

Response to interactive comment on “Use of an observation-based aerosol profile in simulations of a mid-latitude squall line during MC3E: Similarity of stratiform ice microphysics to tropical conditions” by Ann M. Fridlind et al. by Anonymous Referee #2

This study examines and reports aerosol size distribution profiles for six convection case studies observed during the MC3E field campaign, intended for use in model simulation of those cases. The authors demonstrate use of the aerosol size distribution profiles in NU-WRF simulations of the 20 May case study with Morrison twomoment microphysics focusing on examining the stratiform cloud microphysical properties. There are some interesting findings such as ice crystal number concentrations are consistently dominated by a single mode near D_{max} of 400 μm , and a mass mode near D_{max} of 1000 μm becomes dominant with decreasing elevation to the -10 OC. Therefore, the study is worthy being published. However, this reviewer does have some concerns about the current form as listed below,

We very much appreciate the helpful questions and comments. Point-by-point responses below have greatly improved the manuscript by reducing figures, adding section numbers, and making corrections and clarifications throughout.

(1) I am a little confused about the objectives for the second half of the paper that demonstrates the use of the derived aerosol size distribution. The Introduction does not have a clear statement about the goal of this part. Their results show that simulation using the aerosol size distribution derived does not much affect ice microphysics and stratiform microphysical properties including particle size distribution. These results kind of dispute the importance of aerosol size distribution used in model simulations. Logically, to show the importance of the developed product (i.e., aerosol size distribution), the paper should present results that are significantly changed by aerosol size distribution such as precipitation rate, convection, etc. But the authors did not go to this direction and kind of ignored the point about the importance of the derived aerosol size distribution to MCS simulations. This is ok only if the authors clearly state the reasons for doing what they chose to do and the relevant objectives.

Clarification added to Section 5: "If a warm-temperature ice multiplication mechanism is dominating outflow ice distributions in a manner that cannot be generally reproduced in simulations and is not well understood, it is difficult to confidently assess how or to what degree hygroscopic and ice-nucleating aerosols can be expected to modulate outflow ice properties. For instance, in this study we cannot be confident of the relevance of our sensitivity tests for understanding natural convective outflow owing to inadequate baseline fidelity compared with observations."

(2) Section 3 does not have a clear structure. This part is very important to the entire paper, and the authors need to be clear about (a) the methodology of how the aerosol size distributions are derived, (b) the final products provided to the community, and (c) the discussion about caveats and uncertainties. However, the current writing in this section makes readers difficult to get those. The authors are still talking that the methodology in the last 4 paragraphs of this section.

We now use two levels of subsections in Sections 3 and 4. Some additional text is added for clarification.

(3) The contribution of small CCN to droplet nucleation and ice particle concentration at upper-levels needs some further examination. The conclusion is premature. See comment #20.

Our activation treatment does not omit secondary droplet nucleation above cloud base (see response to comment #20 below). We also now clearly state that the value of our sensitivity tests is limited (see response to comment #1 above).

(4) About Section 5, although I enjoyed reading the discussion, much of the discussion should be moved to the Introduction since they are the very relevant literature studies providing the background for this work.

We consider results unexpected based on past literature, and therefore do not present discussion of results before presenting the results themselves. In the introduction we do mention Ackerman et al. (2015) as a motivating factor.

In addition, some of the things discussed here are not even mentioned in the main text or not much related (for example, the lack of the positive differential radar reflectivity and the importance of the tropical convection in global circulation).

Clarification added to Section 5: "Case studies are generally better for model development if they are relatively typical rather than unusual or rare. ... Analyses of dual-polarimetric radar observations could be further systematically employed to identify the environmental conditions associated with stratiform microphysics regimes ..."

Reference to global circulation now refers back to introduction.

(5) There are many inconsistencies between Figure, Figure captions, and the corresponding text, and also a few figure captions do not clearly describe the figures. There are quite a few sentences what do not make sense or are wrongly stated. Please refer to the specific comments below for the details.

Please see responses below and those to referee 1.

(6) Too many figures: some figures can be combined such as Fig 4 and 5, and some are not key to the main points such as Fig. 9-11, and Fig. 16-17, which could be the options for the supplemental materials since there is already a supplemental file.

We combined Figs. 4 and 5 and removed 6, 10–11, and 19–20. We retained Fig. 9 to show one comparison of derived PSD aloft with observations and 15–17 (main focus).

Detailed comments,

1. P1 Line 14-15, not sure what you want to say here, especially about the specific meaning of "the microphysics pathways associated with deep tropical convection outflow".

Reworded for clarification: "Based on several lines of evidence, we speculate that updraft microphysical pathways determining outflow properties in the 20 May case are similar to a tropical regime, likely associated with warm-temperature ice multiplication that is not well understood or well represented in models."

2. P2 Line 2, aerosol should be plural here.

Changed.

3. P2 Line 10-14, this is a very long sentence. Suggest to break into two sentences to make it easier to read.

Done.

4. P2, last paragraph, the last a few sentences of this paragraph need to be revised to clearly state the objectives of this study. If the objective is to achieve more accurate simulations, then is the goal achieved?

With respect to the last four sentences in this paragraph, we achieve the goals stated in the first to third, which respectively begin "Here we" and "We also". The last sentence begins "Enabling accurate simulation" because we intend the derived aerosol PSDs for that purpose. Since the latter is better discussed in Section 5, we removed the last sentence.

5. P3 Line29, aerosol should be plural here.

Changed.

6. P4, Line 5-11: the description here about Figure 3 suggests Na is from DMA or CPC and kappa is from HTDMA. However, the Figure 3 caption said only HTDMA, and no DMA data is shown. Please clarify the inconsistency. In addition, description about instrumental uncertainty for each instrument would be helpful here.

HTDMA now used consistently throughout. Clarification added to Section 3.2: "Based on the discrepancy between ground-based CPC and HTMDA measurements, we estimate that overall uncertainty in derived total aerosol number concentrations is roughly a factor of two throughout this work."

7. P4, Line 15-16, something is missing in the later half of the sentence. Otherwise, it does not make sense.

Latter half simplified to "nucleation mode aerosols were commonly present in large concentrations but were also commonly absent."

8. P4, Line 15-19, the description here would be clearer if the ratios of CCN to CPC aerosol concentrations are shown.

Agreed, but since we only show CCN data for completeness (not used in our fits) and we list values in Fig. 3a, we prefer to briefly state the range of ratios rather than adding another figure panel.

9. P5, Line 8 and Lin 17: what are non-case-study dates and case study dates?

Figure and sentence removed (Section 2 describes case study selection).

10. P5, I do not understand what is said in the sentence "UHSAS/CPC again sometimes decrease, not because UHSAS decreases but because CPC increases, consistent with evidence that the surface is also a source of fine particles". CPC increases suggested more small particles, which could be from particle nucleation at the elevated altitudes. This is observed quite often. So, I do not understand why we can infer that surface is the source.

Sentence clarified: "However, the local minimum in the ratio of UHSAS to CPC seen at the surface is consistent with a surface source also for fine particles (e.g., Wang et al., 2006, their Fig. 7), which could be both spatiotemporally variable and regional in nature (e.g., Crippa et al., 2013)."

11. Figure 7, there are no red and blue lines.

Figure corrected and caption revised also in response to referee 1: "The median of airborne CPC and UHSAS aerosol number concentrations within 1-km-deep layers for each MC3E flight, and the ratio of those median values for the seven flights with both instruments (black lines). The median of profile values at each elevation (red lines) are archived as Supplement 2."

12. Figure 8, why are there two colored solid lines for the measurement from HTDMA? It is really confusing with so many numbers on each panel and the description is not clear for some numbers such as the numbers at the right bottom part of each panel. Strongly suggest to use a table to show the parameters for the three modes. Also, need to explain the purpose of showing the 0 and 8000 cm⁻³ in the nucleation mode for May 20 case.

The black values are archived with Supplement 1 and we disagree that the underlying values deserve a dedicated table. Clarifications added to caption also in response to referee 2: "Aerosol dry number size distributions ($dN_a/d\log D_a$) reported from HTDMA during the two-hour pre-rain period (colored solid lines; legend indicates Julian date in UTC), lognormal fits to HTDMA (colored dashed lines; text indicates fitted number concentrations in cm⁻³, geometric mean dry diameter in μm and standard deviation), and the final case study distribution derived from the mode-wise linear mean of contributing parameters and its hygroscopicity parameter (κ) derived as the number-weighted mean of contributing HTDMA values (black dashed lines and black text; archived with Supplement 1). In the 20 May case, zero and 8000 cm⁻³ particles in the nucleation mode illustrate BASE and NUCL simulation inputs (dotted black lines)."

13. Fig. 8, there are such large differences in the measurements of HTDMA for 4/25 and 5/24 in the smallest mode (although it is not clear each colored solid line represent), then any fit should have very large uncertainty. Is it meaningful for such a fit?

Clarification added to Section 4.1: "Since nucleation-mode aerosol (in the smallest fitted mode) are present very non-uniformly in time and space during some MC3E case studies (cf. Fig. 6), we finally test whether that is likely to be important."

14. Fig. 9, what is N? What is total aerosol number size distribution?

Clarification added to figure and caption: "Derived modes and aerosol number size distribution over 1-km-deep layers (black dotted and dashed lines, respectively) compared with bin-wise mean and median out-of-cloud UHSAS size distributions (red and blue lines, respectively) for the 25 April case study, with sample size (cf. Fig. 4) and total aerosol number concentration (N_a) in cm⁻³."

15. P6 Line 27-32, the text here is confusing: first, need to be specific about aerosol configurations in AERO. It is not enough to just say "initialized with the aerosol profile described above" since it is not clear "above". To me, Fig. 8 is above but there are many different aerosol parameters listed on the panel for 5/20.

Clarification added: "Aerosol are initialized within all domains to the 20 May aerosol input profile derived as described in Section 3.4 (see Supplement 1), and are fixed to it at the outermost domain boundaries."

Second, since AERO has prognostic droplet number concentrations, I do not understand why need to fix droplet number concentrations at the boundary? Shouldn't fixing aerosol be enough?

Clarification added per response to comment #16.

Third, I do not understand "Unknown aerosol source terms are neglected", thus I am confused with the later part of the sentence "how all else being equal, this increases the difference between BASE and AERO results".

By unknown we meant that aerosol source terms cannot be readily observed and specified. Simplification and clarification made also in response to referee 1: "Aerosol source terms beyond advection across outer domain boundaries are neglected (e.g., primary emission and gas-to-particle conversion)."

Lastly, it is not clear what cloud microphysics scheme is used for other simulations besides BASE.

Clarification added: "We compare observed hydrometeor size distribution properties with those simulated using Morrison et al. (2009) two-moment microphysics with hail." Additional detail is then added on the ice nucleation parameterizations used throughout (mostly off in HOMF).

16. P6 Line 33, BASE should have no aerosol since droplet number is not prognostic as shown in Table 1.

Clarification added: "In the baseline simulation (BASE), we use a fixed droplet number concentration of 250 cm^{-3} . In the AERO simulation, droplet number concentration is treated prognostically as follows."

17. P7 Line 1-2, why 8000 cm^{-3} ? This sounds a very large aerosol number concentration.

Reference added and clarification also in response to referee 1: "Based on the April and 1 May nucleation-mode fits listed in Fig. 6, this represents the most commonly fit mode diameter and rounded mode standard deviation, and a modest number concentration (maximum on 1 May) that is lower than typically observed in the 10–30-nm diameter range during intense new particle formation events (e.g., Crippa and Pryor, 2013)."

18. P7, the third paragraph and Fig. 12: Q2 and Q2corr cover the entire domain, why not compare the precipitation over the entire domain? Suggest to add such a plot to Fig. 12 (after all, it would be a more robust comparison compared with that over a small domain of $100 \times 100 \text{ km}^2$).

We illustrate observed and simulated precipitation rates over the entire domain in Figs. 9 and 16 for context, but the objective of Fig. 8 is to show the observed and simulated time series specifically within the aircraft sampling domain that is also used for the comparisons of stratiform ice and rain properties. Clarification added to caption: "averaged over the region sampled by aircraft after 13 UTC indicated by a red rectangle in Fig. 9."

19. Figure 14, There is only one observation dataset shown in the figure, why are there two sources (Wang et al. 2015a and Wu and McFarquhar 2016)? The related discussion about the two measurements is on P8 Line 9 but the figure does not show both.

The box and whisker plots contain both observational data sets. Caption simplified" "from aircraft observations (left, see text) and from the BASE simulation (right)". Clarification added Section 4.2.1: "Fig. 10 shows ice water content (IWC) and ice number concentration (Ni) from both independently derived observational data sets."

20. P9 Line 12-14, If Morrison scheme is used, do you consider second droplet nucleation or only cloud-base nucleation is considered? I would expect secondary nucleation at higher altitudes could make significant differences if small CCN is present. Therefore, I would suggest to do another test with the secondary nucleation considered if it is not considered in the NUCL.

Clarification added to Section 4.1: "Aerosol activation follows the treatment of Abdul-Razzak and Ghan (2000), in which the supersaturation is taken as the minimum value over the time step following Morrison and Grabowski (2008, their Eqn. A10), as in Vogelmann et al. (2015, see their Sect. 5.1)." This approach does not limit droplet activation to cloud base.

21. P9 Line 18-20, I think the point is mainly supported by much smaller ice particle number concentration simulated by the model.

We consider uncertainty in observed particle number concentration far greater, as emphasized in the last sentence of the following paragraph.

22. Figure 21, please define Z_m and Z_{HH} . Also, I do not understand why each panel is plotted for a different time? And the figure order does not reflect a time evolution, and the color legend is different for the same type of figures between observation and model simulation such as Panels 2 and 3. What does the red color denote in the first four panels?

Clarifications added to caption also in response to referee 1: "Horizontally polarized radar reflectivity (Z_{HH} in dBZ) from KVNK radar (left, dotted red circle): (top) example updraft object at ~12 UTC (solid red) among others identified in units of dBZ km (red-enclosed, see text), (middle) movement of example updraft from initial location (solid red) towards intersection with the aircraft sampling location (white-enclosed, see text) projected onto 2-km Z_{HH} at ~14 UTC, and (bottom) Z_{HH} curtain obtained from column-wise averages over tracked regions from ~12–15 UTC with Citation ascent legs in time and height (white bars) and averaging time used in Fig. 22 (white lines). From the AERO simulation (right): (top) identification of a typical updraft object projected onto simulated Z_{HH} at ~11 UTC (solid red) among others identified (red enclosed, see text), (middle) its movement from the identified location (solid red) to intersection with the aircraft sampling location (white-enclosed, see text) projected onto simulated 2-km Z_{HH} at ~13 UTC, and (bottom) Z_{HH} curtain obtained from column-wise averages over tracked regions from ~11–14 UTC with mid-point of hour-long averages used in Fig. 22 (white lines)."

23. P10 Line 5-6, why suddenly talking about BASE since only AERO is compare with observations in both Figures 21 and 22.

Corrected, thank you.

24. P10 Line 30-31, suggest to reword the sentence. It is not easy to understand currently.

Agreed, reworded: "We note that breakup equilibrium is thought to require rain rates on the order of 50 mm h^{-1} , substantially greater than typical of stratiform regions (e.g., less than 15 mm h^{-1} in Fig. 8), but its existence, size distribution characteristics, and prevalence in nature have been elusive (e.g., McFarquhar, 2010; D'Adderio et al., 2015)."

25. P11 Line 15-16, "we find that predicted and observed stratiform ice size distributions are similarly coherent within the stratiform region": I am not sure what this sentence really means since simulated and observed size distributions are totally different as shown in Figs. 14-17.

Clarification added also in response to referee 1: Reworded to "both predicted and observed stratiform ice size distributions exhibit relatively well-defined properties that do not vary rapidly in time."

26. The third paragraph in Section 5: this paragraph summarizes observed results. It is natural to comparatively describe how model does here, and this information is missing from the summary currently.

Simulated number concentration and peak of ice mass size distribution are summarized in the last sentence of the second paragraph. Added there re sensitivity tests: "Results are insensitive to prognosing droplet number concentration using an observation-based profiles with or without nucleation-mode aerosol (in place of fixed droplet number concentration). Additionally turning off all ice nucleation and multiplication parameterizations except homogeneous cloud droplet and raindrop freezing leads to less and larger ice."

Added to the end of the third paragraph : "In simulations, unlike in observations, the D_{max} where the mass size distribution peak increases substantially with mass concentration at each elevation (where there is more ice mass, it is also systematically larger) and the number concentration decreases rapidly with elevation. Beneath the aircraft-sampled region, simulated mass-weighted mean diameter of rain is roughly 0.7 mm larger than retrieved, consistent with overlying ice size bias; collocated reflectivity within the range observed is consistent with a corresponding low bias in precipitation rate (Fig. 8)."