an estimate must still be provided. The errors in the CloudSat retrievals propagate into this dataset, but there are also additional error sources. One such source is collocation mismatches. Another is that the CloudSat data just cover a part of the MHS footprint and the effect of so called beam filling is not totally covered by the collocation approach. It would also be useful to have an estimate of the fraction of false cloud defections (more below).

2. The scope is unclear. Will the dataset be made public available? Can it be extended to higher latitudes? It is not clearly defined how IWP is here defined. What is the exact lower limit? And with respect to readers with a model background, is IWP here including both "cloud ice" and "precipitating ice" (snow)?

3. The comparison between the two radiative transfer models provides no useful information and should be removed. In fact, as it is presented, it rather gives an incorrect view of the performance of forward model (more below).

4. Essential information is lacking in several places. On the other hand, quite a lot of irrelevant information is found. Examples are given below. The manuscript could be made considerably shorter.

In summary, the main changes I suggest is to add an error analysis, while removing the radiative transfer comparison.

Detailed comments:

P8188L9: Unclear sentence. In addition, I would suggest to use "empirical relationship" instead of "forward model". Yes, a forward model can be empirical, but "forward model" gives the impression of something more advanced than the very simple relationships

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derived here.

P8188L15: I found this comment highly misleading. There exist several forward models that can treat all relevant aspects of the simulation problem. The issue is instead big uncertainties in the input to the forward model, mainly what single scattering properties to apply. This distinction must be made clear, here, in the abstract and in other places.

P8188L20: In LaTeX terminology \citep should be used for Stephens et al. This error is repeated at other places.

P8190L13: This is not generally true, only valid for relatively low Tcir, as made clear by Eq 2. Or if true, only a small part of the possible range of Tcir is actually used, and the IWP-range of the retrievals is low, which is a drawback.

P8190L18: It is reversed, a RTM or forward model relates radiative properties to radiance.

P8190L19: Is there any empirically RTM?

P8190L20: This comment is irrelevant.

P8190L22: Please, distinguish between forward model input and simplifications made for efficiency reasons. And that there exist forward models without any such simplifications.

P8192L24: Another missing \citep.

P8193L5: More common is to say that this is valid in the Rayleigh limit. Anyhow, the expression "Mie scattering" is vague, used differently in different contexts.

P8194P5: Several comments here are not very relevant as those instruments not used.

P8195L14: Why making an interpolation of CloudSat's IWC. Both IWP and pIWP can be calculated with the original data.

P8195L16: This comment indicates that retrievals are tested for different pIWP, but this is never done. Or do I miss something? Anyhow, please clearly define the IWP product that you output. A comment here is that setting a lower altitude limit for the IWP is not ideal. A more common approach is to specify the lower limit as a temperature. This as the main IWP retrieval problem for CloudSat is to discriminate between liquid and ice particles. The limit between liquid and ice has rather a temperature threshold, than an altitude one.

P8195L27: CloudSat can be "truth" for creating the empirical relationships, but its errors must still be considered when making an error assessment.

P8195L28: Comment not totally logical. Yes, microwaves penetrate deeper and should be better for IWP retrievals, but you can apply CloudSat collocations also for IR/VIS (as done in SPARE-ICE).

Eq1: Explain the meaning of CIR and CCR. This makes it easier to remember what e.g. Tccr stands for.

P8196L15: "sate" should be "state".

P8196L20: How is "significant" defined here? Or why not just give a value of the maximum difference?

P8197L8: What is a "clear-sky surface"?

P8197L28: Specify if 5K is one standard deviation or something else.

P8198L6: Should be 157 GHz here.

P8198L20-29: Several unclear points here (but anyhow removed if the forward model comparison is removed.

P8199L18-22: Irrelevant comments.

P8199L25: Repetition of information.

P8200L1: 60 000 samples, does that mean 60000 MHS footprints? Or a lower value

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of footprints, where each footprint is connected to several CloudSat profiles? That is, it would be helpful to describe the complete collocation approach here. Are single CloudSat profile used, or averages? If averages, are all these CloudSat profiles inside 10 km? Or suffice that one CloudSat profile is inside 10 km?

P8200L1: The latitude range is only given inside parenthesis, despite it is crucial information. Part of the actual scope of the work.

P8200L25: A repetition of lambdaˆ4 relationship! Apparently, the Rayleigh limit is discussed here. In this limit, the particle extinction is proportional to radiusˆ6,, while the particle mass (IWP is a mass measure) is proportional to radiusˆ3. Thus, the first part of the sentence is incorrect.

P8200L21: Why this threshold for defining ht? It is probably reasonable, but a motivation is needed. In fact, a small sensitivity analysis of the threshold would be highly interesting. And a general comment: In abstract and conclusions, it should be made clear that ht is not the "radiative" top altitude, but a more instrument specific altitude, more related to mass.

P8201L10: Incomplete sentence.

P8201L24: Does "linear regression" here mean assuming a direct linear relationship between IWP and Tcir?

P8202L8: Remaining 52%, at intermediate levels or outside?

P8202L26: The comment here indicates that CRM only can treat limb sounding geometry. If true, why comparing CRTM with a model with such a strong limitation? If false, this part must be made clearer.

P8203L12: I can not find any error bar for "without height separation".

Eq5: Don't see the point in citing Livesey et al. here, as they did not add anything to what is found in Rodgers (2000). Write out the part that is here denoted as Sx (Sx is

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here just recombination of the other matrices, I got confused as Sx is frequently used for the matrix here denoted as Sa). Seems unnecessary to be needed to download Livesey et al. to understand the equation. Write out that this is just the Gauss-Newton version of OEM.

P8205L2: First of all, I don't follow this. Anyhow, my interpretation of the text is that x(q) is always equal to a. If correct why include this term in Eq5 at all, as it is then always zero?

P8205L13: This is a common misunderstanding about Sa. This matrix describes only the a priori uncertainty, it does not control the step length, nor sets a limit on the final solution (all solutions has probability>0 and can occur, especially if the input radiances are corrupt in some way).

Sec 4.1: It is indirectly described that the IWP PDF depends on horizontal averaging, would be good to express this more clearly. Is the same averaging of CloudSat used here as applied in the collocation process? It seems not, why change?

P8207L9: <10%, refers to change in individual average IWP or in PDF values.

P8208L25: This aliasing is not clear to me. Please explain. One significant aliasing effects should be diurnal variations. I would say that the "spottiness" is mainly due a non-sufficient sampling of non-Gaussian statistics.

P8209L14: Surface emissivity probably a more important issue.

Sec 4.2: My guess is that a large part of the difference to CloudSat is due to "false detections", at least over oceans. See eg. higher values east of Australia for MHS. This section should discuss this possibility. And somewhere an estimate of false detection rate is needed.

Sec 4:3: Why a special study for +-(25-30)deg? This is a relatively small "extrapolation" from the -25to+25deg used for setting up the retrievals. The basic question is if the methodology can be extended to give global coverage (are there sufficient colloca-

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tions)?

P8211L1: Did Wu and Jiang study this for limb or downward looking observations? If limb, the comment is not relevant.

Sec 4.4: A meaningful comparison between the models requires likely a complete study in itself. The differences are significant and many tests are needed to understand the source the discrepancy. Anyhow, many aspects are here unclear, such as what simplifications do the models use, are both models actually using the same microphysical assumptions and can some difference originate in different parameterisation of water vapour absorption? If the motivation is to plan for model development, why not compare to a validated model where the simplifications are kept as low as possible? Such a model is probably slow, but this amount of testing can anyhow easily be performed.

Sec 5: The conclusions should be revised, considering comments above.

P8215L14: This is just the "zenith angle", not the solar one. Same issue in text of Fig A1.

P8215L19: Took me a long time to figure out that the value 2.1 comes from the figure.

P8216L13: The term "limb darkening" is probably not know to everybody. What is Tside?

P8216L15: I would guess that surface emissivity assumed is the main cause to the bias.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 8187, 2013.