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Interactive comment on "Adverse Effects of Increasing Drought on Air Quality via Natural Processes" *by* Yuxuan Wang et al.

Anonymous Referee #2

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While I like the research topic and design of this paper very much and think that it's long overdue for a comprehensive paper on understanding the effects of drought on air quality, some of the main statements are too broad. Several related issues were mentioned in the initial review. If changes were made, it would have made this review simpler.

I will use the abstract for illustrations. A statement such as "These enhancements show little sensitivity to the decreasing trend of US anthropogenic emissions. . ." is too strong. The decrease of sulfate has been very significant in the US. It is hard to believe that this decrease has no effect on an analysis of the effects of drought on sulfate (or PM2.5 in general). In this paper, there is no comparison of the general decreasing trend of sulfate due to emission reduction with the effects of drought to support the statement.

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Discussion paper

Another statement in the abstract “Most climate-chemistry models are not able to reproduce the observed responses of ozone and PM2.5 to drought severity, suggesting a lack of mechanistic understanding of drought effects on atmospheric composition.” The results from this paper show that there are deficiencies in climate-chemistry models. These deficiencies do not necessary imply that there are missing mechanisms in the models. In the discussion section of the paper (which I like better than the abstract), uncertainties in the models were described. It seems to me that the model deficiencies are more a problem of model representation of drought events not that there is clear evidence for missing climate-chemistry mechanisms.

The statement “Drought thus poses another aspect of climate change penalty on air quality not recognized before” would imply that there were no studies of the kind before. Wang and Xie et al. (2015), for example, obviously discussed some of the issues.

My understanding of the “lack of mechanistic understanding” that the authors referred to is that it is more an issue of how to diagnose the reasons that the model cannot reproduce the observed effects of drought on ozone and aerosols. Drought affects pollutant concentrations through meteorological processes represented by variables such T, RH, and wind speed. there are many papers discussing these “mechanisms”. Some are already referenced. But as I wrote in the initial review, the more relevant recent papers were not referenced. Zhang and Wang (2016) discussed the collinear problem in the correlations of ozone with T and RH (as was also seen in this paper) and ozone sensitivities to isoprene emissions. Ozone is much more sensitive to isoprene emissions in the fall than the summer. It is obviously relevant to the discussion of isoprene emissions in this paper.

Zhang et al. (2017) showed that ozone high extremes are more likely to co-occur with high T and low RH. But they also showed that PM2.5 high extremes co-occur with high T and low wind speed but do not depend as much on RH in spring and fall. Drought events have high T, low RH, and low wind speed. Therefore, a drought index, which is more related to RH and T than wind speed, is not the most optimal variable to define the effects of meteorology on PM2.5 in seasons other than summer. The model errors that the authors referred to may be related to model biases in wind speed simulations under high T and low RH conditions (i.e. simulated drought).

Based on Zhang et al. (2017), I suspect that ozone concentrations have a better correlation with a drought index than PM2.5 in spring and fall, even though the slope seems steeper for PM2.5 than ozone as a function of a drought index. This is obviously important in how the regression slopes can be used to infer the effects of drought. I already suggested in the initial review that the authors include a figure for the distributions of correlation coefficients of ozone and PM2.5 with a drought index (akin to Fig. 1). I make the same recommendation here.

It would take another paper to sort out all the details of why drought conditions lead to higher ozone and PM2.5 concentrations. That is not what I suggest that the authors do in this paper. But the relevant discussion suggested above should be included in the paper. Grouping data in summer with those in spring and fall is not a good choice (Zhang et al., 2017). Analyzing the data in summer and spring+fall separately will be much better. It may be a large amount of work, so I leave the choice to the authors. The authors may choose not to redo the analyses based on season. It is fine with this reviewer as long as the discussion of this seasonal issue is added.

References

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