We appreciate the reviewers' constructive comments. We have revised our manuscript accordingly, and we hope the reviewer will find the revisions satisfactory.

Review of "An evaluation of microphysics in a numerical model using Doppler velocity measured by ground-based radar for application to the EarthCARE satellite" by Roh et al., submitted to Atmospheric Measurement Techniques (AMT)

[Article#: amt-2023-1997]

To Reviewer 1:

SUMMARY AND OVERALL ASSESSMENT

The manuscript deals with several aspects of how new EarthCare Doppler velocity (hereafter DV) measurements might address problems of hydrometeor identification using a variety of observations and simulations.

The revisions to the manuscript improved both the grammatical usage and presentation and methodology.

MAJOR COMMENTS

1. I appreciate that you added some context to specific values associated with the hydrometeor classification scheme chosen.

You state in your response: "This study does not aim for an accurate classification of hydrometeors but rather for a quantitative intercomparison of models on the same basis," and also "We think the accuracy of the classification's names is not very important in this study."

This, and the new text you added, suggests that the thesis of the manuscript is that the Doppler velocity vs. height space that you use throughout the paper (I will call this DV-H space) is useful for comparing model vs. model and model vs. observation. I agree with that assessment.

But the fact remains that you do break down this space into classes, and then use those classes to interpret the microphysics. For example, in Line 222 "The fraction of CW/drizzle is underestimated in both simulations". Strictly speaking, I can only accept that statement if I also accept that your CW/drizzle classification is correct. (In this case, I believe it's probably pretty good).

It is not unreasonable to ask whether these hydrometeor classifications, at least in the models, actually populate the regions of the DV-H diagram that you assert in the classification scheme imposed. You provide some secondary evidence suggestion they do, for example in Figure 5 where you show the separation between different hydrometeor types. One way to demonstrate this would be to produce a version of one of the panels of Figure 6 (for example) where only one hydrometeor class at a time is shown.

→ The two microphysics schemes do not have a class called drizzle, but we think that drizzle could be understood as rain with a small size. A notable issue arises with the NSW6 scheme, which struggles to accurately depict small-sized rain, often classified as drizzle in the D-H observations. Despite the absence of a direct 'drizzle' classification in the model, recognizing this size-based categorization is crucial for understanding raindrop size distribution. This distribution, along with the terminal velocity of raindrops, significantly influences the evaporation process within the boundary layer. In our future research, we aim to conduct long-term simulations to compare models that effectively represent drizzle against those that do not, to further investigate these dynamics.

2. You should address how the integration time affects the implications of this analysis when your method is used on real EarthCare data. You show results collected over a geographic region during a 24 hour period. EarthCARE will have flown a over these storms in seconds, with a very small footprint, and likely not sampled them again.

→ Thank you for your comment. When we use satellite data with active sensors, we need to collect data for a more extended period (more than one month) and a large domain for statistical analysis. We have experience in the evaluation using satellite data like TRMM or CloudSat (Roh et al., 2014; Roh et al., 2017). When we used satellite data, we collected the data for more than one month and a large domain for statistical analysis. According to our experience, the characteristics of simulation data like CFADs were controlled by the microphysics schemes rather than the integration time.

MINOR COMMENTS

- Figure 7 was a welcome addition to show the overall small effects of vertical air motion. It's much more compelling than Figure 4, which unsurprisingly shows that velocities on the order of cm/s are much more common than velocities of m/s. The problem there is that certain interesting phenomena (heavy rain, buoyant updrafts, etc.) are crammed into that 0.2% that have absolute vertical velocities above 0.2 m/s. Also your analysis is presumably affected by the ~km scale width of a grid box.

→ Thank you for your comment.

- HG-SPIDER is indeed polarimetric, which could help with hydrometeor classification in the observations. However I accept this might be difficult to do in practice.

→ I am sorry for my mistake. I misunderstood the coauthor’s comment. The HG-SPIDER was developed as a polarimetric radar. HG-SPIDER did not observe the depolarization ratio in these cases.

- Line 89 suggests that the Case 1 integration time is 24 hours, but Line 101 suggests it is 12 hours. Please clarify.

→ The integration time is 24 hours for simulations, but observation data is only available for 12 hours because of the instrument issues.

- Line 202: Doesn't NDW6 have the clearer separation? Or am I misinterpreting overlapping lines?

→ NDW6 shows the overlapping terminal velocity with 2m/s between graupel and large cloud ice or snow.

- Line 230: "We checked the impact"...

→ We changed it based on your comment.

- Line 234: Convection is often its own plural in this context (without the 's')

→ We changed it based on your comment.

- Line 253: "We focused the data"; perhaps replace with "Therefore we analyzed data"

→ We changed it based on your comment.

- Line 270: "The Joint-Simulator can simulate the EarthCARE CPR", removing "the signals like"

→ We changed it based on your comment.

- Line 338: "In this study we developed a methodology"...

→ We changed it based on your comment.

References

Roh, W. and Satoh, M.: Evaluation of precipitating hydrometeor parameterizations in a single-moment bulk microphysics scheme for deep convective systems over the tropical central Pacific, J. Atmos. Sci., 71, 2654–2673, https://doi.org/10.1175/JAS-D-13-0252.1, 2014.

Roh, W., Satoh, M., Nasuno, T.: Improvement of a cloud microphysics scheme for a global nonhydrostatic model using TRMM and a satellite simulator, J. Atmos. Sci., 74, 167–184, https://doi.org/10.1175/JAS-D-16-0027.1, 2017.

To Reviewer 2:

The revision improved the presentation of the work and made it easier to read. However, it is still recommended that the authors continue to improve some confusion parts in the manuscript and its overall readability before it is accepted.

The model information of the terminal velocity for different hydrometeor categories is only used to justify the thresholds that are used to set categories in the observations and the simulated Doppler velocity. Although the terminal velocity information could be better used in the analysis, due to the scope of this study, it is ok.

→ Thank you very much for your feedback. Based on the comments received from both the editor and the reviewer, I have made further refinements to the manuscript to enhance its clarity and readability. I appreciate the suggestion to utilize the terminal velocity information in our analysis more effectively. While the current scope of our study primarily leverages this data to justify observation and simulation categorizations, I acknowledge the potential for a deeper exploration of this aspect in future work. We remain committed to addressing any remaining areas of confusion and improving the manuscript's overall coherence in preparation for acceptance

Woosub Roh
Atmosphere and Ocean Research Institute, The University of Tokyo
5-1-5, Kashiwanoha, Kashiwa-shi, Chiba, Japan
Phone No: 81- 04-7136-4371
Fax No: 81- 04-7136-4375
Email Address: ws-roh@aori.u-tokyo.ac.jp