Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-1083-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "WRF-Chem simulated surface ozone over South Asia during the pre-monsoon: Effects of emission inventories and chemical mechanisms" *by* Amit Sharma et al.

Anonymous Referee #1

Received and published: 12 January 2017

The paper describes uncertainty of modeled ozone to emission inventories of precursors generated by three different international effort. An evaluation of two chemical mechanisms MOZART and RADM-2 are also presented for one of the inventories. Results for April 2013 are presented. As presented it is a fairly unconstrained problem in terms of evaluation of the goodness of one emission field over the other purely based on ozone alone. I have tried to learn something new from the manuscript that I could have not guessed by just looking at table 1. They all have about the same NOX and HTAP has nearly 50% more NMVOC's than the other two emissions. If we are in a hydrocarbon limited regions (as it seems like most of India is) then HTAP will produce more ozone. I don't see the mystery in this conclusion. Fixing emissions to get the cor-

rect answer is patently wrong in a situation like here, where there so many physical and chemical process unknowns. (a) It would have been very useful if we could have some figures showing comparison between observed and measured hydrocarbon. I am sure, we will get the answer that there are not any. I would suggest that the group should collect some data on NMHC's to support this analysis if that were the case. (b) Where is the evaluation of NOX simulated at these sites? I have never seen a ozone evaluation paper that completely ignores the precursor observations and entirely based on ozone measurement. The comparison between MOZART and RADM-2 also hinges on an unknown in the model performance over India. I have seem a few papers on WRF from India that shows huge (+/- 1000 mts or more) differences in PBL heights by just using two different PBL schemes in the model. If MOZART is producing more ozone in the upper troposphere and is getting entrained into the PBL, where is the evaluation of PBL heights or entrainment rates in the study. (a) Why is MOZART producing more ozone in the upper troposphere than RADM? Is it because the photolysis rates used in RADM different than the ones used in MOZART? (b) I am guessing the photolysis code used for both RADM and MOZART are the same – but please check. (c) It seems like the ensemble based cloud scheme (GD) doesn't perform well over India. It has too much downward flux of air from the upper troposphere to surface. I recommend you try with a different scheme or carefully evaluate the UT/PBL fluxes in the model with observations.

(d) I have also noticed that lines 130/131 probably refer to spectral nudging and not really a FDDA. Do you have or assimilated any observational meteorological data from the Indian Meteorological Department (sondes, surface weather stations etc) to perform the FDDA?

(e) Performing spectral nudging to ERA probably is not a good idea, unless you can establish that it is a good representation of synoptic scale conditions over India during this period. Many instances (specially at 12 km resolution) it is better to run the model in data poor areas with model physics than nudging the entire wind profile to ERA or

any other reanalysis. Have you evaluated the model synoptic scale meteorology for the simulation period with any observations?

(f) Line 85/86 cites a paper that show the differences between simulated ozone is 4.5% with different emissions. Is the goal to improve upon that. I personally will be quite happy if you can predict ozone at less than 5% accuracy using a model.

(g) A Taylor diagram makes lots of sense when you are trying to find out which model (or model physics) is getting close to a reference point. Emissions by themselves have no real value and improving them is not really a model issue, more of an inventory developers problem. I don't see the point of this as the errors could be due to any number of physics or chemistry issues and not related to emissions at all. I can simply scale the HTAP emissions to a lower value and get closer to the other two emissions, that doesn't lead to a model improvement.

(h) The metric CH2O/NOy was presented in several figures. What am I supposed to learn from this? I am guessing the RADM scheme has no methane and MOZART has methane in its chemical trace list. How is NOy defined, does it include HNO3? The variability you see is most likely because of different loading of NMHC from each emission. Doesn't tell much about anything in my opinion.

(i) During this time of the year the atmosphere over the central plains in India is loaded with dust. What role does dust play in the ozone production / removal?

(j) The biomass burning identified has a major source of precursors also produces copious amounts of aerosols and in particular brown carbon. Brown carbon can change photolysis rates quite significantly and reduce ozone formation. How much of the disagreement is due to not accounting for these types of effects that are unique to India? We may have to fix these issues before trying to fix emissions. This only adds one more bad scientific processes to an already poor decision making in India for pollution control.

(k) Have you evaluated the water vapor in the model during these months. Does the error in water vapor in the model explain some of the differences?

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