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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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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 two-moment 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 Dmax of 400 μm, and a mass mode near Dmax of 1000 μm becomes dominant with decreasing elevation to the -10 0C. Therefore, the study is worthy being published. However, this reviewer does have some

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concerns about the current form as listed below,

(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.

(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.

(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.

(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. 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).

(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.

(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.

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".

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

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

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?

5. P3 Line29, aerosol should be plural here.

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.

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

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

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

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.

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

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.

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?

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

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. 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?

Third, I do not understand “Unknown aerosol source terms are neglected”, thus I am confused with the later part if the sentence “how all else being equal, this increases the difference between BASE and AERO results”. Lastly, it is not clear what cloud microphysics scheme is used for other simulations besides BASE.

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

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

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 100x100 km2).

19. Figure 14, There is only one observation dataset shown in the figure, why are there two sources (Wang etal. 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.

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.

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

22. Figure 21, please define Zm and ZHH. 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?

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

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

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.

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.

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