Response to Reviewer # 2 (Manuscript ID: amt-2019-223)

First of all, we would like to thank the reviewers for their valuable comments. In the revised manuscript, we have accommodated all the suggested changes into consideration and revised the manuscript accordingly. The reviewers' comments are copied here as texts in BLACK. The authors' responses are followed in BLUE, and our changes in the manuscript are in *italics*.

Reviewer # 2

General Comments: This paper is about assessment of cloud properties from the re- analysis with satellite data over East Asia. Three sets of reanalysis data are used, including the newly developed China Meteorological Administration Reanalysis data (CRA), the ECMWF's Fifth-generation Reanalysis (ERA5), and the Modern-Era Retrospective Analysis for Applications, Version 2 (MERRA-2). And, to avoid the unrealistic assumptions and uncertainties on satellite retrieval algorithms and products, a radiative transfer model (CRTM) is used to transform reanalysis data into radiance/brightness temperature that can be directly compared with the Himawari-8 satellite data. Although cloud properties from CRA, ERA5, and MERRA-2 have their own advantages, the results show that ERA5 reanalysis data is best representative of cloudy atmosphere over East Asia, while the results in CRA are close to those in ERA5. This study may contribute to the improvement of cloudy property representation in models and satellite observations. This paper is within the scope of Atmospheric Measurement Techniques but some improvement should be conducted before the paper could be accepted for publication.

Major concerns:

1. The authors claim that the radiance-based evaluation approach could avoid unrealistic assumptions and uncertainties on satellite retrieval algorithms and products, and thus it is a better way to carry out the assessment of cloud properties from various reanalysis. However, I would say I only partially agree with the authors on the perspective that the conventional way to compare cloud variables could be still indispensable. Without knowing the quantitative and qualitative differences in cloud properties, it is still hard to explain the radiance/brightness temperature differences resulting from the radiative transfer modeling. Thus, more discussion about the cloud optical properties should be added.

Response: We agree with the reviewer that the comparisons with retrieved cloud products are still necessary for assessment of model simulations. As we have discussed in the Introduction Section (as well as Figs. 1 and 2 in the original submission), such direct comparison may be also problematic due to the uncertainties related to retrieval product. Of course, the radiance-based evaluation has its own disadvantages as well. Thus, we decided to focus only on the radiance-based evaluation, and more detailed quantitative and qualitative evaluation based on direct comparison is suggested be performed in further independent studies. Besides removing the retrieval-based evaluation parts, we also included the following discussion in the revision (Lines 73-77):

"*The retrieval-based evaluation is an indispensable approach in the evaluation of atmospheric properties from various simulations, and quantitative and qualitative analysis of the cloud optical properties, e.g., the cloud effective radius and optical depth, can be evaluated directly. However, to avoid uncertainties associated with satellite retrieval algorithms and platforms, another alternative radiance-based comparison is chosen for the cloud properties assessment in our study.*"

2. Previous studies (i.e., Yi et al., JGR, 2017a, b) indicate that a consistent cloud optical property parameterization scheme should be used in satellite retrievals and modeling studies to well simulate the radiance/flux at the top of the atmosphere under cloudy sky. Any mismatch in cloud optics parameterization could induce large bias in the retrieval and simulations. Taking that into account, it seems the study here using CRTM with a new set of cloud optical property look up tables (it is also not clear what kind of ice cloud particle model is used) that is inconsistent with the Himawari-8 cloud retrieval algorithm, could be potentially problematic in the satellite radiance/brightness temperature simulation. The authors may need to consider using the Voronoi ice scattering model by Letu et al. (2016; 2018).

Response: We agree with the reviewer that inconsistent cloud optical property models could be a potential problem for the differences in different satellite retrievals. This is the reason that we think the retrieval-based evaluation can be problematic. We have omitted the figures showing the direct comparison. In our radiance-based evaluation, no satellite cloud product is used, so such differences for different cloud product will not influence our results.

Meanwhile, we clarified that the optical properties of aggregate columns with eight elements and severe surface roughness are used for CRTM. We think it is interesting to check the influence of cloud optical property parameterization on our evaluation, and this is suggested as a future study as following (Lines 173-176):

"*It should be noted that schemes for both cloud optical properties (e.g., ice cloud model) in the RTM and coupling between atmospheric reanalysis and RTM (e.g., approximation of cloud effective radius) may influence simulated BTs/reflectances, although the influences are relatively minor compared to presences of clouds (cloud amount). The potential numerical uncertainties due to different schemes will be performed with more details in further studies.*"

3. Apart from the potential problem in cloud optical property, another important issue is about the differences in the atmospheric profiles. The simulated radiance/brightness temperature is closely related with the atmospheric profiles. Whereas, differences in the atmospheric profiles among the reanalysis datasets are prevalent. And these differences may contribute to the simulated results under cloudy sky. Thus, I think it would be best that the authors provide some analysis of the clear-sky evaluations (maybe in appendix). This would be helpful for the reader to distinguish the impacts of atmospheric profiles and the cloud properties.

Response: Thanks for the suggestion. It is interesting and meaningful to consider the cloudy and clear-sky pixels separately and to evaluation the contributions from cloud or atmospheric profiles.

(1) First, for the solar channel results (Figs. 1 and 3 in the new version), the differences are almost all contributed by cloud representation, because atmospheric profiles have little effect on the reflectance in the 0.64- and 1.6-μm channel. We added brief discussion and analysis in the revised paper.

(2) Comparison between simulated and observed BTs in the IR channels does show the overall performances of the reanalysis data due to both cloudy and atmospheric profiles. However, the discussion and classification based on BTDs can significantly remove the influence of atmospheric profiles, because the BTDs between the selected channels are mostly influenced by the cloud properties (e.g., cloud height and cloud amount).

(3) Furthermore, we include the following discussions in the revision. If pixels are separated as cloudy or clear ones based on a criterion of 0.1 for the integrated column cloud optical depth in each pixel, the figure below shows the pixel-to-pixel comparisons between observed and simulated BTs in the 11.2-μm channel. The top row is for cloudy pixels, and the bottom one is for clear-sky pixels. Larger correlation values for the clear pixels indicate that the cloud properties do significantly contribute to the differences.

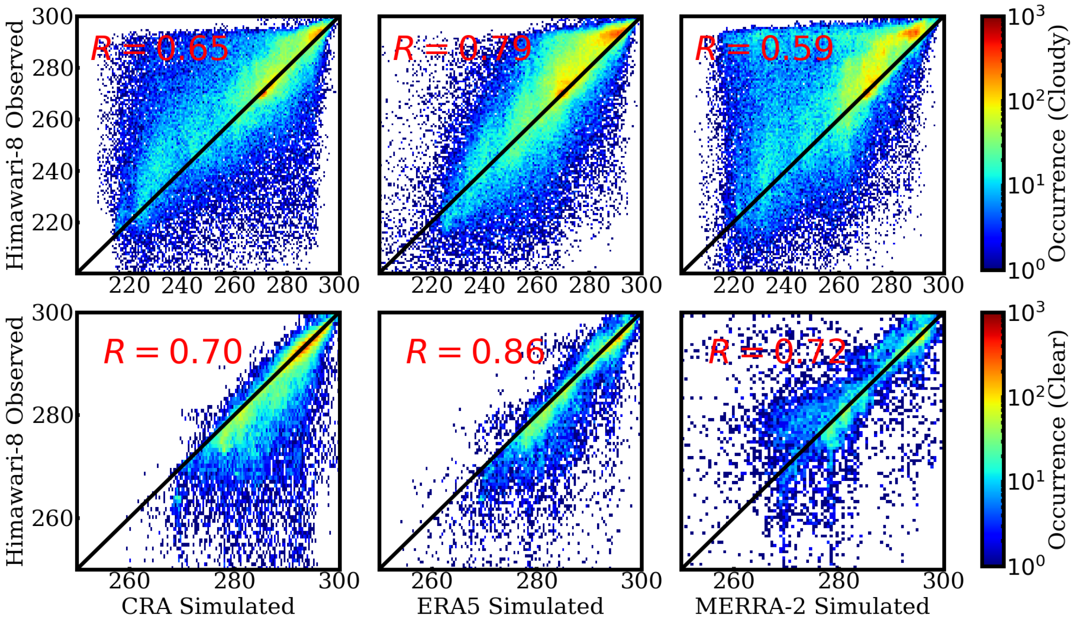

Figure 1. Pixel-to-pixel comparisons between the observed and simulated BTs in the 11.2-μm channel. Top panels indicate the comparison for cloudy pixels, and the bottom panels show the comparison for clear pixels. The results are taken at 00:00 (UTC) on 12 September 2016.

(4) Last but not the least, the reviewer raised an interesting and important point, which should and will be done in the future, we have added the following discussion (Lines 410-413):

"*The radiance-based approach is a reliable choice for the evaluation to avoid uncertainties due to retrieval products, and its drawbacks may be investigated in further studies. For examples, differences between simulated and observed radiances can be contributed by both cloudy and atmospheric variables, and these may be distinguished by considering the same atmospheric profiles in the RTM simulations.*"

4. In part 3: methodology, to derive the necessary cloud property inputs for RTM, the authors also make quite a few assumptions. Especially in deriving the effective radius (Line 145), the used definition is somewhat different from those normally used in parameterization. As the effective radius is a very important quantity that decides the cloud optical properties in the parameterization, the authors need to analyze how the differences in the definition of effective radius will influence the results.

Response: The reviewer noticed an important point of our study. In fact, the couple between reanalysis cloud variables and RT simulations is one of the most essential parts of this study. We have tried our best to avoid empirical relationships for cloud property estimation.

(1) For water cloud, the effective radius scheme is based on Thompson et al. (2004) a popular scheme in mesoscale meteorological forecast models (e.g., the WRF model). The cloud number concentration over continent and ocean regions are assumed as typical and widely used values (Miles et al. 2000; Thompson et al., 2004; Wendisch and Yang, 2012).

(2) For ice clouds, the effective radius is physically estimated by mass extinction coefficient, which is given by an empirical relationship related to ice water content (Heymsfield and McFarquhar, 1996; Platt, 1997; Heymsfield et al. 2003), and the ice water content is from reanalysis directly.

(3) As also noticed by the reviewer, the coupling is far from being a done work. There could be multiple ways to estimate the effective radius. For example, in our previous study (Yao et al. 2018), the effective radius of ice particle is calculated based on ice crystal mass and mass-radius relation (Hong et al. 2004). The following table compares observations with simulated BTs calculated based on the schemes used in this study (Scheme A) and the previous study (Scheme B, Yao et al. 2018). The correlations between observations and simulations from two different radius parameterized schemes are close to each other, and slight differences are noticed for the mean BT differences (MBTD) and BTD standard deviation (SBTD). This indicates that the schemes for effective radius estimation matter, whereas the influences are limited. Considering the length and focus of this study, we will not include such discussion in the manuscript, but we do think such sensitive study is interesting for a further study.

Table 1. The mean BT difference (MBTD), BTD standard deviation (SBTD), and correlation coefficient (R) between the observation and simulations (simulations based on two different particle effective radius estimations).

Varibales	6.2-µm		11.2-µm	
	Scheme A	Scheme B	Scheme A	Scheme B
R	0.87	0.85	0.70	0.68
MBTD (K)	-0.52	-1.71	-1.71	-6.43
SBTD (K)	4.98	4.98	16.13	18.50

5. There are quite a few places in the text that are not clearly stated and are difficult to understand. For example:

Line 301: It is not clear how the probability and cumulative probability are calculated here. And how do you “obviously” figure out from Figure 7 that “total cloud is overestimated in ERA5 and MERRA-2”?

Response: Here the probability and cumulative probability indicate the occurrence of pixels with certain BTs.

The probability (P_{BT_o}) is numerically calculated as:

$$P_{BT_o} = \frac{Number\ of\ pixels\ with\ BT\ between\ BT_o - \Delta BT\ and\ BT_o + \Delta BT}{Total\ pixel\ number}$$

, and the cumulative probability (C_{BT_o}) is given by:

$$C_{BT_o} = \frac{Number\ of\ pixels\ with\ BT\ less\ than\ BT_o}{Total\ pixel\ number}$$

The cumulative probability distribution is a good metric to give the occurrence of cloud. If we simply use a BT threshold of ~ 275K in the 11.2-μm channel to distinguish the cloud (BT < the threshold) and clear-sky (BT > the threshold) pixels, the cumulative probability with BTs less than 275K is approximate 0.8 and 0.7 for MERRA-2 and ERA5, respectively, whereas the cumulative probability with BTs less than 270-280 K for CRA and Himawari-8 observation is only 0.6. This suggests that over the observational domain, ~80% of the MERRA-2 and ~70% of the ERA are covered by clouds, which is larger than that from the observation.

We have rephrased the discussion and analysis in the corresponding paragraph.

Line 348: How do you define “ratio of the simulation-to-observation frequency of pixels with particular BTs”?

Response: The “ratio of the simulation-to-observation frequency of pixels with particular BTs” is defined by the ratio of number of pixels with particular BT interval in simulation and observation. The value (RA) is numerically given by:

$$RA = \frac{Number\ of\ simulated\ pixles\ with\ between\ BT_a\ and\ BT_b}{Nummber\ of\ observed\ pixles\ with\ between\ BT_a\ and\ BT_b}$$

To better distinguish different clouds, the threshold of BTDs of 6.2 – 11.2-μm is used in the revision, and the corresponding explanation and discussion in the paragraph are rephrased.

Line 353: What does TCC mean?

Response: TCC here is the abbreviation of Total Cloud Cover, we have add the full name of it.

Line 376-377: How do you define mean error (MBTD) and standard error (SBTD) ?

Response: For each snapshot, the MBTD is the mean BTDs over the entire comparing region, and the SBTD is the corresponding standard deviation. The MBTD and SBTD are calculated over the whole Himawari-8 observation domain between simulated and observed BTs. We have clarified this in the revision.

6. Figure captions in this paper are not clear enough to show what the figures are about. For example: Figure 7 "Probability and cumulative probability density for the observed and simulated results . . ." – what kind of "results" do you have here? The authors failed to state the name of the variable.

Response: Sorry for the confusion. The "results" means the observed and simulated BTs or reflectances. We have rephrased the captions.

Figure 8 " The results are from Figure 4 marked by blue dashed lines" – couldn't see the "blue dash line" in Figure 4, and actually, there are too many elements in Figure 4.

Response: Sorry for the mistake. The caption has been changed into "*The profiles are for the track marked by blue solid lines Figure 2.*". The regions or tracks particular discussed in the text are marked by boxes or lines in the new Figure 2, and we have improved the figure. Furthermore, to present Figure 8 more clearly, we have removed the cloud mixing ratio panels.

Minor problems:

Line 33: "The ERA5 reanalysis is found the most capability . . ." should be "The ERA5 reanalysis is found to have the most capability . . ."

Response: Thanks, and we have updated the sentence.

Line 97: Do you have some references for the CRA-interim?

Response: Because the CRA reanalysis dataset is producing and it will be released in 2020, and only a few papers have been published. Two papers by Liao et al. (2018) and Wang et al. (2018), which discuss the datasets assimilated in the CRA, have been referred in the revision.

Line 142: "Ignore the uncertainties . . ." should be "Ignoring the uncertainties . . ."; In addition, is it reasonable to assume mixed phase cloud can be ignored?

Response: Thanks. We have changed the "Ignore the uncertainties …" to "Ignoring the uncertainties". In our study, we distinguish cloud with different phases based on the temperature profiles, so the mixed clouds are treated ice cloud and they are not ignored. We have tested that this would lead little bias, and clarified this in the revision.

Line 187: "The correlation between the two is small." – This sentence is vague, as it is not clear about what are "the two".

Response: It should be "the correlation between the CTT from CRA and the CTT from satellite retrieval based on the solar measurement". The section has been removed in the revision.

Line 191: "We notice that . . ." should be "It is noted that . . ."

Response: Thanks and we have removed the paragraph.

Line 215-217: The authors mentioned the cloud scattering properties in the CRTM are recalculated. Then some necessary validation and description are needed to prove the validity of the new implementation.

Response: The validation of the CRTM was done in our previous study (Yao et al., 2018). As discussed in Figure 1 of Yao et al. (2018), the BTDs between the CRTM and rigorous (DISROT+LBLRTM) simulations for ice and water clouds in different channels are generally less than 1 K, and they coverage to 0 K as cloud optical thickness increases to 10 or larger. We have added some discussion on the validation of the cloud optical properties in the CRTM model in the revision.

Line 230: "From" should be "from"

Response: Corrected.

Line 272: "with a mean BTs of . . ." should be "with a mean BT of ..."

Response: Thanks, and it has been corrected.

Line 324-325: "an abnormal excessive cloud mixing ratio" should be "an abnormally excessive cloud mixing ratio"

Response: Corrected.

Line 373: "as marked in region A in Figure A" – where is Figure A?

Response: It should be Figure 2 in the revision, and we have changed it.

Line 390: “the in-site observation”?

Response: We have changed it into “the in-situ observation”

Line 413: “demonstrate that . . .” should be “demonstrating that . . .”

Response: Thanks, and it has been corrected.

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