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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The long-term trend and production sensitivity change of the U.S. ozone pollution from observations and model simulations" *by* Hao He et al.

Anonymous Referee #1

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The authors presented a study on ozone pollution trend and its sensitivity to key precursors in the US over 1990 – 2015. While the lack of measurement data of ozone precursors in time and space makes it difficult to study long-term trends in ozone chemistry, increasingly available model simulations may be in place for such analysis with caution exercised. This study represents such effort. Studies like this one are needed for information of changes in ozone chemistry due to climate change and anthropogenic emissions control. I have a few comments as follows.

The authors stated that, to avoid introducing inconsistency for model evaluation caused by "direct comparison", they used the EPA RSIG software to visualize the observed

and modeled ozone values. The question I have is how they quantified the difference between the modeled and observed values besides directly comparing the modeled value from a grid and the observed value(s) contained in that grid. Incidentally, what did "gridded" mean in "AQS station data were gridded to the CMAQ grid" in the Figure 5 caption? Didn't they just superimpose the station data on the CMAQ simulated distribution there?

The authors stated that in subdomains CMAQ performance exhibited large interannual variations (Table 2) and they further stated that their CWRF-CMAQ simulations showed improved performance in the Northeast and Midwest. It would be illuminating to the modeling community if they could expand on those two statements by explaining why.

The authors brought up an interesting point that for Baltimore and Denver the peak ozone increased in some years after 2002 although anthropogenic emissions were continuously decreasing in the past decades. From there they inferred that the increased ozone pollution on those areas "could be caused by other factors such as higher summer temperatures in certain years or enhanced stratosphere-troposphere exchange" (lines 234-244). Their Figure 4 showed the largest peak of fires emissions over a couple of years starting in 2002, which could have influenced ozone during those years. Also, were the summer temperatures really higher and STE enhanced during those couple of years? The authors might want to avoid making such sheer speculations if they had no intention to get into making these points.

I am a bit concerned with their threshold value of the O3/NOy ratio used to examine changes in the ozone formation regime. In our experience the model simulated O3/NOy ratio could differ greatly from the observed values and between simulations using different models. I am not sure if the threshold value of 15 from Zhang et al. (2009b), which used completely different models and had very different emissions and chemical environments, would be applicable. The authors need to find their own threshold value for their model simulations.

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