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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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Unfortunately, the paper is lacking significantly in terms of grammar and phrasing. Some sentences are difficult to read and understand. Examples are found under grammar/technical comments below. Careful proofreading is required before publication.

We highly appreciate Reviewer 4's in-depth review and constructive suggestions, especially on providing so many detailed suggestions on English/grammar, which are

really helpful. 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 comments:

1. While theoretical calculations are not the focus of the paper, only meant to augment the analysis, I think they could be described in more detail. For instance, what particle size distribution was assumed in the triple frequency calculations? More information can be given on the habits and the assumptions made for the riming model. This should not need more than two paragraphs I believe.

A new sub-section 2.4 is added to summarize the two RTMs and set-ups.

2. There is a lack of discussion regarding the sample size of the used data. Naturally, the amount of collocated GPM and CloudSat measurements of relevant cloud types are limited outside the polar regions. However, there is a lack discussion on how this could affect the analysis. There are only 62 high PD samples in total (table 1), meaning that features visible for the low PD data might not be captured for high PD data (in figure 6 for instance). How does this affect the credibility of the conclusions made in this study?

We admit that the original threshold for "high-PD" scene resulted an imbalanced sample pool size compared with "mid-PD" and "low-PD" scenarios, especially when collocation with CloudSat is further required. This caveat is now explicitly discussed in Section 2.3, which reads as "Admittedly the sample size is strongly imbalanced between high-PD and low-PD scenes, as this is a trade-off between distinct disparities and statistical significance. Differences presented in Section 3 have passed the 95% statistical significance level unless otherwise noticed. But discussions in Section 4 are largely qualitative only".

Specific comments:

1. L132: Could you please provide some motivation or background for the regime limits, especially the 150 K limit for deep convection.

This question was also raised by RC2. Here is our response:

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.

2. L198: "Because the KuPR reflectivity does not saturate with particle size as rapidly as Cloudsat, we can also infer large ice particles high in the atmosphere in the deep convective and low PD cases...". Do you mean that the limited sensitivity of KuPR to smaller ice hydrometeors indicates that high altitude KuPR reflectivities are due to large particles? Perhaps this could be phrased better.

Yes. This is what I meant. Now the sentence has been re-phrased as "Because the KuPR reflectivity is barely sensitive to cloud ice-sized particles, we can. . .".

3. L209: Could you perhaps use some other word than regime? Regime is already used (high-PD, deep convective, etc), so this caused some confusion for me. Perhaps

"mode" is better?

Changed to "mode". Thank you for this suggestion.

4. L215: Please rephrase the sentence. Suggestion: "The latter scenario indicates the late stage of a convection life cycle, where the convective cell disappears and a stable stratiform layer forms to dominates the whole column."

Rephrased according to your suggestion. Thanks.

5. L262: What types of observations are referred to here? Reference?

Houze [2004] is a very nice review paper of MCSs. It cited a work by Bartels and Maddox [1991] where they collocated satellite IR observations with ground radiosonde observations to study the impact from low-level shear to the initiation and growth of MCSs. It also cited Kingsmill and Houze [1999] of TOGA CORE aircraft measurements of wind and MCSs.

6. L283: Should not the unit of the reflectivity ratio be unitless (or dBZ)? Confusing.

dBZ is decibel of relative to reflectivity, which is unitless. For example, see https://en.wikipedia.org/wiki/DBZ$_{(meteorology)}$

7. L294: I think it is prudent to also refer to Toyoshima (2015), for the DPR thresholds (Yin et al. (2017) added the CPR threshold).

Thank you. Citation added.

8. L294: Forgive me if I've missed this, but I can't find any description on the graupel spheroid in Leinonen and Szyrmer (2015). I suggest you provide at least a short description of both the habits and also on how the riming is modelled. I also find it contradictory to talk about a graupel spheroid under no-riming conditions, since graupel implies growth by riming.

Section 2.4 has been added to summarize the two RTM simulation set-ups, including

PSD assumptions and particle model.

9. L296: Why is repositioning of the theoretical curves necessary? To augment the analysis? Please explain.

Because all theoretical curves always start from [0, 0] as naturally all DFR disappears if there's no water mass (equivalent LWP = 0). However, the origin from satellite triple-frequency diagram is centered at [5, 0], which is consistent with Yin et al. [2017]. Both of us believe this is likely due to the imperfect match between CloudSat and DPR. Therefore, theoretical curves are moved to [5, 0].

10. L339: How is density defined in this context?

Now section 2.4 has been added to summarize the two radiative transfer simulations. The second one used for generating isolines of Fig. 7 is a theoretical calculation of DFR as a function density, where snow, rain and mixed-phase particles were all assumed to be spherical, so density is indeed as simple as particle density.

11. L344: Sentence is very long and difficult to read.

Now this sentence has been broken apart into several short sentences. Hopefully now it's easier to read. Thanks for point that out. It's funny to re-read this sentence and realize how long it is.

12. L354: "evidences" -> "evidence". However, given the limitations (including sample size), I wonder if it would not be more prudent to use "indications" instead. I don't think this would take away from the novelty and importance of this work.

Thanks. Changed to "indications".

13. Section 6: I'm of the opinion that this section could better summarize the key conclusions and future work, in a more concise way. As it is now, it is rather a summary of what was presented and discussed in the paper. What are the take-home messages of this article?

This is not a short paper. Therefore in consideration of readers that do not have time to go through the paper, section 6 is a comprehensive summary of the key methods, outcomes and implications.

14. Figure 3: Could this figure include standard deviations (in dashed lines for instance)?

For simplicity and not overwhelm the lines with overlapping shades (as some lines are really close to each other), we intend to keep Fig. 3 as is. Thanks for your understanding.

15. Figure 5: I found it surprising that high PD signals are prevalent even in the presence of high wind speeds. Shouldn't high wind speeds promote random orientation? Just a comment.

Our interpretation is based on the conceptual picture of formation and boosting of MCS systems, where low-level wind shear was believed to really help the formation and fast development of MCS systems. Please see the last paragraph of Section 3.2 for our interpretations and references. Because ECMWF-AUX is interpolated from 0.5 X 0.5 degree, 3-hourly forecast data, there is no capability of resolving circulations within the cumulus convection, which is largely unresolved and parameterized. Therefore, wind contrasts shown in Fig. 5 are more large-scale or meso-scale. For the high-level shear such like Fig. 5a, we really have no clear clue or interpretation at this moment.

Grammar/technical comments:

1. L14: "ambient environment" -> "the ambient environment"

Added. Thanks!

2. L15: "...impact up to the future climate projection and down to the details of the surface precipitation". Needs rephrasing. Suggestion: "...have on impact on climate projections as well as on the details of surface precipitation".

Suggestion adopted. Thanks.

3. L18: Remove “are”.

Suggestion adopted. Thanks.

4. L43: “cloud” -> “clouds”

Suggestion adopted. Thanks.

5. L55: “tornado” -> “tornados”

Suggestion adopted. Thanks.

6. L72: Sentence needs rephrasing. Suggestion: “With swath widths typically over 1000 km and footprint sizes of 7-15 km, their combined usage can readily generate ice hydrometer production temporal and spatial scales that suits the needs of both weather and climate studies.”

Suggestion adopted. Thanks.

7. L77: Problematic sentence. Suggestion: “While some of the recent products have advanced from using spherical ice models to more realistic habits, random orientation is still nearly always assumed, as it reduces computational complexity and decreases the degree of freedom for the otherwise severely under-constrained inversion problem.”

This sentence has been rewritten according to RC1’s suggestion. It reads now as “While some of the recent products have advanced from using spherical ice models to more realistic habits [e.g., MODIS collection 6 assumed a bulk column-aggregate shape globally for its ice cloud properties retrieval, Platnick et al., 2017], random orientation is still nearly always assumed to avoid the complexity of deriving size and orientation simultaneously, as well as to avoid solving equations for 4 Stokes parameters simultaneously.”

8. L81: “putting” -> “put”

Error corrected. Thanks.

9. L162: Sentence difficult to interpret. Should it be: “For example, Yin et al. [2017] used collocated CPR-DPR reflectivity profiles from this dataset to study the discrepancies found in triple frequency radar signatures and inferred different microphysical processes in convective and stratiform regimes.”?

Suggestion adopted. Thanks.

10. L245: Sentence difficult to difficult. Please rephrase.

The situations are discussed under “in-cloud” and “ambient” conditions. Now it’s rephrased as “Inside cloud, this feature . . .”

11. L253: It is difficult to understand what “which” refers to in the previous clause. Please rephrase for more clarity.

“Which” has been replaced with “Both of them”.

12. L297: “property” → “properties”

Suggestion adopted. Thanks.

13. L303: Try use a more formal word than “blob”. Perhaps “accumulation”?

Changed to “enhancement”.

14. L356: “signal” -> “signals”

Suggestion adopted. Thanks.

15. L365: Problematic sentence. Suggestion: “It is well established that anvil clouds are likely associated with low-PD signals, while high-PD signals are instead linked to stratiform layers.”

Suggestion adopted. Thanks.

16. L400: I think fig. 10 is introduced a bit suddenly here.

"The other way to evaluate the $PR_sfc - PD$ relationship is to composite the statistics." has been included to make the transition smoother.

17. L415: Sentence is difficult to understand. Please rephrase.

For squall line, "other precipitation flag" usually occur at the peripheries of the system. An example is given in the squall line cases below (from our ongoing work). Yellow is convective, light blue is stratiform, dark red is "other", and dark blue is no-precip. Although we think "other precipitation flag" should likely come from anvils, we need to make sure we are comparing trailing edge and leading edge to be consistent with the rest of this section. Therefore, an additional condition is added to exclude those "other" pixels that are at the trailing edge.

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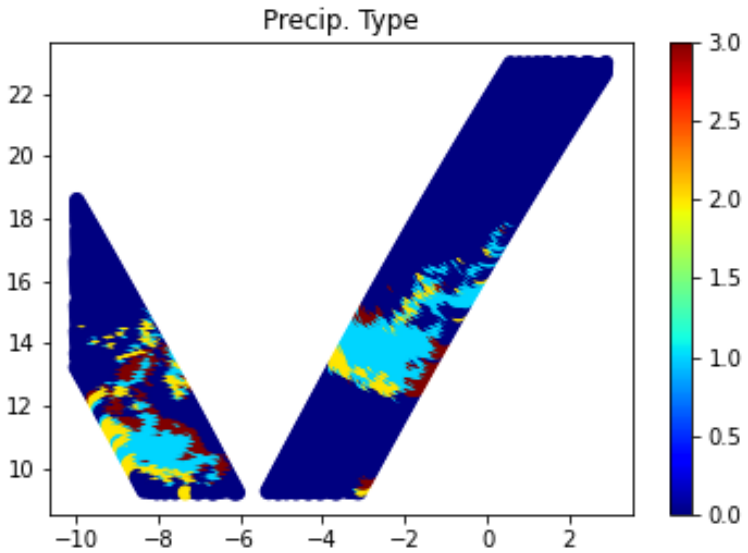

Fig. 1.