Response to Reviewer #2, ACP-C3335-C3337, 2012: "On the robustness of aerosol effects on an idealized supercell storm simulated with a cloud system-resolving model".

I thank the reviewer for his/her helpful comments that have improved the paper. Reviewer comments are in bold, responses are in plain text.

Specific comments:

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

   It is agreed that the abstract is fairly long (382 words). Most of the points in the abstract are needed to give the reader an overall sense of the study, however, the sentence "This occurs because of compensation by other process interactions, illustrating network-like behavior of the system." is not necessary and has been deleted to address the reviewer's point.

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

   Reference to Tao et al. (2012) has been added (this was also a suggestion of Reviewer #1). I thank the reviewer for pointing out this paper; it was actually published after submission of the current manuscript in late February.

3. **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).**

   I did not see anywhere in van den Heever et al. (2011) or Seifert et al. (2012) papers where they specifically discuss the difficulty in quantifying aerosol effects based on a single realization, due to rapid growth of small perturbations and fundamental limits on predictability. The simulations of Seifert et al. (2012) suggest the importance of this issue, given that aerosol effects on precipitation can be large locally at a single time, but are much smaller when averaged over larger spatiotemporal scales. However, the authors of this paper suggest this is due to multiscale feedbacks and in particular feedback with mesoscale dynamics (see p. 722 in their paper). An alternative interpretation of their results is that the growth of small perturbations, which occurs preferentially at the smallest scales, yields large local, instantaneous differences in precipitation in different realizations, but these differences are small when averaged over larger spatiotemporal scales because perturbation growth rapidly saturates over space/time (that said, feedback with the mesoscale dynamics is certainly an important aspect of this perturbation growth as well, so these two interpretations are closely related to one another).

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

   Reference to Storer et al. (2010) has been added to the revised manuscript as suggested by the reviewer. I thank the reviewer for pointing out this paper. While Matsui et al. (2006, JGR) also examined aerosol effects on clouds, they focused on low clouds, not deep convection, and therefore this paper has not been included as a reference here.

5. **Line 175: While supercell simulations are frequently conducted with free slip bound- aries 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.**

   Mention of the impact of free slip lower boundary and neglect of surface heat fluxes on cold pool evolution, and possible influence of the magnitude of the response to aerosol loading has been added to the revised manuscript. See lines 177-180 on p. 9.

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

   The reason this was altered was because this is the hodograph used in the default idealized WRF quarter-shear supercell case, which was modified from Weisman and Rotunno (2000). This has been clarified in the revised manuscript (see lines 184-186 on p. 9).

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

   Agreed, the van den Heever et al. (2011) paper should have been included here; this was an oversight. The citation has been added to the revised manuscript.

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

   I disagree with the reviewer about aspects of aerosol microphysics such as nucleation contributing to latent heating (except for nucleation and growth of haze particles, which has negligible contribution to latent heating).

Activation of CCN as cloud droplets, strictly speaking, does not result in a phase change (therefore, technically, it's not a "nucleation" process). Moreover, any droplet condensational growth and resulting latent heating is represented by saturation adjustment, which is certainly a simplification, but explicit representation of condensational growth (i.e., through explicit prognosis of the supersaturation field) is also possible without having to explicitly represent aspects of aerosol physics such as activation of cloud droplets from CCN. See Lebo et al. (2012, in review ACPD) for detailed discussion of the impact of supersaturation prediction on simulations of a supercell storm. The use of saturation adjustment is mentioned in the revised manuscript, along with reference to Lebo et al. (2012). See lines 208-213 on p. 10.

9. **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?**

   This is an excellent point. It is agreed that a droplet concentration of 50 cm-3 is lower than typically observed for shallow cumulus in environments comparable to those supporting supercells (i.e., mid-latitude continental regions), even for remote and relatively pristine locations. For example, during CCOPE measurements were taken in cumulus clouds in Montana and described by Blyth and Latham (1991, JAS). They found concentrations typically between ~ 100 and 750 cm-3 for non-precipitating shallow cumuli; however, mean concentrations were as low as 70 cm-3 for one penetration (see Table 1 in their paper). Similar values were found in New Mexico cumuli; values were as low as 78 cm-3 for one penetration but typically between 100 and 300 cm-3 (see Table 2 in Blyth and Latham 1991). As to the question "Have supercells ever been observed in such clean environments?", this is difficult to answer given limited observations. Given the difficulty of obtaining in-situ measurements from aircraft in supercell updrafts, droplet concentrations in supercells are uncertain; it is unclear how representative observations in non-precipitating cumuli are in terms of strong, deep convection. Detailed modeling with bin microphysics has suggested that rather low droplet concentrations can occur in updraft cores due to significant collision-coalescence, with concentrations far below the background CCN concentrations (Z. Lebo, personal communication); however, it is uncertain if this occurs in reality given the lack of observations.

   Although 50 cm-3 is probably on the low end of what might occur in reality even in highly pristine conditions (for mid-latitude continental locations), given the uncertainty of droplet concentrations in supercells and to understand supercell behavior over a wide range of conditions, values from 50 to 750 cm-3 were tested. Thus, this study demonstrated that that overall effects are small and uncertainty related to the complexity of process interactions and sensitivity to small perturbations in initial conditions are important even across a very large range of droplet concentrations. Nonetheless, the reviewer's general point concerning the range of droplet concentrations tested here is well-taken, and discussion has been added in the revised manuscript concerning the realism of using 50 cm-3 for the droplet concentration in pristine conditions (see lines 218-222 on p. 11).

Finally, it is noted that even though aerosol effects are small overall, which was noted in several places in the text including the abstract, there are notable differences between MOD and POLL. This is illustrated by adding results for MOD in Figs. 3, 4, and 5, as suggested by the reviewer in comment #11 below. Additional discussion of this point has been added in the revised manuscript as well (see also the response to comment #11 below).

10. **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.**

    This is a good suggestion. Radar reflectivity at the lowest model level at 4 different times (30, 60, 90, 120 min) for BASE-PRIS has been included as an additional figure in the revised manuscript (Fig. 7) so the reader can see the general storm evolution over the 2 hour simulation period, especially in terms of storm splitting and evolution of the left- and right-moving storms.

11. **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.**

    Following the reviewer's suggestion, results from MOD have been added to Figs. 3, 4, and 5 in the revised manuscript, so that the reader can more easily see differences between PRIS, MOD, and POLL. More discussion of differences between PRIS, MOD, and POLL has also been added to the revised manuscript in several places. Overall, differences between MOD and POLL are similar to differences between PRIS and POLL, but with a somewhat reduced magnitude of effects (e.g., see the revised Figs. 3 and 4). See also the response to comment #9 for further discussion of this point.

12. **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.**

    All final figures have been re-plotted using high-resolution tiff, and figure panels a), b), etc. have been added as suggested by the reviewer. If the production staff feels that better quality figures are needed, I will convert all figures to eps.

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

    Yes, MFc (average convective mass flux as defined in the paper) provides a more robust measure of differences in overall convective intensity at other heights as well (see Fig. R1 below for an example at a height of ~ 10 km). This point is clarified in the revised manuscript.

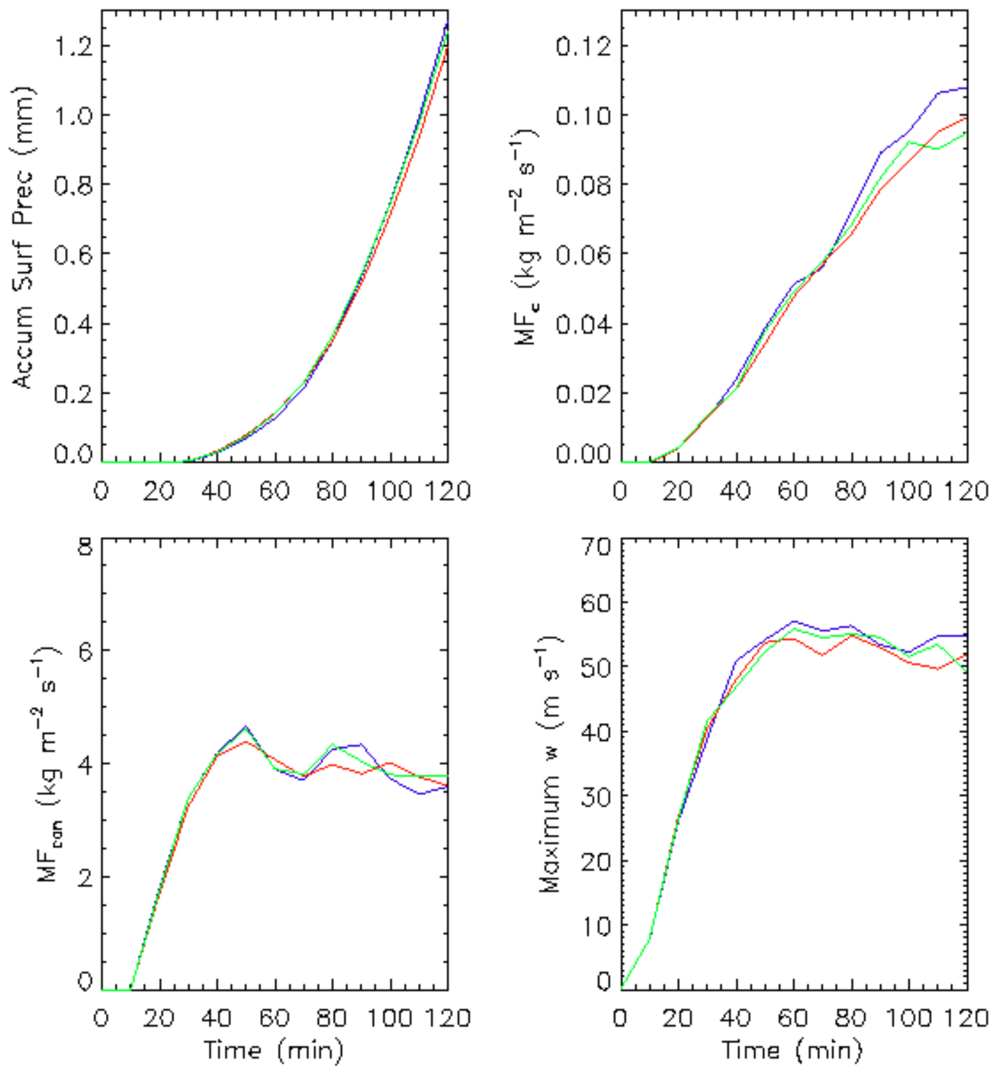
14. **Lines 250-269: A paper by Storer et al (2010) also found mixed responses in vertical velocity, greater differences in the left-moving supercell and similar cold pool responses for a range of aerosol environments. The results from this paper should be referred to here.**

    Reference to the Storer et al. (2010) paper has been added in the revised manuscript, along with a few sentences comparing and contrasting results of their study with the current study. See lines 295-299 on p. 14-15 of the revised manuscript.

15. **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.**

    This sentence has been reworded to clarify that the left-moving storm separates into two distinct updraft cells in both POLL and PRIS. This is perhaps the initial stage of a transition from supercellular to more multicellular convection associated with the left-moving storm, as was found by Storer et al. (2010). However, I don't feel that the multicellular characteristics are clear enough here to definitively refer to the storm as such.

Figure R1. Timeseries of a) domain-mean accumulated surface precipitation, b) domain-mean convective mass flux, MF_c, c) convective mass flux averaged only within convective cores, MF_{con}, and d) domain-maximum vertical velocity, w, for the baseline model configuration (BASE). Results for pristine, moderately polluted, and highly polluted conditions are shown by blue, green, and red lines, respectively. Results for MF_c and MF_{con} at a height of ~ 10 km are presented.



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

    Gilmore et al. (2004) is referenced here.

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

    The Storer et al. (2010) paper is referred to when discussing changes in cold pool strength in polluted conditions and contrasting results with Tao et al. (2007), Lee et al. (2008), and Lee (2011).

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

    Yes, POLL also produces a moderate decrease in cold pool strength relative to MOD as well as a small decrease in mean surface precipitation and convective mass flux.

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

    Agreed, it is not surprising that other measures of cold pool strength such as cold pool area are consistent with differences in mean theta within the cold pool (this was also found in Storer et al. 2010, as an example). However, this is not necessarily always true. Therefore, I would argue it is worthwhile to point this out in the paper and have retained this in the revised manuscript.

20. **Lines 331-339: When reading the manupscript this paragraph takes the reader some- what 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?**

    All terms used in this paragraph should be able to be understood by most readers; in my view it is well understood that "cloud water and rain" does not include ice. Therefore, this paragraph has not been modified.

21. **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 at it.

The reviewer brings up a very interesting point here. I agree that turning off processes can in some instances lead to compensation and pathways that may not be realistic (e.g., in the simulations with no ice), and this should be mentioned in the paper (this has been added to the revised manuscript). There are two main points in response to this:

1) If a process is turned off and this leads to a large change in the system response, this implies that the process is important in driving the response. An example is the difference in the cold pool response to polluted versus pristine conditions in NOEVAPR (rain evaporation turned off) versus BASE and several other configurations. In NOEVAPR, there is little difference in cold pool strength between polluted and pristine, and a much larger response in many of the other configurations including BASE. This implies that rain evaporation is a key process explaining the cold pool response in BASE (as opposed to, say, changes in melting or some other process). Thus, even though the interaction pathways of a configuration such as NOEVAPR may not occur in nature, this configuration can help to determine processes that are important in driving the response in BASE.

2) Another key point is that even with large changes to the process parameterizations across different configurations (i.e., with the process turned off), in many instances the model produces a similar response due to compensating process interactions. The implication is that different parameter settings (or formulations) for a process may have limited impact on the overall system response, even when the differences are large (as demonstrated by a similar response even when processes were completely turned off). This compensation, which is reminiscent of "buffering" (see response to comment #22 below), can make it difficult to foresee the impact of parameter changes on simulations of aerosol effects on deep convection. Moreover, compensation by other process interactions when processes are turned off suggests the difficulty of isolating process interactions driving the system response. The fact that such compensation occurs even when processes are completely turned off suggests the likelihood of compensation when processes remain turned on but are modified in terms of formulations or parameter settings. Again, this highlights the difficulty of isolating processes driving the system response, as well as understanding causes of model differences in simulations of aerosol effects on deep convection.

This paragraph as well as that relevant to the related comment #25 below have been modified to clarify these points. See lines 380-387 on p. 19 in the revised manuscript.

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

    Network-like behavior in this instance is similar to the concept of "buffering" from Stevens and Feingold (2009), whereby a similar system response is evident despite key processes being turned off (e.g., ice microphysics), because of the existence of compensating processes and multiple interaction pathways. However, the concept of buffering in Stevens and Feingold was used to explain the tendency for a dampening of perturbations due to compensation by other process interactions within the system; here network-like behavior is invoked to explain the similarity of the system-wide response to a perturbation despite large differences in process representations among different model configurations. Thus, I would argue that the concepts are similar but not the same. Discussion of this point has been added to the revised manuscript, along with reference to Stevens and Feingold (2009) (see lines 375-379 on p. 18).

23. **Line 354: replace weaker with less**

    Done.

24. **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.**

    I don't quite follow the reviewer's comment. Both ice deposition and droplet condensation lead to latent heating. Here deposition strictly means the process of vapor deposition onto ice, condensation means vapor uptake by liquid droplets, and freezing means conversion from liquid to ice. Although all three processes lead to latent heating, it is useful to separate them because they are distinctly different microphysical processes. However, to avoid confusion these terms have been clarified in the revised manuscript.

25. **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!

As stated above, this is an interesting and certainly valid point of view. See response to comment #21 above for further clarification.

26. **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.**

    Van den Heever et al. (2011) should have been cited here. This has been added to the revised manuscript.

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

    This is a good point – I completely agree that we are going to need improved observational datasets and in particular better use of observations to improve and test models. This is clarified in the revised manuscript.