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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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The manuscript by Mashayekhi and Sloan investigates the aerosol effects on different types of precipitation (i.e., convective and non-convective) in northeastern of North America by conducting sensitivity simulations using the online coupled air quality meteorological model WRF/Chem. They attributed the changes of precipitation to the radiative feedback for convective cloud and microphysical feedback for non-convective cloud. The title of the manuscript reflects the contents of the paper. The topic of the work is definitely of scientific interests to the Journal of Atmospheric Chemistry and Physics and may contribute to the scientific understanding of the aerosol impacts on regional climate. However, the paper could be written better. The necessity of improvements/revision in terms of both a robust simulation design, a more streamline discussion of results, and more in-depth analyses would require at least a major revi-

sion.

Major comments: One of my major concerns of this paper is about the simulation design to examine the effects of chemistry (majorly aerosols) on cloud then precipitation. The authors performed two sets of simulations using WRF/Chem: one with the full chemistry component and the other without chemistry and claimed that the differences between them would tell the impacts of chemistry (particularly aerosols) on precipitation. However, there's a major deficiency in the approach used here. In fact, the microphysics scheme (i.e., the Lin scheme in this work) of WRF/Chem will use the prescribed aerosols for cloud activation when there are no prognostic aerosols available from the chemistry module. Thus the differences between two simulations here can only tell the impacts of prognostic aerosols vs. prescribed aerosols instead of the "real aerosol" on precipitation. That's why it's not surprising that we see the overall increase of non-convective cloud precipitation over the domain which is conflicted with the 2nd aerosol indirect effect (more aerosols can lead to more and smaller CCNs and thus suppress the precipitation). So a more accurate way to simulate this effect is to do a 3rd simulation (which is what I would like to see in the revised paper) with chemistry but disable the aerosol emissions including precursors and aerosols related chemistry including cloud chemistry.

Another concern is about the model evaluation as also pointed out by the other reviewer. The authors only conducted the surface evaluation, which in my opinion is a little weak itself as compared to many other WRF/Chem studies. Only one site was chosen (the reason for choosing it is also not very clear to me) for the time series analysis. Wouldn't the evaluation be more robust if more representative sites (such as urban vs. rural and coastal vs. inland) are selected? Also why don't evaluate 1-hr or 8-hr max O3 and PM2.5 components which are routinely evaluated by most of air quality modelers? The model performance for higher altitudes (aloft) and for cloud-aerosol interactions is also missing. The satellite measurements such as MODIS can provide aerosol optical depth (AOD), cloud optical thickness (COT), and cloud condensation nulei (CCN) data and would be a great addition to the evaluation.

The authors explicitly mentioned in a few occasions that their results showed the impacts of anthropogenic aerosols on the precipitation. Does it mean that the simulation didn't include any dust and seasalt emissions? I failed to see any information regarding this. Please clarify.

Specific comments:

Page 27938, line 12-13: it's not surprising because aerosol-cloud feedback was not treated for convective cloud in the current version of WRF/Chem.

Page 27941, 1st paragraph: This paragraph is redundant and should be removed. The description should be put into the related subsections. For example, the part about emission should go to Sect. 2.2 if it's not covered there. The ones about the model setup should go to the corresponding section.

Page 27941, line 13-14: redundant and should be removed.

Page 27941, line 19: This version of model was released in 2012 and the references are very old ones. Please cite the more recent papers.

Page 27942, line 2-3: I don't think MOSAIC simulates methanesulfonate, carbonate, and calcium.

Page 27942, line 7-8: what are the schemes for thermodynamic equilibrium and aqueous-phase chemistry?

Page 27943, 1st paragraph: this paragraph is mainly for SMOKE and should be moved to Sect. 2.2.1

Page 27945, line 3: +1.27 C (40%). This bias is quite large for temperature and what could be the reason?

Page 27945, line 9: what do you mean by the stable? Stable boundary layer condition,

which I don't think should be the case? Please clarify.

Page 27945, line 12: "especially during the summer months". I don't think it can be told by Fig. 1b.

Page 27945, line 12-13: The reason doesn't seem to be right, since apparently model predicts more rain instead of less rain.

Page 27945, line 17-19: The plots for time series of precipitation should be added. Page 27946, line 2-3: It's confusing to me that the overprediction is due to the underprediction of nighttime T?

Page 27946, line 5-6: Again this statement is confusing how the positive bias is due to underprediction at night?

Page 27947, 1st paragraph: This paragraph is isolated and doesn't fit there. Please either remove it or move it to the more suitable section.

Page 27947, line 9-13: Figure 2 shows disagreement or large "spikes" instead of just well reproduced time dependence of PM2.5. The statement needs revision.

Page 27947, line 22-25: The content within the parenthesis is well known and should be removed.

Page 27949, line 11-13: How about the large areas with warming? What's the causes for it?

Page 27950, line 14-16: How is aqueous phase (cloud) chemistry treated in this study? Some background information should be provided.

Page 27952, line 10: The model performance is reasonably good, but I won't say it's very good based on the statistics table.

Table 2: It might be interesting to show the performance for the WRF run without chemistry as well to see how the feedback could affect the model performance.

Fig. 1: The stride of scales was too large for both T2 (every 4 degree) and preciptiation (3mm/day) and makes the results looking much better that they should be. Please use smaller ones.

Fig. 5b: The integrated mass in ug m-3? Shouldn't it be ug m-2?

Technical notes:

Page 27940, line 14: such an interactive

Page 27940, line 26: and with

Page 27941, line 17: NOAA/ESRL

Page 27942, line 2 and 4: "including" to "include"; delete "certain"; delete "are all included"

Page 27942, line 9: It should be "by Chapman et al. (2009)". There are similar typos throughout the paper and should be fixed as well

Page 27942, line 26: allowing for

Page 27944, line 17: the US.

Page 27944, line 23: "measured results" to "measurements" or "observations"

Page 27945, line 28: ":" to ","

Page 27945, line 29: #60430?

Page 27948, line 8-9: "probable" to "possible"

Page 27948, line 14: The highest

Page 27952, line 21, the prediction of convective precipitation

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