We are grateful to the two reviewers' constructive comments. We've revised the manuscript accordingly. The two major revisions are: (1) move section 4.4 to Appendix C. (2) add the error analysis in section 4.4.

Comments from reviewer #2:

The approach to retrieving IWP from operational microwave sensors is innovative. This paper should be published after some revising. The authors show in a convincing way that the new IWP product is likely to be much more accurate or at least more similar to a CloudSat measurement than the existing NOAA operational IWP product. I am assuming of coarse that the CloudSat ice measurement is the best and most accurate available. The idea that the authors have exploited is using CloudSat ice measurements co-located with MHS (NOAA-18) cloud induced radiance measurements to establish monotonic relationships between ice water path and measured radiances. In effect the CloudSat and co-located MHS radiance correlations are being used in lieu of a radiative transfer model and serve as a training set in a neural network sense. The authors find that in addition to IWP, there is also a correlation with cloud top height.

Therefore in effect they train on these two variables. The authors demonstrate that the new data set potentially will be more useful for some applications than either CloudSat or the NOAA product and the end result will simulate in an imaging sort of way, what CloudSat might provide if it could measure swaths. The major issue with the paper is that as presented, it is a demonstration of an approach as opposed to description of a scientific product. These are some things that need to be addressed if this approach were to be used to produce a scientifically useful data set.

1) the product needs to have estimates of uncertainties. I think two simple things that could be done here is to include CloudSat's estimate of 40% plus the scatter of a correlation function derived from the MHS/IWP scatter plots taking into account the cloud top dependence. This would be a minimum error budget but it is a starting place.

Thanks for pointing out the lack of error analysis in this paper. Now it is included as a separated sub-section (section 4.4).
In short, the error sources are CloudSat (~40%), retrieval uncertainty (~25%), and the mismatch of collocation points (hard to estimate, but < 10%).

2) IWP (or more specifically pIWP) needs to be defined, that is where does the column integration start.
This algorithm retrieves total column IWP, instead of pIWP. That is based on the assumption that CloudSat is the truth. As the IWC retrieval algorithm from CloudSat is purely temperature based, it hardly gives you an ice cloud below the freezing level in the tropics. Therefore, these MW channels would merely encounter the problem of saturation of Tcir.

3) The cloud top height product needs to be dropped from the title as in the current form it is mostly a parameter used to improve the IWP retrieval. Respect what is said in line 13 page 8208.
We agree with the reviewer that cloud top height is a by-product of this algorithm. However, it gives a rough estimation of the true cloud top height when other more precise measurements (e.g., IR) do not exist. So it may potentially operationally useful given the aforementioned situation.

4) Is this a product that will be produced for public use. If so it probably should only have data from +/- 30 degrees because the paper states that the retrieval is not good for higher latitudes.
Yes, it is available to the public for free upon request. We emphasized several times in the paper that this algorithm is only valid up to +/- 30 deg. We are currently working on the mid-latitude scheme, where temperature lapse rate becomes the dominant factor, while cloud top height merely has an impact. Therefore, it will be a slight modification of the algorithm at mid-latitudes.
Our scope is to eventually publish a publically available IWP dataset up to +/- 60 deg. Beyond that latitude, surface snow/ice becomes so important that a simple empirical model cannot handle.

Minor comments:
8189 line 15 assumption –> assumptions
Suggestion adopted.

8195 line 4 (after sec 2.1.2) change to to into
Suggestion adopted.

8196 line 12 (sec 2.1.3) statistically to statistical
Suggestion adopted.

8197 line 5 (sec 2.1.3) readily to ready
Suggestion adopted.

8197 line 16 the pdf peak would still be broadened by radiance noise right?
You are right. Now the wording is changed to “all negative values that are smaller than the radiance noise would be classified as clouds”.

8201 line 17 showed to shown
Suggestion adopted.

8202 line 23 For a given channel TAB is the same... As what?
Comparing thick and thin ice clouds, T_{AB} remains the same. It’s clarified in the text now.

8202 line 22 I would say beneath/below rather than behind
Changed to “beneath”.

8206 line 2 rests –> rest
Suggestion adopted.

8206 line 20 delete hour
Suggestion adopted.

8207 line 16 I think the term slightly is optimistic given that the disagreement approaches a factor of two.
A typical IWP difference among different satellite retrieval products is one or two magnitudes. So a factor of two is quite acceptable and a great improvement.

8207 line 27 Maybe the noise level is set too high if the algorithm is detecting features at the sub noise level.
The noise level is determined from the error bar of Tcir-IWP relationship shown in Fig. 4. Our algorithm probably still has sensitivity below the noise level, but we don't know if it is trustable or not. In this case, it's trustable.

8208 lines 7–10. If the Ht is off by 5 km would that adversely affect the IWP retrieval.
You are absolutely correct. I suspect the jump of color gradient on the IWP map at the hurricane periphery is probably associated with this impact.

8209 line 5 The day night difference is not so obvious to me in the figure. But this might be expected because the day night sampling times of the CloudSat orbit are very poor for observing the temporal behavior of tropical land convection.
Agreed.

8209 line 20 change to maritime continent
Suggestion adopted.

8210 line 26 change 0K to 273 K or 0C.
Suggestion adopted.

8213 line 15 adviced to advised
Suggestion adopted.

8214 line 5 change still has issues in to is not accurate for.
Suggestion adopted.

8214 line 17 a statement saying we found our results closer to CloudSat IWP, closer than what, the NOAA product?
Than many other satellite IWP products, including MODIS and NOAA product. See Wu et al. [2009] for a comparison with other products.

8214 line18 change substituting with replacing.
Suggestion adopted.

8216 line 15 I would replace corridendum with errors.
Suggestion adopted.

Figure 7. Where CloudSat measures IWP below the MHS IWP I would color white rather than black so it does not contrast with the MHS measurement, but keep a thin black cross to show the measurement track. I would also say Cuba is the island to the left of the plot.
For the former comment, now we add a sentence to clarify it in the figure caption. We choose to not change the figure because CloudSat retrieved zero IWP value, while we don't do the IWP retrieval or the retrieval is failed because it's below our sensitivity. For the latter, thanks for pointing out this error!

Figure 10 I would remove the 1:1 line as it has no physical relevance here. I found it impossible to distinguish the assortment of black symbols from each other.
1:1 line is important as it shows which channel depresses more. According to Mie theory, which is the theoretical basis of MLS RTM, 183+7 GHz channel should have a larger depression than 150 GHz channel, but the observation is contradictory.

For the symbols, the intent is to use color symbols to represent the two model results, while all black symbols are from the observation. Sorry for the confusion. It's now clarified in the figure caption.