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Interactive comment on “Effects of aerosols on precipitation in north-eastern North America” by R. Mashayekhi and J. J. Sloan

Anonymous Referee #1

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This paper uses a coupled meteorology and chemistry model to examine the sensitivity of precipitation over a portion of North America during a 5-month period. They also examine the relative role of unresolved (parameterized) and resolved clouds on precipitation and quantify the correlation between cloud droplet number and aerosols, and the correlation between cloud droplet number and cloud-borne aerosols. This paper contributes to a growing number of studies examining the impacts of cloud-aerosol interactions via regional models. The paper is generally well-written and organized; however, there are three major comments I have with the manuscript before the paper is suitable for publication. I also have a number of specific comments that identify areas to be fixed or that need clarification. Some of my major concerns are also pointed out the specific comments.

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1) One concern I have with the paper is the interpretation of the results in the context of the parameterizations employed. The model uses a horizontal grid spacing of 12 km and thus also use a cumulus parameterization to represent convective precipitation. However, aerosol effects on cloud droplets only occur within the resolved clouds (Lin microphysics) and aerosols do not affect the unresolved clouds. Therefore, the results presented represent an incomplete effect of aerosols on clouds in the atmosphere and it is not surprising that “Although, total non-convective rain is less than total convective rain in the domain, chemistry-induced effects on the former are more pronounced than those on the later” as stated in lines 12-13 in the abstract. For example, including aerosol effects on convective clouds could either enhance or reduce the overall effect of aerosols on clouds. While there are currently few convective parameterizations that include indirect effects (they are currently under development), changes in the resolved clouds do indirectly affect convective clouds in addition to the direct effects of aerosols on meteorology. The authors need to put their results into the proper context throughout the manuscript.

2) Another concern I have is how the model has been evaluated and the lack of context regarding previous regional modeling studies of aerosol-cloud interactions. I appreciate the evaluation done in Section 3, but it relies solely on surface observations and aerosol-cloud interactions occur aloft. It is well known that there can be large variations in PM in the vertical; therefore, the performance aloft is not necessarily the same as at the surface. It is not clear why this period in 2009 was chosen, when there have been campaigns over the past several years that have sampled aerosol concentrations in portions of the domain. The authors could compare simulated AOD with satellite measurements to get an idea of how well the column burden of aerosol is simulated. In addition, there is no evaluation of whether the aerosol-cloud interactions are reasonable. While the authors focus on previous papers that cite the performance of WRF-Chem in terms of air quality, the purpose of the paper is to study the effects of aerosol-cloud interactions on precipitation. At a minimum, the authors should discuss the performance of previous studies of WRF-Chem where aerosol-cloud interactions have been

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evaluated more rigorously (e.g. Yang et al. 2011; Saide et al. 2011; Shrivastava et al. 2013). Another potential metric would be to compare the simulated cloud-top droplet number with MODIS measurements to determine the differences between WRF and WRF-Chem.

3) The paper neglects secondary organic aerosol formation. That could be an important factor for this domain and time period, which would subsequently affect aerosol-cloud interactions. At a minimum, the authors need to discuss the implications of this omission in the model description, where PM is evaluated, and in the conclusions.

Specific Comments:

Page 27938, Line 16-17: It is interesting that the authors examine two particle ranges separately, but what is the motivation for doing so? Where they expecting to be differences in the correlation between the two size ranges?

Page 27938, Lines 17-21: Here more results are presented, but the reader is left to figure out the importance of these statements. Would be useful to clarify what these findings mean.

Page 27940, line 16: McKeen et al. (2007) did not use aerosol-cloud interactions in WRF-Chem. Please check the references for accuracy and/or whether they apply to this statement.

Page 27940, lines 19-20: The sentence follows WRF-Chem and implies these studies are WRF-Chem studies, but Rosenfeld et al. (2007) and Lynn et al. (2007) are not. I believe they just use WRF and use prescribed aerosol numbers. Please re-phrase the text to be technically correct.

Page 27940, line 23. This sentence seems to begin a new paragraph.

Page 27941, line 17: I suggest replacing “agencies” with “organizations”, since some of them are not “agencies”. “NOAA, ESRL” should be NOAA/ESRL”. The authors should write out what the acronyms are as well.

[Interactive
Comment](#)

Page 27942, line 1: The authors note at the end of the introduction that aerosol size is important in terms of aerosol-cloud interactions; however, 4 size bins are used for MOSAIC. How different would the aerosol-cloud interactions be if 8 size bins were used, which is also available in the public release of WRF-Chem?

Page 27942, lines 2-4: It appears that the authors are using a version of the code that does not include secondary organic aerosols (SOA). Given this is often a large fraction of the aerosol mass, especially during the summer, what impact will have that omission have on the present results? Including more organic mass could reduce the average hygroscopicity of the particles and inhibit aerosol activation.

Page 27942, lines 15-22: At this point it was not clear what the motivation was for this particular domain. The time period is mentioned in the abstract, but I cannot find any mention of time period in Section 2. Another question is why the summer of 2009 is chosen? If one is interested in how changing emissions affect precipitation, one would think all seasons would be important to investigate.

Page 27942, line 25: A 3-day period is rather long one to use without data assimilation. Why not use a 2-day period? However, the relatively small domain may limit large errors in the synoptic fields through the boundary conditions. Some discussion regarding this aspect is warranted.

Page 27944, line 24: Figure 1a has a contour of 4 degrees, which is pretty large and will make the model look better than it really is. I do not doubt the model performs reasonably well, especially when examining time series such as in Figure 2. However, I think at a minimum a 2 degree contour interval should be used in Figure 1b.

Page 27945, line 10: The color contour in Figure 1b is biased towards the large values over the ocean where there are no observations. I suggest reducing the to 0.5 or 1 mm up to 5 or 6 mm so that differences between observed and simulated values can be seen more clearly.

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Page 27945, line 20: The authors mention that some stations have larger errors that may be due to grid spacing. What is leading the author to this conclusion? Are the stations located near land/water boundaries for example?

Page 27945, line 26-27: The authors should change “WRF-Chem convective scheme” to “Grell 3D convective scheme” to be more specific. They have not shown the performance of other convective schemes in WRF-Chem, so their sentence is implying there is a problem with all the schemes. Also, the errors may also be due to other parameterizations (land use, PBL, radiation, microphysics) as well that will influence the meteorology and affect precipitation. It is not clear why just the convection scheme is blamed here.

Page 27945, line 29: It would be useful to include a small panel on the right of each plot showing the diurnal average of the observed and simulated quantities.

Page 27946, line 1: I assume that in addition to the urban land use category, the fact that it is likely located close to a land/water boundary is another factor for making predictions there more challenging?

Page 27946, line 6: I cannot see the under-prediction in wind speed at night in this figure. Perhaps if a diurnal average were shown, this point would be more clear to the reader. I am not following why the PBL errors would contribute to the wind speed errors. Please be more specific.

Page 27946, line 27: I am quite skeptical of the authors reasoning with errors in the “treatment of radiative transfer (or photochemistry)”. It is equally possible that the simulated cloud cover (which have not been evaluated) is the problem, which would subsequently affect photochemistry. Have the authors allowed the convective clouds to affect radiation? If not, that could be a reason contributing to photochemistry errors. That is a point that should be included in the model description.

Page 27947, lines 1-6: I do not follow the logic regarding the emission processing and

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[Interactive
Comment](#)

the performance of the model in this paragraph. Having emissions using the simulated meteorology is obviously better than pre-defined emissions, but they have not done another simulation with pre-processed emissions to shown any change in performance. They seem to be suggesting that the current statistics for chemistry is similar to previous studies that use pre-processed inventories, so that at least they did not make the results worse.

Page 27947, lines 7-12: The model does not include SOA, so the inclusion of SOA would likely make the bias even higher (21-31% is not that bad, however) for April, May, and June and could improve the results for the other 2 months. The authors need to comment on how missing SOA affects these results. It is hard to know that the PM simulation results are sufficiently “small” as stated in the next paragraph when SOA is excluded.

Page 27946, lines 26-27: The thermal effects (which the authors here imply are from the differences in parameterized convective rain) here are due to changes resulting from both the direct effect and the aerosol effects on the resolved clouds

Page 27948, line 1: In Figure 4, what does the total in a single column mean? I suggest changing Figure 4 to have 3 columns per month. One column for observed precipitation and the other two columns for simulated precipitation (WRF and WRF-Chem) – divided into convective and non-convective precipitation.

Page 27949, line 6: The direct effect will affect both convective and non-convective precipitation.

Page 27949, line 13: Are the units for the column integrated PM correct? Should it not be $\mu\text{g}/\text{m}^2$?

Page 27949, line 15: It is clear that aerosols lead to cooling over the southern part of the domain, but how do aerosols lead to warming over the northern US and Canada?

Page 27949, line 26: Should “increase” be “decrease”?

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Page 27949, lines 27-28: I am not following the logic. Yes there is more non-convective rain in the northeastern US, but the decrease in convective rain in that region is not as strong as in the southern part of the domain.

Page 27950, line 9: Is the primary wind direction really east-north-east? For the whole five-month period? I would have guessed the primary wind direction is from the west to southwest. To they mean that transport is primarily towards the northeast?

Page 27951, line 9: Suggest dropping “correctly”. Yes the model is producing the aerosol indirect effect, but to say it is correct requires further observations (e.g. observed cloud droplet number) that the authors have not shown.

Page 27951, line 18: This statement could actually be proved by saving the aerosols (by size) removed by precipitation and analyzing those results.

Page 27951, line 26: Change “reproduce” to “produce”. To reproduce means the model was compared against some observations which is has not in this case.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27937, 2013.

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