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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 #1

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General comments This article constructs hygroscopic aerosol size distribution profiles from MC3E aircraft and ground-based data over six days. These profiles are used to run 4 NU-WRF simulations of a squall line case study. Observed and simulated cloud ice microphysical properties in the stratiform outflow region are then compared.

The work is very comprehensive and cites the existing literature thoroughly. The results about similarity between continental and tropical ice microphysics are quite interesting. Although factors like "fall speeds, aggregation and vapor growth rates, [etc]" are listed in the results, I would have appreciated more discussion on how the modeled

ice microphysics might be improved to bring something like the number and mass size distributions into better agreement with observations. I missed also a discussion of the one-hour offset between the simulated and observed rain event initiation. Is there a hypothesis for this?

There were two other points on which I would have appreciated clarification. I was surprised by the result that modeling only homogeneous freezing (HOMF) results in "substantially larger and fewer outflow ice crystals". Normally homogeneous freezing yields many more and smaller crystals (e.g. DeMott et al. 1998 GRL). Why does the opposite occur here? Then I found the results for the size distributions in Figures 14 to 17 and radar reflectivity in Figure 22 incongruous: the distribution comparisons indicate that the simulated ice crystals are far too big, while it is suggested from the reflectivity comparison that the simulated ice is too small. Am I missing something? Clarification in both cases would be helpful.

Otherwise my comments are related to readability. I find the article rather figure-heavy, and I think the results would be made be more accessible if the figures were condensed in some places and simplified in others. For example, Figure 2 is only referred to once, and since only the 20 May panel is particularly relevant, this panel could be combined with Figure 12. In Figure 13, only the rain gauge-corrected QPE measurements and BASE simulation are discussed, so panels a and c could be removed. Or Figures 17 and 18 could be moved to Supplemental Information, since the altitudinal dependence of Ni and mass distributions is already seen between Figures 15 and 16 and the discussion of 2DC images is quite brief.

I think breaking down the "Evaluation of hydrometeor size distributions in 20 May case study simulations" section into subsections, e.g. "Precipitation intensity", "Mass and number concentration distributions", and "Radar retrievals", would also ease readability.

Specific comments Page 4, Line 6 – Please be consistent in the instrument acronyms.

What is called the “DMA” here is later called the “HTDMA” in Figure 3 and introduced as the “TDMA” in Section 2. Again for the CPC, it is not always clear whether the measurements to which you refer are form the ground-based or aircraft CPC; it is inferred from the other instruments you mention. You could make this more explicit.

Page 6, Line 31 – The statement “unknown aerosol source terms are neglected” is unclear to me. The airport and power plants are mentioned in the section of Aerosol input data, but there is no discussion of back trajectories or systematic confirmation of hypothesized sources.

Page 7, Lines 1-2 – Is there also a quantitative basis (other than “similarity to April case studies”) for the 8000 cm-3 and 0.005 um values chosen in the NUCL simulation? If so, this should be mentioned.

Page 7, Line 3 – The statement “simulations use a preliminary version of the 20 May aerosol input data” is unclear to me. The Aerosol input data section does not mention multiple processings or versions.

Page 8, Line 1 and Page 47, Table 2 – Could you please include the standard deviation in the “top three elevations”, e.g. 7.6 $\pm$ x m, and associated temperatures?

Page 11, Line 16 – It is not clear what “similarly coherent” means here. Could you word this more substantively?

Page 28, Figure 7 – The caption indicates that the CPC profiles on the left and UHSAS profiles on the right are in red and blue respectively as in Figures 4 and 5, but this is not the case. The thick black line for layer-wise median ratio is not so easily distinguished from the thinner black lines; perhaps the UHSAS/CPC traces can also be changed from black in the rightmost subplot. Finally it is not clear what the “layer-wise” ratio means; are these values also calculated for km-deep layers?

Page 29, Figure 8 – The numbers in the subpanels of this figure need to be moved to a table. This will significantly ease comparing the values between days and allow the

y-axis to be readjusted for better comparison of the different traces. It is also unclear to me what the various colors (red, green, blue, purple, black) represent. The caption refers to “measurement time”, but this should be clarified. A brief discussion of why the 2-mode fit is better than the 3-mode and vice versa at certain times might also be included in the third paragraph of page 5.

Page 35, Figure 14 – In my opinion, this figure could be removed, and the simulated values added to Table 2.

Page 41, Figure 20 – I am confused by the black BASE trace for number concentration. Doesn’t this simulation have a fixed droplet concentration of 250 mg-1, as stated on page 6, line 26?

Page 42, Figure 21 – It is unclear whether only the top left panel is an integrated reflectivity; it seems so given its different scale, but this should be clarified in the caption. A definition of ZHH (as the horizontally-polarized radar reflectivity, right?), along with definitions for the pink, white, and red circles in various subpanels, would help in the interpretation of this figure.

Page 43, Figure 22 – “Time 1”, “Time 2”, etc. have not been defined for the simulations. It would be clearer to label the gray traces ‘AERO, Time1’ etc. so that the reader knows these are only from that simulation.

Technical comments / suggestions Page 3, Line 25 – A term like “droplet activation” or “ice nucleation” or “new particle formation” would more clearly indicate the process(es) meant by “aerosol consumption” here.

Page 3, Line 26 – Remove the second “be”.

Page 6, Lines 9 – 10 – Add a ‘to’: “appears to be variably biased relative to the ground-based measurements”.

Page 8, Lines 16-22 – Reword through here for clarity, e.g. “Consistent with underestimated Ni, the Dmax at which BASE mass distributions peak is roughly 3-5 times

larger than that at which the observed distribution peaks. The Dmax at which the BASE mass distributions peak increases monotonically with increasing mass concentration, whereas the observed mass distributions tend to..

Page 8, Line 28 – There is an unfinished sentence beginning with “At 6.7 and 7.6 km”.

Page 12, Line 18 – “Updrfts” to “updrafts”

Page 14, Line 4 – “have” to “has”

Page 14, Line 6 – “are” to “is”

Page 22, Figure 1 – It would ease readability if the ARM central facility were marked with a color other than yellow, since the pentagon, bull’s eyes, and thumbtacks are all yellow as well.

Page 24, Figure 3, panel d – Is there a red trace for 0.013 um here? If so, it is not visible.

Page 33, Figure 12 – It would ease readability if Q2 were expanded to National Mosaic and Multi-Sensor QPE system in this caption, as well as in the text, and again if QPE were expanded here and in the text.

Pages 36-38, Figures 15-17 – The red and blue traces should be labeled PMS 2DC and HVPS rather than obs1 and obs2.

Page 41, Figure 20 – The y-axis should be ‘[km]’ not ‘[m]’.

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