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Interactive comment on "A ubiquitous ice size bias in simulations of tropical deep convection" *by* McKenna W. Stanford et al.

Anonymous Referee #1

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This manuscript compares ice-phase particle sizes simulated by the WRF model with aircraft measurements during the recent HAIC-HIWC field campaign. The three different microphysical schemes used in WRF all show larger mass median diameters when compared with the observations. Many previous studies have demonstrated the same bias. However, uncertainties exist as to what extent the bias is caused by model errors in dynamics (e.g., overestimation of the updraft velocity), bulk microphysical schemes' assumptions in particle size distributions, and the uncertainties or misrepresentation of microphysical processes. The unique aspect of the HAIC-HIWC field campaign is that it simultaneously measures particle sizes and vertical air velocity. The manuscript compares the observations and model simulations by stratifying MMD and TWC with w, in an effort to separate errors in dynamics from microphysics. Uncertainties in bulk microphysical schemes' PSD assumptions are also investigated by comparing two dif-

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Discussion paper

ferent bulk microphysical schemes and an explicit bin microphysical scheme, which does not have any PSD assumption. This study is scientifically sound and utilizes data from a new field campaign. However, the presentation quality is very poor. So much so that two of the figures (Fig. 4 and Fig. 9) are missing from the manuscript. Fig. 3 is duplicated in Fig. 4, and Fig. 8 is duplicated in Fig. 9. This is unacceptable for any publication.

Specific comments:

In addition to the 2 missing figures, some figures will need to be reorganized/combined in order to facilitate comparisons and discussions. Some of the descriptions are difficult to understand, as listed below. I hope the authors will consider rewrite them to improve readability.

1. The manuscript compared observed TWC and MMD stratified by w and T/height. Ostensibly missing is the comparisons of the total number concentration. The total number concentration should be the most accurate observation and is predicted in all 3 microphysical schemes. Comparison of Ntotal stratified with w and T and/or TWC will provide more insights in model errors. In a generalized microphysical framework, one-moment schemes solve mass equation (first order of mass); two-moment schemes in general solve both mass and number concentration (zeroth and first order of mass). There are also three-moment schemes which solve the zeroth, first and another higher order variable, usually the second moment of mass. Comparisons of the simulated total numbers with observations is essential for all microphysical scheme validations. It can also be carried out in high confidence especially when using in-situ data. I suggest that the authors add N_total with respect to both w and TWC in Fig. 6. And add corresponding N_total in model simulation plots.

2. Fig. 7, 8 and 9 can be combined to a 3x3 panel figure for easier comparison and discussion.

3. The color scheme in Fig. 7-9 is also confusing. The convention is that blue has

smaller value than red. I was initially confused by the fact that red represents negative and blue positive. I'd suggest the authors to flip the color scheme in these figures.

4. What is the rationale of using only data with w>1m/s? Can data with w<1m/s be used for model comparisons, too? Will it lead to the same conclusion?

5. The biggest problem with the observation is its sampling bias. Due to safety concerns, the airplane must avoid lightning and area with radar reflectivity above 40 dBZ. The authors mentioned this error, but didn't do anything to quantify it. Another problem is that all data from different events are used indiscriminately to compare with a single case simulation. The author should add some analyses to address these uncertainties. For example, Fig. 1 seems to show that on Feb. 18, the airplane sampling might be on the weaker part of the system. This case also seems to have the warmest Tb among the four cases shown. Is this true? Does samplings with high w mainly come from other cases? One way of determine the bias is by plotting sampling sizes for Feb. 18 case only, and compare the result with the same plot using all samples, on a T-w and/or T-TWC plot. Another possibility to address the sampling bias is to combine the 3D C-Pol data with Feb. 18 sampling. For example, one could plot C-POL sampling sizes in T-dBZ space. Then use observed PSD to calculate dBZ for in-situ measurements, and plot sample sizes on a T-dBZ diagram, too. This could roughly show how much/what type of biases existed in airplane sampling.

6. The comparisons are made for model output between 18Z on the 18th to 00Z on the 19th, because this time period was considered to represents mature and dissipating stage of the MCS, according to the manuscript. However, Fig. 5 compares reflectivity at 16Z, which is outside the window for PSD comparisons. I suggest the authors to plot C-Pol radar reflectivity CFADs (Yuter, 1995, Mon. Wea. Rev., Vol123, P1941-1963) for the same 6-hour period. Then compare it with CFADs of model simulations.

Technical Corrections:

1. Please put correct Fig. 4 and Fig. 9.

2. P1, L24: ". . . ., differences with observations for a given particle size vary greatly between schemes." I don't understand this statement. Please rephrase.

3. P4, L12: "Uncertainty in w calculations is estimated at ∼1ms-1". Please give reference.

4. P5, first paragraph: Can the authors list the values of alpha and beta derived from the observations. They can be used to compare with model parameters, and are essential for reproducing the results (e.g., deriving MMD) shown in this study.

5. P5, L5: Deq is defined here as "area equivalent diameter", but later in eqn. 5 is used as "melted equivalent diameter". This is confusing.

6. P5, L27: ACCESS-R is used as the initial and boundary condition, not as "large-scale forcing", according to the conventional term usage.

7. P7 L2, where is the citation for CCN concentration? How high is the boundary layer?

8. P7, L9, "(not shown)" Can the authors show it, perhaps in the supplemental material. This is import if we want to use one case to represent all simulations.

9. P7, L15: The last sentence needs to be broken into two. The sentence has two unrelated issues about inner domain and total simulation time, if I understood it correctly.

10. P10, last paragraph: The discussion of Fig. 10 needs clarification. I guess the first question I have is: why use 90%? Would 50% do? Why or why not? Also, the descriptions are hard to comprehend. Please make an effort to clarify it.

11. P13, L30: "The majority of graupel at T>8C is formed by freezing raindrops". Can you give references and/or supporting evidences? My understanding is that this is only true when updraft velocity is high. Otherwise riming could be the dominant process.

12. P15, L1: "distribution tails" could mean tails at both small and large size end. May be change it to "large size tails"?

13. Fig. 11 to 16: Can the x-axis be extended beyond 25 m/s to include higher w simulated in the model? A line can be added to indicate the observation range of 25m/s. This will give a full picture and help the readers better understand model differences and their related processes discussed in the paper.

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