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Atmospheric Chemistry and Physics Discussions

Interactive comment on "WRF-Chem simulation of aerosol seasonal variability in the San Joaquin Valley" *by* Longtao Wu et al.

Anonymous Referee #1

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This paper examines the performance of a regional-scale chemical transport model in representing aerosol properties in the San Joaquin Valley over a one year period. The model is compared with surface measurements of composition and AOD as well as satellite measurements. The motivation for the paper is sufficient (although could be improved), but the main weakness is their approach and interpretation of the simulations. In addition, the paper is poorly written.

Major Comments:

The most important problem the manuscript has is how the model was configured to address the purpose of the study. WRF-Chem is a useful tool, but as with all models can only perform well when it is configured properly. The following is a discussion of items the authors should consider to revise and/or address.

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Domain and Dust Emissions: It is clear that the model domain is larger than the one shown in Figure 1. But it is hard for me to assess the importance of dust emissions since those are not shown. For local sources, dust is likely generated in the desert areas to the southwest of the SJV. It would be useful to show the emission regions from GOCART and DUSTRAN. My understanding is that the emission regions in DUSTRAN as implemented in WRF-Chem are rather ad hoc. They may depend on vegetation type. I suspect that dust is being generated locally in the SJV in DUSTRAN but not in GOCART. The authors mention how many grid nodes are used in the vertical direction, but should give an idea of the vertical resolution near the surface that will affect dust emissions. Dust emissions will depend in part on wind speed, and representing wind speed in California depends a lot on circulations affected by terrain. Both a fine horizontal and vertical resolution is needed to represent those winds that will affect dust emissions. It is not clear how well the model performed in winds – particularly over the dust emission regions. While some evaluation of the thermodynamic structure is given, there is nothing for the winds.

Boundary Conditions: The authors half the amount of aerosols from MOZART following Fast et al. (2014). But the errors in a coarse global model, like MOZART, will likely change in time and depend on meteorological conditions. There is no sensitivity results or evidence whether such a change in boundary conditions is warranted in the present study. I believe the version of MOZART the authors use prescribes dust using climatology which would affect the simulations over California. The potential errors in MOZART that will contribute to AOD over California will likely vary over a year-long period.

Simulation Period: On line 167, the authors state that the simulation period is from 2012 to 2013. There is no rationale as to why this period is chosen. Perhaps it does not matter and they are only looking at seasonal variations. But this are these seasons "typical" or not?

Anthropogenic Emissions: The authors use the 2005 NEI, but it would have been more

appropriate to use this 2011 inventory which is closer to the time of the simulation period. Even more ideal, would be to use emissions generated by CARB that are likely to have local emissions in California better represented. There are papers describing this inventory that at least be cited and the changes in SO2 and NH3 emissions in the SVJ valley (which are likely to be very different that the NEI 2005) will contribute to the nitrate and sulfate errors described in the paper. Since dust is an important factor over a large portion of the year, the differences in anthropogenic emissions are not likely to affect that conclusion. But it would affect the relative contribution of anthropogenic to natural sources over the year.

Model Evaluation: The authors used satellite equivalent potential temperature to evaluate the temperature profiles in the model. As seen in Figure 9, it seems that the vertical resolution is coarse so it is not the best source to examine near-surface temperature gradients. Two of the near-surface AIRS profiles look unrealistic to me. In addition it appears to have a 1 deg uncertainty (which is large for temperature) and is from a 1 degree grid – which will average out substantial temperature variations in areas affected by terrain. Using radiosondes would be a much better way to evaluate the model. The coarse vertical resolution of AIRS also leads to misinterpretations about boundary layer mixing. They claim that boundary layer mixing is too weak and explains why the simulated extinction profiles are wrong in AMJ and JAS. There is simply not enough aerosols around, no matter what the vertical distribution.

Missing Aspects: While the authors have evaluated simulated aerosol composition and PM25/PM10 mass, they have not examined aerosol water. During dry conditions of the summer months, this may not be a large factor contributing to extinction. Aerosol water is likely to become more important aloft, where RH is likely to be higher. But one does not know unless it is examined. Is there significant aerosol water in the simulations? Aerosol water will be influenced by simulated RH, so an evaluation of simulated RH is in order. A second missing aspect is SOA. I assume the version of MOSAIC they use does not include SOA. Yet SOA has been shown to be a major factor in PM25 for

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much of the year in California. While SOA concentrations will be lower than dust concentrations (when significant dust is present), it seems that omitting SOA is problem. One motivation factor in the study was related to using and air quality model (such as WRF-Chem) to guide emission control strategies. That would include OC emissions. But it seems that only primary OC is included, so that comparing simulated OC to observed OC is misleading. Also, MOSAIC simulates organic matter (both carbon and oxygen), so do the authors account for the missing oxygen parts in the measurements that are labeled OC? The authors also use a 4-bin version of the model which coarsely represents the aerosol size distribution. The authors should at a minimum discuss how this assumption affects their results and conclusions. It would have been useful to see some sort of evaluation of aerosol size distribution, since that also affects extinction and AOD. So the authors are really not probing all the aspects that affect uncertainties in simulated extinction and AOD.

Model Interpretation: All of the above factors will affect the interpretation of the model results and whether local (due to WRF-Chem) or long-range transport (not WRF-Chem related) sources of dust contribute to the errors in simulated dust concentrations and the vertical distributions. As stated in the summary, the authors claim the errors are largely due to errors in the dust emissions (not clear whether they mean local emissions or those from long-range transport) and vertical mixing. Given how the model has been used, they have not provided sufficient evidence to convince me that is the case.

Specific Comments:

Lines30-31: Change “in cold season” to “in the cold season” and similarly “in warm season” to “in the warm season”. This is the first instance of poor use of English in the text. I will not comment on other problems since I seem my role as commenting on the science, rather than correcting the grammar. The authors should use an editor if the co-authors are not willing to help out with the English.

Lines 43-45: This statement is an obvious one and I am not sure it is needed. The

focus of the paper seems to be on dust, so this is a secondary issue. Lines 92-104. This paragraph provides an important motivation for the study, but could be strengthened. Many readers will not know why models, such as WRF-Chem, are needed to develop/verify/modify satellite retrievals. It would be useful to add a few sentences describing how such models are used to demonstrate the purpose. Line 214: “averaging process” is a phrase that is not clear or specific enough. It is not clear how the authors apportion the NEI 2005 emissions to the WRF domain, and the procedure should be some sort of “reapportionment” rather than interpolation. Simple interpolation cannot be used since that would not conserve mass. Did they check to make sure the total mass emitted from NEI 2005 with the WRF domain was actually the same as what was used after the emissions were reapportioned to the WRF domains?

Line 257: The sensitivity experiment mentioned does not contain sufficient details for the reader to know why or how it was performed.

Line 264: The authors start discussion Figure 5c before 5a. Why not change the order of the panels then to match the progression of the discussion in the text?

Line 338: There are far more studies evaluation WRF-Chem in simulating biomass burning than simply the one the first author led.

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