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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Potential indirect effects of aerosol on tropical cyclone intensity: convective fluxes and cold-pool activity" *by* G. M. Krall and W. R. Cottom

Anonymous Referee #2

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Review of "Potential indirect effects of aerosol on tropical cyclone intensity: convective fluxes and cold pool activity" by Krall and Cotton

Major comments:

This manuscript describes simulations of a Western Pacific typhoon conducted with RAMSv4.3. A series of sensitivity studies with increasing concentrations of cloud condensation nuclei (CCN) are analyzed, and conclusions are made about how the CCN influenced the evolution of the typhoon. The paper is well-written and easy to follow, and the quality of the graphics is fine. However, I do not feel that the conclusions made are justified through the analysis presented.

It is well known and there have been multiple studies published showing that tropical cyclones with different intensities will result when CCN concentrations are varied. The authors attempt to expand upon these previous studies by showing how quantities such as the total amount of supercooled liquid water, downward flux, size of the cold pool, and hydrometeor mixing ratios vary for simulations with different CCN. The authors claim that the differences in these fields, and the difference in evolution that they see between their simulations are caused by the variations in CCN number. However, a closer look at their analysis shows that this conclusion is worrisome for a number of reasons:

1) For the most part, the variation between simulations is not a monotonic function of CCN number. For example, in Fig. 5 the largest spike in the supercooled water is associated with the C3000 simulation (not C5000). The two simulations with the lowest supercooled water seem to be either C100 or C5000. The C1000 and C3000 simulations typically have more cold-pool grid cells than the C400 or C5000 simulations. The C1000 and C3000 simulations also typically have larger wind speeds than the C400 or C5000 simulations. The lack of a monotonic relation with CCN number lends one to suspect that a lot of the differences between simulations are not directly caused by variations in CCN number, but rather simply exhibiting the response of a non-linear system due to noisy initial conditions. In order for the authors to demonstrate that their results are due to variations in CCN, their results must be interpreted in the context of how variations in other fields affect the TC intensity.

2) A lot of the results in the paper are shown at specific times (e.g., 86, 84 or 68 hours after the simulations were initialized). Given the spikiness of the signals shown in Fig. 5, 6, 9 and 13, comparing plotted fields at specific times may result in misleading conclusions about whether the differences exhibited in supercooled water or downward flux at specific times are due to the differences in CCN between simulations, or whether they are just due to slight differences in the evolution of the fields between the simulations. Why were these specific times chosen? If alternate times were chosen

would the same systematic differences exist? Can you average over longer time periods and apply some statistical tests to determine the degree of difference between the simulations?

3) The authors make a number of claims about certain processes causing the differences between simulations that have not been categorically shown in the presented analysis. For example, the authors state that “cloud droplet collision and coalescence is suppressed” for increased CCN concentrations. In order to make this conclusion, the authors should specifically present an analysis of this prognostic term in the relevant microphysical equations. The authors also state that “the fall speed of the smaller particles is reduced, causing the system to thrust more condensate to higher levels.” Again, it would be nice to have a budget analysis showing thisâĂŤgiven the small fall speeds of cloud droplets and their corresponding small terminal fall speeds, how important is this difference in size when compared to the speed of the updrafts? The authors also need to demonstrate through analysis of terms in the prognostic water budget equation that the other following statements are true: “smaller droplets are less likely to be rimed by ice particles. . .[and] less likely to freeze at near-zero freezing temperatures;” “results in elevated latent heating of freezing;” “excessive transport of water substance to anvil levels occurred;” etc.

4) I am concerned about the resolution of the simulations. The use of 20 vertical levels and horizontal resolution of 3 km seems quite coarse given the resolution of other simulations that are and have been conducted of aerosol/cloud interactions in typhoons. Have the authors done any sensitivity tests to determine whether their conclusions are dependent on the resolution of the simulations that area being used?

5) It would be easier to believe that there is a tipping point in the response of the typhoon to changes in CCN if there were was more than one simulation showing the decrease after this tipping point. For example, a C4000 simulation or C4500 simulation showing the same response in the same direction would add more evidence to the claim of a tipping point.

There are also some other more minor points that should be taken care of before this paper is ultimately considered for publication.

Abstract: The abstract is overly long with some things repeated. The conclusions should also be made more quantitative and less qualitative, sticking to the points that were actually demonstrated in simulation (e.g., the simulations did demonstrate that SCLW and DFX are correlated in a statistically significant sense).

P. 352, line 15: Statistical tests need to be applied to show that the perturbation of windspeed, convective fluxes and hydrometeor species depend on the elevated CCN. Given the large temporal variation in these terms, it is not readily obvious that a statistically significant dependence exists.

P. 352, lines 20-28: A lot of the statements in this section have not been demonstrated through analysis in the paper, and need to be removed of shown in the paper (e.g., enhanced rainfall; more vigorous convectively-produced downdrafts (seems to depend on which simulations examine); do a budget analysis of amount of condensate thrust into storm anvil, etc.

P. 357, line 25-26: Can you supply a figure or identify the criteria that shows what part of the domain was affected by MODIS retrievals of elevated aerosol concentrations (i.e., how high to the concentrations have to be to be considered elevated?)

P. 358, lines12-13: Explicitly show how much riming growth and collection are suppressed because they depend on many variables, and will also vary depending on what the liquid water content is (lots of non-linear dependence can affect how large the riming and collection terms are)

P. 358, line 17: Can you show the CCN? It is hard to interpret the results without seeing the evolution of the CCN field?

P. 359, line 1: If the delays are being attributed to CCN, why is the C3000 spike before the C1000 spike?

P. 359, line 19: It is a bit worrisome that the C5000 case deviates little from the C100 case. Because of this, how do you know that all the differences shown are not just caused by variations in system due to non-linear responses to varying initial conditions?

P. 359, Line 26: Why is a lag time of 2 h used? How do the results change if a different lag time is used?

P. 360, Section 4.3: It is well shown that SCLW is correlated with DFX to a statistically significant degree. However, the dependence on aerosol amount has not been demonstrated to a statistically significant sense.

P. 362, lines 20-22: The “invasion” of the pollution plume into the eyewall region should be explicitly shown.

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