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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Extremal Dependence between Temperature and Ozone over the Continental U.S." *by* Pakawat Phalitnonkiat et al.

**Anonymous Referee #3**

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The authors present an interesting study on the dependence of ozone and temperature extremes in both observations and model simulations. Particularly the methodology is innovative and expands beyond the standard tools in the field. However, as varies studies addressing this complex topic before, the present work is not able to provide clear answers and raises several questions while answering others.

Thus I recommend revisions and publication in ACP after the authors have addressed the comments below.

Major Comments:

1. The authors provide an elegant methodology to minimize the influence of year-to-year variability and seasonal effects. At the same time this procedure comes at

the cost of not considering all/many extremes on record. I am wondering about the effect the omission number a (here a=10) has on the results. How robust are the presented findings for values of a=5, a=15, a=1? Also I am curious if an approach omitting a certain number of extremes is more robust than more standard de-clustering approaches? It would be great if the authors could include a statement on this (or maybe an additional appendix) in the revised manuscript.

2. CAM4-chem is only one of the models contributing to the CCMI effort. In this light it would be great to see how other models that performed the REFC1SD experiment agree with observations. That said, I am not asking the authors to provide an in depth analysis across various models. It would be though interesting and provide important context for the community if the bias found for the model considered is ‘common’ in magnitude and sign across models and coherent across regions.

3. The authors use the set of CASTNET sites for comparison with model data, however we do not learn about which site selection criteria have been applied.

4. CASTNET data seems a robust choice to evaluate a global model for the US domain. However given the complications in the spatial correlation patterns of temperature and ozone I wonder if supplementing the observational data set with a suite of selected AQS sites might help. In the current analysis only 5 sites are used to cover the entire West, which raises robustness concerns.

5. The authors choose three sites (Ashland, Sand Mountain, Beaufort) to illustrate ozone temperature correlations. How have those stations been chosen from the CASTNET set, and would it be not more intriguing to pool stations for the spatial domains indicated in Fig. 2a?

6. The correlations reported are relatively low, which is acknowledged by the authors. I am wondering though if it would not be worthwhile to report explained variance throughout the manuscript instead of correlation coefficients.

7. The key conclusions of the manuscript should be highlighted and also included in the abstract. Right now the language is a little vague regarding the significance and robustness of the overall extremal dependence. Despite all caveats raised in the discussion section, the study suggests a weak but robust relationship between ozone and temperature extremes. At the same time the spatial mismatch between regions where high ozone and high temperature extremes occur is of relevance and will motivate future modelling work.

Technical Comments:

Title: "Extremal dependence ... " might not be very accessible for non-statisticians

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