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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.

Anonymous Referee #1

Received and published: 25 May 2020

General Comments:

This study is focusing on the ice related microphysical processes and scattering properties, which are significantly difficult, especially from satellite platform, but play an important role in the Earth's radiation budget. I appreciate that this study uses passive microwave measurements as the primary dataset, combined with space-borne radar observations to gain insight into the vertical structures of the ice properties. I com-

mend the authors for an interesting manuscript and an extensive work. I really like the comprehensive analysis in sections 4 and 5, especially when relating PD variations to different cloud life stages. It is a very interesting angle of looking at the deep convective cloud properties. However, I do have some major concerns mainly related to Section 3.

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.

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.

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.

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

Minor comments:

1. L17. Specify 'high-frequency'.

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

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

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

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

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. It would be nice to see the results for midlatitude and high latitude, e.g., Figure 2.

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.

9. L178-180 Please provide references.

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

11. L239, L244, see major comment #2.

12. L249. Do these differences pass significant tests?

13. L283. RTM needs to be defined.

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.

15. L360. ICI needs to be defined.

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

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