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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Linkage among Ice Crystal Microphysics, Mesoscale Dynamics and Cloud and Precipitation Structures Revealed by Collocated Microwave Radiometer and Multi-frequency Radar Observations" *by* Jie Gong et al.

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We highly appreciate Reviewer 2's in-depth review and constructive suggestions. The English/grammar errors pointed by this and other reviewers have been corrected. The revised draft has been carefully proofread by the 4th author, who is a native speaker. The missing colorbars have now been added back. Below are point-by-point responses (questions in black, and responses in blue).

Major concerns:

1. It is interesting to investigate which condition promotes higher PD, but when authors discuss the background atmosphere differences between High-PD and low-PD cases, the large-scale environment data that used in the analysis are actually from in-cloud or partially in-cloud pixels. This large-scale thermodynamic and kinematic fields have already modified by the convective systems. It is not representative of the environmental or thermodynamic conditions that the convective systems initiate and develop. The pre-storm thermodynamic profiles that prior to the convection should be used.

We used ECMWF-AUX dataset generated by CloudSat team, which is basically interpolating ECMWF 3-hourly 0.5 X 0.5 degree forecast to CloudSat footprint [Cronk and Partain, 2017 in references]. At this spatial resolution, deep convective core is largely unresolved but parameterized, although the mesoscale system is resolved in certain sense. Therefore, we feel it's appropriate to consider ECMWF-AUX data as the "background" atmosphere, and admittedly this is not a strict definition of "background" as it contains in-cloud and ambient circulation information (first paragraph in Section 3.2). Interestingly, one of this paper's coauthors had a completely opposite argument to this reviewer. She believed from mesoscale modeling point of view that the dynamic and thermodynamic conditions from ECMWF-AUX cannot represent any in-cloud circumstances, unless we were to use a cloud-resolving model simulation. Since Fig. 4 and 5 and related context are based on statistics of many samples, we believe it is representative of the ambient circulation difference.

2. The differences in large-scale conditions between High-PD and low-PD are found to be fairly small in the Tropics. I am wondering how much of this just from the land-ocean contrast or seasonal variabilities (wet vs. dry). It's worth further development.

This is an excellent point. We never thought about that direction. We've tested on separating the tropical samples to land vs. no-land (ocean+coastal), but we found no distinct differences. This is more or less expected to see because abundant water vapor in the tropics really smear out ocean-land contrast at 166 GHz when it's cloudy-sky. We didn't test the seasonal variability because sometimes we have 0 samples for a given scenario (e.g., dry season Indian monsoon plus American monsoon area contains 0 samples of "high-PD" scene, which doesn't mean there's no high-PD pixels, but just mean there's no collocated CloudSat-DPR observations that happen to have a "high-PD" GMI reading). We cannot construct meaningful statistics based on too few samples.

3. One thing I think necessary is to provide more context/details for certain aspects of the study, such as the radiative transfer simulation setup and assumptions, and necessary references for certain sentences.

Now a section 3.4 has been added to briefly introduce the models we used. The triple-frequency DFR simulations share the same set-up and scattering database with Leinonen and Szyrmer [2015], which was included at the end of 3rd paragraph. Simulated density isolines are replicated from [Liao and Meneghini, 2011], which were not explicitly explained but now has been included. Since the focus of this work is not to develop nor validate RTMs, these two sets of simulations are rather employed to facilitate qualitative interpretation of the observed features.

4. The colorbars are missing for almost all the shading plots.

Since the PDFs are always calculated to reference to the maximum value and the colorbar is linear, they are neglected. We realize this should not be omitted in a scientific paper. Now they are all added back. Thanks for the suggestion.

Minor comments:

1. L17. Specify 'high-frequency'.

High-frequency is channel frequency > 150 GHz. This has been added. Thanks.

2. L77. "while some of the recent products . . .", here needs references.

An example is given in the parentheses now: ". . .realistic habit [e.g., MODIS collection 6 assumed a bulk column-aggregate globally for its ice cloud properties retrieval, Platnick et al., 2017]."

3. L90-91. Give full names to TMI and MADRAS.

The acronyms have been spelled out now. Thanks.

4. L110. This section lacks references for the datasets and the instruments overall.

Two references (Skofronick-Jackson et al., 2018; 2019) and GPM website are now added. Thanks.

5. L129. In the paragraph, authors use "PD-TB", it is actually "PD-TBV". Please keep it consistent throughout the whole manuscript.

Thank you for notifying this mistake. We've corrected three places that PD-TB appears.

6. L137. "the PD-TB relationship is largely latitude-independent. . ." needs references for this sentence. Even though the mean tends to be similar, but the standard deviation may be different between tropical and high latitude events, which could add potential uncertainties to the regime definition.

The latitude-independence is reported in Fig. 4 of Gong and Wu [2017]. See the Fig. 1 for an example of 166 GHz global ocean statistics, and Fig. 2 for 45N land PDF. We can see that the 45N PDF looks not much different from that of the tropics. At higher latitude, the range of TB is smaller so we can only construct the right portion of the upside down bell-curve (dark red line in the left panel for example), but the curvature, the PD peak and where the peak occurs remain largely the same.

7. L139-L142 are confusing. Do you mean congestus in general lacks stratiform clouds? I do not think the reason for including shallow clouds should be the difference in area fraction of convective core and widespread stratiform. Please re-phrase it.

8. L162. “This dataset has been used by many other researcher. . .” Please provide references.

An example (Yin et al., 2017) has been given in the following sentences. Our previous works, including Gong et al. (2017) and Zeng et al. (2019) also employed this dataset. These two citations have been included. Thanks.

9. L178-180 Please provide references.

A reference of Kirstetter et al. [2014] has been added, where the authors have found consistently larger beam-filling effect due to subpixel inhomogeneity for convective rainfall versus stratiform rainfall using TRMM 2A25 product.

10. L208. Authors should make it clear to readers why regime 1 is defined as “deep convection”.

We agree with the reviewer that there is a counter-intuitive logic here in our definition. Usually people define “deep convection” based on the maximum radar reflectivity passing a certain threshold, which is more or less arbitrary as well, and the definition between CloudSat and DPR are not consistent because they work at different frequencies. Our definition is purely based on a 166 GHz TB threshold (TB < 150 K). This is of course arbitrary too.

If you revisit left panels in Fig.A2, you can see 150K corresponds to the very deepest depression of 166 GHz TB, which is the center of the deep convective line. Note that to make Fig. A2, we don’t set up any threshold but simply assign the coldest TB at each scan as the center of convective core. At 89 GHz this deep convective core ensemble is roughly < 225K. This value has been used previously in literatures to identify deep convections from TRMM TMI 85 GHz (e.g., Spencer et al., 1989; Nesbitt et al., 2000). So our 166 GHz threshold is consistent with 85 GHz threshold that studies used before. These two citations are now included to support our regime definition.

11. L239, L244, see major comment 2.

Please see our reply under major comment 2.

12. L249. Do these differences pass significant tests?

13. L283. RTM needs to be defined.

The acronym has been spelled out now. Thank you.

14. Figure 6. color bars are needed. Is this for the whole data or just tropical cases? The legend on Figure 6(a) is wrong.

It’s from the whole data samples. Legend has been fixed, and colorbars are added.

15. L360. ICI needs to be defined.

Ice Cloud Imager. The acronym has been spelled out now. Thank you for point that out.

16. Figure 9. The SD is very hard to see. color bar is missing.

I’m sorry but Fig. 9 are line plots so colorbars are not needed. And what does “SD” stand for?

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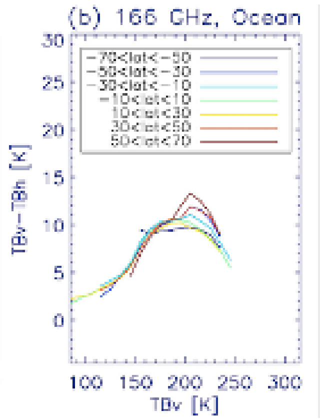
(b) 166 GHz, Ocean
-70<lat<-50
-50<lat<-30
-30<lat<-10
-10<lat<10
10<lat<30
30<lat<50
50<lat<70
TBv−TBh [K]
TBv [K]

Fig. 1.

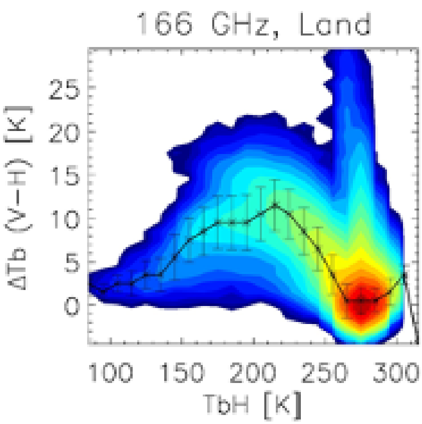
166 GHz, Land
ΔTb (V−H) [K]
TbH [K]

Fig. 2.

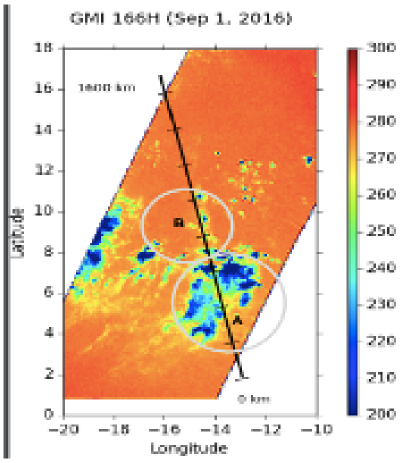
GMI 166H (Sep 1, 2016)
1600 km
0 km
Latitude
Longitude
B
A

Fig. 3.

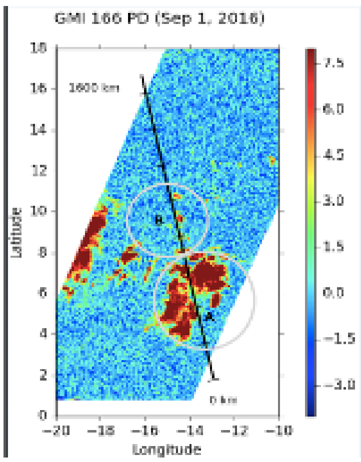
GMI 166 PD (Sep 1, 2016)
1600 km
0 km
Latitude
Longitude
B
A

Fig. 4.