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, and the response is in plain text.

My main criticism concerns the choice of the ensemble members. The author decided to modify the microphysics scheme by turning off individual processes, their latent heat release or by making very strong modifications of particle properties (e.g. setting the fall speed of hail equal to that of snow). Such simulations are interesting to investigate the importance of those processes as it is discussed in the paper, but the disadvantage is that all these model perturbations are very unrealistic and the scheme is deteriorated. Therefore the spread of such an ensemble does not represent the uncertainty of the simulations, but is just a measure of the sensitivity to some quite arbitrary and unrealistic perturbations of the model physics. I would strongly recommend to include, in addition to the model configurations of Table 1, another ensemble which makes an attempt to quantify the uncertainty of the microphysics scheme within some realistic range. This would include parameters like the particle densities and the corresponding fallspeed-size relations, the particle size distribution assumptions (e.g., shape parameter of the Gamma distribution), the collision and sticking efficiencies of ice particles, and assumptions on freezing probabilities and ice nucleation. In addition, the KK au- toconversion/accretion scheme could be replaced, e.g., by the Berry and Reinhardt or Seifert and Beheng schemes to test the uncertainty due to this choice.

If the author is unwilling to perform these additional simulations, it should at least be made very clear in the text that the spread of the ensemble, e.g., as shown by Fig. 8, is not a representation of the model uncertainty.

The reviewer brings up an excellent point concerning the choice of ensemble members. The impetus for this work was to try and understand the process interactions producing the weakening of convective drafts in polluted compared to pristine conditions, as first shown by Lebo and Seinfeld (2011) and Fan et al. (2012) for this microphysics scheme/model. Given the complexity of the process interactions, as shown in the paper, it is very difficult to determine critical processers solely from a diagnostic analysis of process rates. This provided motivation for sensitivity tests in which processes are substantially modified or turned off, and follows a long history of such tests to improve understanding of process interactions in CRM simulations of deep convection. Thus, the motivation was *not* to characterize uncertainty associated with parameterization of various microphysical processes per se, which is a separate topic. As the reviewer points out, such a systematic investigation of sensitivity to microphysical process parameterizations (analogous to "uncertainty quantification" in climate modeling) would be valuable and should be pursued. However, given the large number of tests already run to address the specific goals of this study (robustness in untangling process interactions explaining the aerosol effects and sensitivity to small perturbations), such tests to investigate model uncertainty to microphysics parameterizations are beyond the scope of this paper. This is especially true given that attempts at systematically characterizing such model uncertainty require a large number of model runs and sophisticated statistical techniques for understanding model response across the huge multi-dimensional parameter space (especially given nonlinear interaction between different process and parameter uncertainties). Again, such efforts should be pursued in future research.

Nonetheless, the reviewer's overall point is well taken, and the text has been modified to make it very clear that the ensemble spread does not represent model uncertainty. This is first discussed when the various model configurations are introduced (see lines 226-231 on p. 11 in the revised manuscript). It is then discussed further in the discussion and conclusions section (see lines 516-524 on p. 25). This additional text clearly states that the purpose of these tests is to understand process interactions in explaining the system response, that some of the configurations have physical parameterizations that are not very realistic (e.g., tests with no ice microphysics), and that future work should focus on additional sensitivity tests to explore model uncertainty associated with physical parameterizations themselves.

Additional comments.

1. **The new review by Tao et al. (2012, Review in Geophysics, 50) should be included as a additional reference.**

   Reference to the new paper Tao et al. (2012, Rev. Geophys.) has been added. I thank the reviewer for pointing out this paper; it was actually published after submission of the current manuscript in late February.

2. **The aerosol effect on supercells including the sensitivity to wind shear has also been investigated by Seifert and Beheng (2006). Interestingly, they found a similar weakening of the supercell storms, e.g., about 10-20 % reduction of accumulated precipitation between pristine and polluted, as it was later found by Lebo and Seinfeld (2012) using their bin microphysics schemes.**

   Reference to Seifert and Beheng (2006) and accompasnying discussion concerning aerosol effects on supercells and sensitivity to wind shear and CAPE has been added (see lines 257-260 on p. 12-13). Note that in several studies, including Seifert and Beheng (2006), changes in surface precipitation may not necessarily be the same as changes in strength of convection (e.g. as measured by maximum updraft velocity). For example, in strongly sheared conditions supporting supercell storms, Seifert and Beheng (2006) show a small decrease in surface precipitation but a small increase in maximum updraft velocity in polluted compared to pristine conditions at high CAPE, while under lower CAPE there was a small weakening of surface precipitation and maximum updraft speed (see their Fig. 12). Hence, one must take care in how they define invigoration or weakening of convection.

Minor/technical comments:

1. **The plots look like they have been processed as bitmap (GIF or PNG) instead of vector graphics (PS or EPS).**

   Improved figures have been used revised paper (high-resolution tiff). If the production staff feels that better quality figures are needed, I will convert all figures to eps.