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Atmospheric Chemistry and Physics Discussions

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

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We thank the reviewer for their thoughtful comments. We feel that they have improved the manuscript, and we hope that our revisions satisfy any concerns. Please note that any references to page or line numbers in our "changes to manuscript" responses refer to the final revised version of the manuscript and reference to any figure numbers in our "changes to manuscript" responses refer to revised figure numbers. Figures included for responses below are referenced as Fig. R1-R5. Note that although all 5 response figures (R1-R5) may not be referenced in this particular response, they are discussed in responses to reviews by both ACP referees. All 5 response figures are included in response to both anonymous referees for consistency of discussion. Figures added to supplemental material are referenced as Figures S1-S4.

Specific Comments:

1. Comment: 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 N_total 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.

Response: We agree that total number concentrations are certainly important for microphysics scheme evaluation, however they were not analyzed in detail for two reasons. First, comparisons are really only useful when liquid and ice are separated because liquid dominates the total number concentration when present (e.g., a typical cloud droplet concentration is 100,000 L-1 whereas a high ice concentration is 100 L-1). The issue is that liquid and ice number concentrations are not separated in measurements. Additionally, liquid in the form of small cloud droplets are common in mixed phase updrafts of the bulk schemes (see Figure 14), and will dominate the number concentration in these regions. Apart from this simulated liquid likely often being erroneous (e.g., from not maintaining liquid supersaturations in the bulk schemes), the cloud droplet number concentration in the bulk schemes is held constant and there are no constraints on the CCN PSD in FSBM other than that this event occurs in a clean, tropical maritime air mass. Therefore, liquid needs to be removed from any number concentration comparisons, which is easy in simulations, but difficult in measurements.

Second, the measured total number concentrations have significant uncertainty. Figure R1, provided by coauthor Alfons Schwarzenboeck, shows differences in three 1-minute composite PSDs (resulting from 5-second samples) using derivation techniques from 3 different institutions. Consistent differences in number concentrations can reach an order of magnitude for sizes below $\sim$150 μm (note the logarithmic ordinate scaling). These small particles control the total number concentration, but quantifying the observational number concentration uncertainty is a nontrivial task, and doing so is beyond the scope of this study. The large differences in PSDs shown in Figure R1 partly result from different techniques used to filter out shattered particles. TWC and MMD measurement have greater certainty than number concentration since the IKP2 measures TWC plus water vapor, where the subtraction of water vapor is the primary source of uncertainty, while MMD is not strongly impacted by differences in particle numbers at diameters smaller than 150 μm. Essentially, mass as a higher moment of the PSD than number concentration is much less impacted by small particles that are the basis for uncertainty. For example, Figure 7 of Leroy et al. (2017) shows that the mean 15% MD as a function of TWC for any given flight is never smaller than 100 μm.

Changes to manuscript: We have added language on P5 L16-19 in the manuscript to state why total number concentrations are neglected.

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

Response: We agree that combining these 3 figures into one figure facilitates easier discussion.

Changes to Manuscript: Former Figures 7, 8, and 9 have been combined into a 3x3 panel (current Figure 7)

3. Comment: 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.

Response: We agree that the convention is to use red for positive values and blue for negative values.

Changes to Manuscript: All relative difference plots now use red for positive values and blue for negative values (see current Figures 7, 8, S2, S3, and S4).

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

Response: The original reasoning for using only updrafts was that high IWC regions targeted during the HAIC-HIWC field campaign are typically found in updrafts or regions connected to nearby convective cores, and are hypothesized to form within updrafts and regions detrained out of convective updrafts (see Lawson et al., 1998). However, we agree that this study would be more conclusive by including downdrafts and quiescent regions. This also provides a more statistically robust observational sample size for many significant TWC values (total sample size has increased approximately 8-fold, see Figure 6). An additional constraint was added (see response to Minor Point #2 by Reviewer 2) by which only simulated and observed points with TWC > 0.1 g m-3 are included. This value is similar to the expected uncertainty in measured TWC at -40°C (see discussion on P4 L28-33).

Changes to Manuscript: The dataset now includes all vertical velocity values with only the following constraints: (1) -60 °C < T < 0 °C and (2) TWC > 0.1 g m-3.

5. Comment: 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.

Response: There are several difficulties in quantifying the observational sampling bias. Although Fig. 1 shows that the aircraft sampled warmer brightness temperatures in the Feb. 18 system compared to others, this was necessary to observe convective cores within range of the C-POL radar, cores that still had cloud tops reaching up to 14-km altitude as observed by the onboard RASTA W-band radar. It is possible to compare C-POL reflectivity within the flight path to all C-POL reflectivities over the limited range of temperatures observed during this flight, but the most intense convection during this case was before the flight. During the flight, no cells were avoided within the region of interest (the C-POL domain). Therefore, the flight during this event is not representative of avoidance of high reflectivity regions or lightning, and trustworthy Rayleigh reflectivity was not observed for other cases. Additionally, using full 3-D C-POL reflectivity assumes that the aircraft would observe all regions equally, but this is not true, since the aircraft was specifically targeting regions thought to possibly have high IWC subjectively based on both pilot's radar data and satellite data. Therefore, there is no objective way to quantify this sampling bias in terms of reflectivity or any other variable. The impact of this bias is limited to the extent possible by controlling for TWC and vertical velocity, and is also the reason that the minimum 90% MD is examined, since it is not impacted by this sampling bias.

Additionally, Flight 23 on Feb. 18 is not representative of other flights. Attached is a

PDF (Figure R2) showing the contribution of each flight to the dataset used for this study. The 4 simulated events are indicated. On the right ordinate, Figure R2 shows the mean and standard deviation of temperature. Note that although a single flight may have sampled multiple temperature levels, each discrete flight leg flew at an approximately constant temperature level. This PDF signifies the need to use observations from the entire field campaign in order to properly stratify variables by temperature like the data shown in Figure 6. The Feb. 18 case (Flight 23) contains only ~7-8% of the total samples, the majority of which remain between -10 and -20 °C. This PDF also show that using data from only the Feb. 18 event would not yield information on relatively colder temperatures, where most observations exist.

Figures S2-S4 were added in the supplemental material to address the concern of comparing one simulated event with observations from all field campaign events. These figures are similar to Figure 7, but show relative differences in observed and simulated TWC and MMD as a function of w-T and/or TWC-T bins for 3 additional events (Jan. 23, Feb. 2-3, and Feb. 7 events in Figures S2-S4, respectively) simulated with the bulk schemes. Note that FSBM was only run for the Feb. 18 simulation due to its high computational expense. These supplemental figures show that when controlling for temperature, TWC, and/or w, every simulated event yields very similar differences with observations in MMD-w-T-TWC space (where observations are taken from all flights), particularly in the case of MMDs. While there is certainly variability in the peak vertical velocities of simulated events, we control for vertical velocity, and thus this variability does not impact our results.

We note that the intention of focusing on the Feb. 18 event simulations is that radar (C-POL) observations were available for this event alone, and it contains most of the high TWC and high w observations at relatively warm temperatures. Figures S2-S4 clearly show that simulating a single event with different microphysics schemes yields much larger differences than simulating different events with the same microphysics scheme, at least in the phase space considered in this study.

Changes to Manuscript: Figure R2 has been added to the supplemental material as Figure S1 for extra justification of our comparison methodology. Figure S1 is discussed on P4 L7-8 and P7 L19-22. Figures S2-S4 have been added to the supplemental material to show that using a single simulated event is sufficient for comparison with all Darwin observations in the MMD-w-T-TWC phase space considered in this study. A short discussion of Figures S2-S4 is now provided on P11 L32 – P12 L3.

6. Comment: 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.

Response: Figure 5 has been revised such that it now shows only the 99th percentile to account for strictly convective radar reflectivities since there is a significant amount of stratiform precipitation across the domain (see Figures 3 and 4). Figure 5a shows the 99th percentile for a time period between 12Z and 18Z on the 18th and Figure 5c shows the 99th percentile profile between 18Z on the 18th and 00Z on the 19th. Figures 5b and 5d show sample sizes normalized by domain area (C-POL domain is smaller than the model domain) for the 12Z-18Z and 18Z-00Z time periods, respectively. These two time periods are shown because the most intense convection observed by C-POL was between 12Z-18Z, but the flight took place between 18Z-00Z. Although the most intense convection was not in the C-POL domain during the 18Z-00Z analysis period, it is clear that all simulations produce significantly higher reflectivities aloft compared to C-POL, no matter the time period selected for comparison. 99th percentile profiles and domain-normalized sample sizes are still used as opposed to CFADs. Using percentile profiles on one figure allows quantification of differences between observations and simulations that is not possible in CFADs. Moreover, the high IWC regions targeted by the campaign aircraft are generally found in and around convective cores, and regions of modest stratiform reflectivity that would dominate CFADs were not of interest. The large simulated reflectivity biases aloft that are being explored in this study are commonly associated with convective regions. This is the bias being investigated with the in situ microphysical and kinematic datasets, and thus comparison of high percentile profiles (e.g. 99th) is more appropriate than CFADs.

Changes to Manuscript: Importantly, we note that we have used an updated C-POL dataset provided by Alain Protat who has been added as a co-author. We have also gridded these C-POL radial sweep files ourselves to match the simulation horizontal grid spacing (1 km) and found a ~2.5 dB increase, which is why the cross-section figures (Figures 3 and 4) change from the original version, however this increase is not central to any of our conclusions. Figure 5 has been revised as discussed in the response above. Discussion of Figure 5 has been revised on P10 L4-15.

Technical Corrections:

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

Changes to manuscript: The correct version of Figure 4 is now in place. The correct version of former Figure 9 is now represented in the bottom panel of Figure 7 (g-i), as recommended in Specific Comment #2.

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

Changes to manuscript: Clarified this statement on P1 L22-24.

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

Changes to manuscript: We added a reference to Jorgensen and LeMone (1989) on P4 L13.

4. Comment: 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.

Response: Observed values of α and β are not constant in the observational dataset, as explained on P5 L4-6 and in much more detail in Leroy et al. (2016). For this reason, the > 16,000 m-D coefficient pairs used in the current study are not listed. The β parameter changes during flight since it is related to the exponents of area-size and perimeter-size power laws that are derived directly from OAP images for every 5-second flight sample. The α parameter varies during flight as it is constrained by independent TWC retrievals. Additionally, simulated α and β parameters that are comparable to observations are not possible since observations include all hydrometeor types in derivation of the power law, while simulations have multiple hydrometeor species, each with different α and β parameters, and combining these species to produce the mass size distribution means that the simulated mass-size relationship no longer follows a power law.

Changes to manuscript: Due to possible confusion, the sentence on P16 L20-23 has been modified to indicate that each 5-second flight sample uses a single m-D power law (not the entire dataset using the same power law/coefficients), and that the assumption that the distribution of mass with diameter is well represented by a power law may not be a good assumption in some situations. However, analyzing composite distributions may alleviate this issue to some degree.

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

Changes to manuscript: All simulated Deq values are now referenced as Deq,melt in the computation of reflectivity where equivalent melted diameter is used, whereas Deq is now explicitly used to refer to the 2D area equivalent ice diameter, defined as the diameter of a circle with the same area as particle images from the OAPs.

6. Comment: P5, L27: ACCESS-R is used as the initial and boundary condition, not as “largescale forcing”, according to the conventional term usage.

Changes to manuscript: Changed “large-scale forcing” to “initial and boundary conditions” on P6 L2-3.

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

Response: Perhaps there was some confusion in the values listed that give a vertical representation of the initial aerosol concentration profile in the FSBM scheme. These values are the default values used by FSBM to represent a maritime (pristine) air mass (set using FCCNR_MAR in module_mp_fast_sbm.F, the WRF microphysics module for the FSBM scheme). The text has been changed to make it clear that these values are not observed values, since there are none near Darwin for this field campaign, but rather the default values used by FSBM for a maritime air mass.

Changes to manuscript: We have changed “in the boundary layer” to “near the surface” on P7 L8-10 to note that this initial condition of CCN does not depend on boundary layer height.

8. Comment: 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.

Changes to manuscript: Added Figures S2-S4 in supplemental material showing differences in TWC and MMD as a function of w, TWC, and T for the other simulated events using the Morrison and Thompson scheme. See response to Major Point #5 for more information.

9. Comment: 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.

Changes to manuscript: The referenced sentence was broken into 2 and moved to the middle of the paragraph on P7 L22-24.

10. Comment: 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.

Response: We have attempted to clarify the meaning and utility of the minimum 90% MD in the current study. The mass placed in the upper percentiles of the mass-size distribution (e.g. 90%) is typically from larger particles that may have been less frequently sampled by the aircraft due to the aforementioned sampling bias. However, the minimum 90% MD observed should not be impacted by this bias since it will be associated with a lack of large, dense particles and a lack of relatively high reflectivity or lightning. Therefore, if the simulation is unable to produce a single 90% MD that is as small as the observed minimum 90% MD for a given temperature, TWC, and vertical velocity, then we feel confident in declaring a model bias. This is particularly true because the simulations produce on average ~10ˆ2 more samples for a given TWC-T or w-T bin compared to observations. So, despite 2 orders of magnitude larger sample sizes in a simulation, the simulation is unable to produce a single 90% MD as small as the observed minimum 90% MD in many TWC-T and w-T bins.

Changes to manuscript: Clarification of the 90% MD discussion has been made on P12 L10-19.

11. Comment: 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.

Response: You are correct that the dominant process is riming. We should have stated more clearly that the primary contributor to graupel production was the heterogenous freezing of raindrops due to the collision of rain and cloud ice, which is a riming process that involves freezing of lofted raindrops. This was analyzed in the Thompson scheme and not in the FSBM scheme, however the raindrop sized MMDs and large LWCs between 0 and -4 °C in updrafts in Figure 14 indicate that the majority of the LWC is constituted by raindrops. In FSBM, by -8 °C, nearly all of the liquid is gone and most of the IWC is constituted by graupel (compare Figure 13 to Figure 12), which only decreases with decreasing temperature. This suggests that most of the graupel is being formed by raindrops that are freezing, likely heterogeneously through interactions with ice, as in the Thompson scheme. Since raindrops are smaller for T > -8 °C in FSBM, it intuitively makes sense that this contributes to smaller graupel sizes.

Changes to manuscript: Language has been changed on P15 L21-28 to clarify this and suggest this as a possible mechanism responsible for smaller graupel sizes rather than definitively concluding that it is the reason.

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

Changes to manuscript: Added “at larger diameters” after “distribution tails” to clarify on P16 L25.

13. Comment: 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.

Response: The primary purpose of this study is to compare simulations with observations in order to establish potential model biases. Because observed vertical velocities do not exceed 25 m s-1, extending the x-axis would only allow comparison between different microphysics schemes, which is not a first-order objective in this study. The evaluation of differences between schemes in Figures 11-14 is primarily to provide possible explanations for biases with respect to observations. A more in-depth analysis of differences between simulations would be interesting, but we feel that it would extend the paper beyond a reasonable length for a single study.

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-99, 2017.

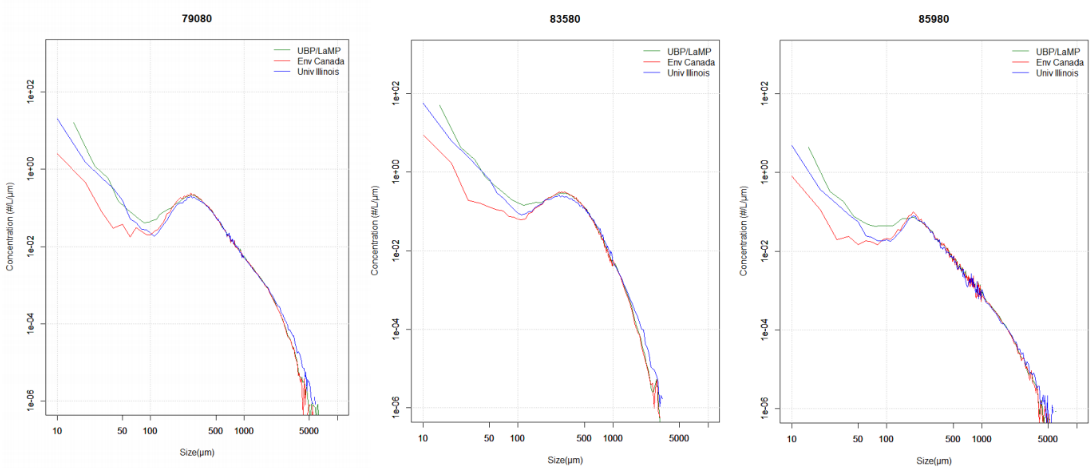

Fig. 1. PSDs from 3 different 1-minute composites (resulting from 5-second samples) showing PSDs derived from 3 different institutions. Number concentration is on the ordinate and diameter is on the abscissa.

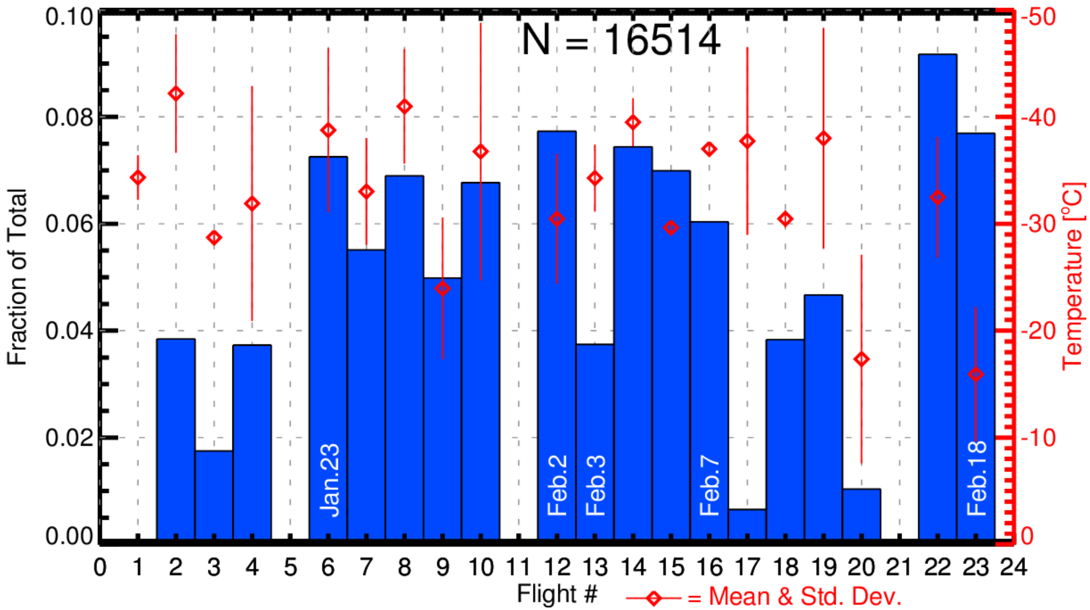

Fig. 2. Distribution of flight samples for the Darwin HAIC-HIWC campaign by flight number (blue bars). Red diamonds show the mean temperature for each flight, with lines that show +/- one standard deviation.

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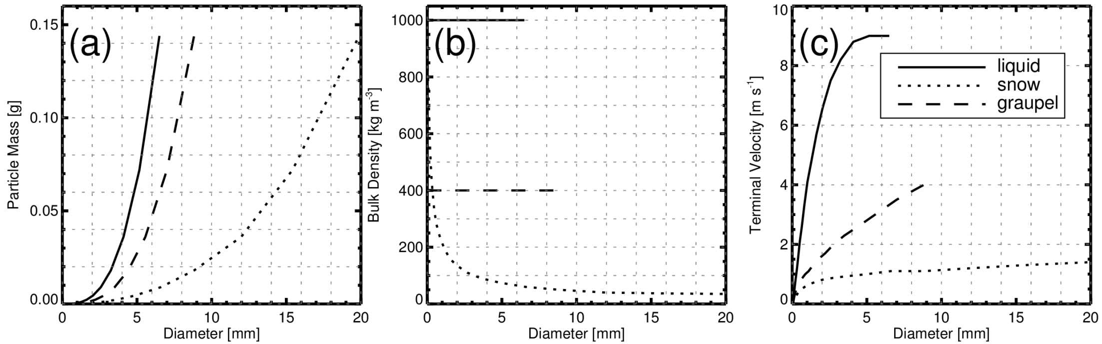

Fig. 3. FSBM (a) mass , (b) density, and (c) terminal velocity as functions of diameter for liquid (solid), snow (dotted), and graupel (dashed).

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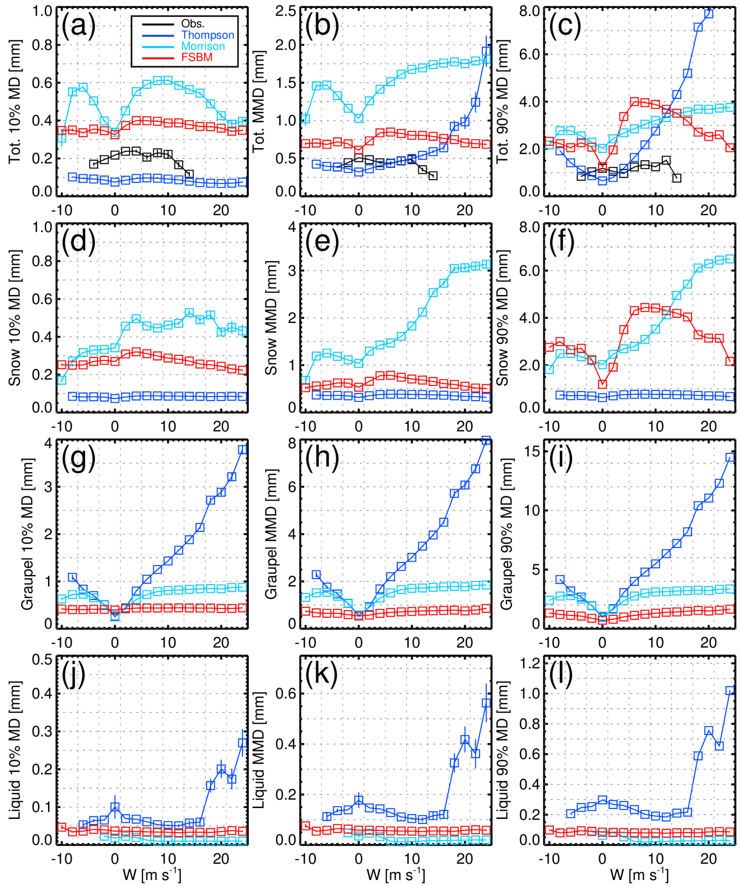

Fig. 4. (a)-(c) Combined hydrometeor 10% MD, MMD, and 90% MD, respectively, as a function of w for T between -32 °C and -40 °C. Snow MDs in (d)-(f), graupel MDs in (g)-(i), and liquid MDs in (j)-(l).

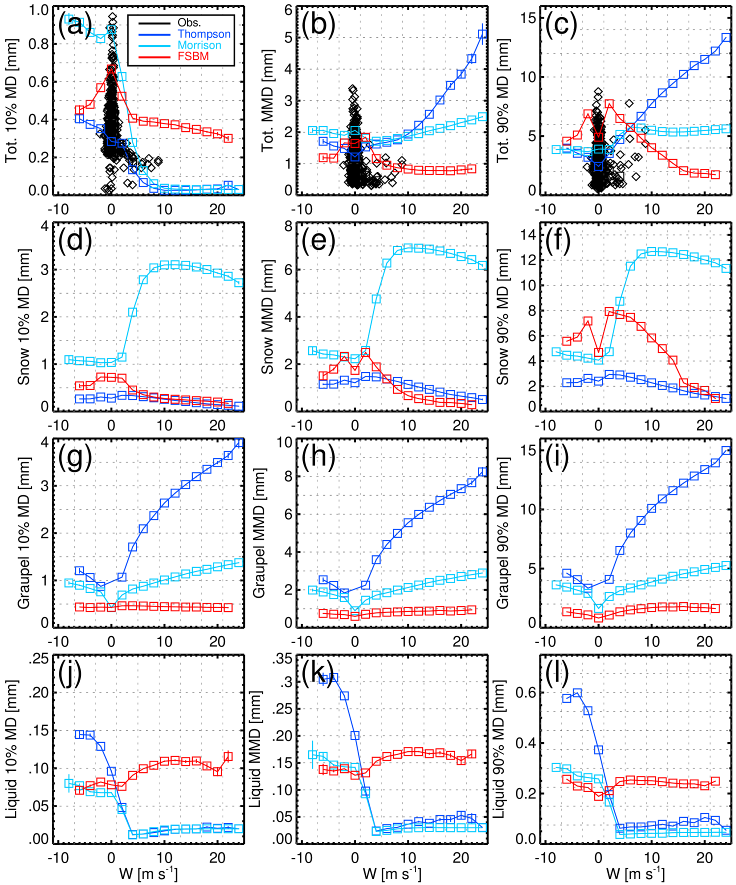

Fig. 5. As in Figure R4, but for a temperature range between -8 °C and -16 °C.

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