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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of synoptic patterns on surface ozone variability over the Eastern United States from 1980 to 2012" *by* L. Shen et al.

Anonymous Referee #1

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The authors investigated the effect of synoptic-scale weather patterns on observed MDA8 surface ozone over the eastern US during summers of 1980 – 2012. They did a fairly interesting study to be quantitative about the effect of the polar jet, the Great Plains low level jet (GPLLJ), and the Bermuda High on surface O3 in the eastern US by examining the relation between the three major EOF patterns and key meteorological variables followed by correlation between seasonal mean MDA8 ozone and variables representing those three synoptic systems. They constructed a model to predict seasonal MDA8 ozone averaged in four regions of the eastern US.

The time period of the data set used in the study needs to be clarified up front. The

authors stated that they were focused on the latest 20 years (1993-2012) at one point (line 2, page 13079). A few lines down they stated that they used data in the eastern US over 1980-2012 for the EOF analysis. When they examined the relation between the polar jet and surface ozone concentrations in Section 5, they used 1980-2012 (lines 23, page 13083).

How many AQS sites are urban and rural? Did the authors separate them and find results to be any different from all used? The bi-modal pattern was more pronounced in the CASTNET data than in the AQS data. If the urban data were removed from the AQS data set, how would the pattern of latitudinal variation look using the AQS data? Major urban areas are along the periphery of the eastern US, so would the greatest relative SD still occur there after urban data were removed?

It is hasty, without support evidence, to decide that larger ozone SD was a result of changes in synoptic meteorology, not anthropogenic emissions, when one looks at a 20 year dataset (Lines 23-26, page 13078; lines 1-2 on page 13080). If a season of data was examined, this statement would likely be valid. As we are all aware the annual 98th percentile mixing ratio of NOx has decreased by 46% in national average from 1990 to 2013, suggesting great reductions in NOx emissions. Over this time span, ozone SD could be fairly large due to emission reductions of this magnitude alone. In lines 24-29 on page 13079, the authors stated that a 2 ppbv discrepancy in ozone SD between the CASTNET and AQS data were likely due to the inclusion of many urban sites in the latter. If this was true, it would mean that the influence of anthropogenic emissions ozone SD was not trivial, which appears to contradict what they decided early on. Again, it seems necessary to remove the urban data from the AQS dataset to see if results would be different.

The EOF analysis suggested 24%, 11%, and 8% of the total variance of surface DMA8 ozone data can be explained the first three EOF patterns, which were hypothesized to be linked to the polar jet, Bermuda High, and the GPLLJ, respectively. Further the authors studied the relation between the JJA mean DMA8 data and variables repre-

senting the three systems and found good correlation. It should be noted that when they did this part of analysis, they used seasonal means. This means that the 53% of the total variance was in fact the portion on seasonal scales. Consequently the model they constructed (Eq. 1) was capable of predicting seasonal averaged MDA8 ozone mixing ratios. Therefore, it is an overstatement that using a single metric of the synoptic systems identified in the study they could predict ozone variability in future climate regimes. I think they might want to be more specific about the time scales and uncertainty of such prediction.

Section 8 is superfluous. It mostly repeats what was already stated in the preceding sections with very little discussion. I suggest that this section be shortened, including only key points and their important implications.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 13073, 2015.