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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the robustness of aerosol effects on an idealized supercell storm simulated with a cloud system-resolving model" *by* H. Morrison

Anonymous Referee #2

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General

The main goal of this very thorough and interesting study is to explore how robust aerosol effects are in deep convection in the absence of various microphysical processes, and under small perturbations to the initial conditions. This is achieved through multiple WRF simulations of a supercell. The results suggest that a similar storm response can be obtained even in the absence of various microphysical and thermodynamical processes due to compensating processes, thus making it difficult to isolate aerosol impacted responses and processes. Some sensitivity in both the magnitude and sign of aerosol responses was observed for small perturbations in the convective

initiation and vertical wind shear. The author suggests that the pathway forward should include improvement to parameterizations and ensemble methods.

The research presented in paper is novel, and makes a sIgnificant contribution to the way in which the community should analyze and think about aerosol effects in deep convection. The manuscript is very well written, and is clear and easy to understand, with the exception of several minor statements. As such, this reviewer has no hesitation in recommending the paper for publication in ACP pending the revisions outlined below, all of which are minor in nature.

Specific

The abstract effectively captures all the main points of the paper however it is relatively long, and could be shortened.

Line 69: The recent review paper by Tao et al (2012) gives excellent summary of precipitation responses in deep convection and should be included.

Line 74: Quite correct although some recent studies have tried to address this issue such as the papers by Van Den Heever et al (2011) and Seifert et al (2012).

Line 79: Responses to static stability (Matsui et al 2006) and CAPE (Storer et al 2010) have also been evaluated.

Line 175: While supercell simulations are frequently conducted with free slip boundaries and no surface heat fluxes, and while such assumptions are valid here too, it would be useful, given the focus of the paper, to point out that such assumptions will influence the cold pool characteristics and hence the magnitude of the responses shown here.

Line 181: A reason should be supplied for why this height was altered.

Line 197: Van Den Heever et al (2011) should also be referenced here.

Line 203: It is agreed that the inclusion of aerosol schemes does add to the complexity

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of the problem, and that such an inclusion would not change the main finding, however it would be instructive to state that aspects such as nucleation processes and the associated latent heat release will not be prognosed and hence explicitly represented in this study.

Line 206: Droplet concentrations of 50/cc would seem to be very clean for typical supercell environments. Have supercells ever been observed in such clean environments?

While it is recognized that the author is examining storm responses to a range of aerosol concentrations, it would make sense to keep these within more typically observed clean values. This becomes even more important when the responses between MOD and POLL don't appear to be that great. Can the author please comment on this?

Line 213: It would assist the reader if a figure was included of the BASE simulation showing the basic storm development over the two hours. This also would help orientate the reader with the left- and right-mover discussion, cold pools etc.

Line 217-220: This point is related to a point raised previously regarding the applicability of such a clean environment. How significant are the differences between MOD and POLL? This is not apparent from Table 2. It would be useful to include plots for MOD on Figure 3 to convince the reader that differences also exist between POLL and MOD, and not just between these two cases and PRIS, especially given the uncertainties of such clean supercell environments. If the differences between POLL and MOD are significant then MOD could be left off the subsequent plots.

The quality of the line plots would all need to be improved before being suitable for publication. Also, the figure panels need to include a,b,c labels etc. Figure interpretation could also be made easier for the reader if the appropriate panels had headings included such as POLL.

Line 250: Are these findings true at other height too?

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Lines 250-269: A paper by Storer et al (2010) also found mixed responses in vertical velocity, greater differences in the left-moving supercell and similar cold pool responses for a range of aerosol environments. The results from this paper should be referred to here.

Lines 278-279: "separates into two separate cells" This statement is a little confusing given that both cases do separate into two separate cells, but the way that it reads it could imply that only POLL case does. It just needs some rewording.

Line 286: Gilmore et al (2004) did extensive work on parameter sensitivities and should be referenced here.

Line 291-292: The Storer et al paper referred to above found warmer cold pools in polluted conditions.

Lines 308-312: Are these results true for MOD too?

Lines 312-313: That the colder cold pools are larger in area is not a surprising result.

Lines 331-339: When reading the manupscript this paragraph takes the reader somewhat by surprise in that very little has been said up to this point about the microphysical processes, the reader to have a prior paragraph introducing the microphysical patents and processes. Line 337: "cloud water and rain" - does this include ice?

Lines 342-345: Turning off a process in the model to assess the importance of this process can lead to confusion in the analysis of the importance of this process. Turning off one process forces the model to compensate elsewhere, sometimes in unrealistic ways, ways that nature may not implement. Thus, while the model demonstrates different pathways when turning off a process, such responses or pathways may not be observed in reality, and hence we need to be careful of our analysis of such results. That said, there are certainly numerous different ways that nature does get to the same end point, the various ice hydrometeors being an example of this. This comment is not in disagreement with what the author is stating, but is possibly a different way of looking

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at it.

Line 344: Can the author comment on how "network-like behavior" differs from the buffering concept in Stevens and Feingold?

Line 354: replace weaker with less

Line 385: Surely deposition leads to condensation and the release of latent heat? It is somewhat confusing to separate condensation from deposition. This simply requires clarification in the text as to what is or is not included.

Line 391: "context of the system as a whole". The reviewer agrees that it is extremely difficult to assess the importance of a process without testing this within the system as a whole. However, this comes back to the point made earlier that models are forced to potentially artificially compensate. For example, perhaps we want to investigate the role of ice processes in a supercell and so we turn off all ice processes. This leads to artificial exaggeration of liquid water processes. Analyzing the model output we may reach some conclusions regarding the importance of fall speeds, evaporation, the lack of melting etc. And yet such alternative processes or routes would not occur in reality for soundings and storm structures that support the presence of ice. This is admittedly an extreme example, but is being used to help describe the point here. Once again, the reviewer does not disagree with the author, but is simply coming to similar conclusions from a different point of view!

Does the author have any comments on this?

Lines 451-461: Van Den Heever et al (2011) should be referenced here as they looked at aerosol effects on large domains over long timescales.

Lines 487-490: Agreed. This goes back to papers such as those by Gilmore et al (2004) who looked at sensitivity of supercells to graupel and hail parameters, as well as to similar research by Fovell, Bryan and the author himself on the sensitivity of squall lines to such parameterizations. The author points to the need to improve such

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parameterizations, however, in order to do this we as a community are going to need better observational data in this regard.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 10493, 2012.