

# ***Interactive comment on* “The mechanisms and meteorological drivers of the ozone–temperature relationship” by William C. Porter and Colette L. Heald**

## **Anonymous Referee #2**

Received and published: 21 May 2019

**\*\* GENERAL COMMENTS\*\*** This paper by Porter and Heald investigated an interesting topic, i.e. the processes able to explain the relationship between O<sub>3</sub> and surface temperature during summer season in North America and Europe. This relationship is particularly important as a potential metric to define the impact of climate-change to future O<sub>3</sub> mixing ratios as well as the required precursor emission mitigation. The topic is well on the focus of ACP and it can be of interest for a wide audience.

Some sections must be clarified before publication. While the discussion of the sensitivity of O<sub>3</sub>/T regression as a function of the four main processes (BIO, DEP, SOIL, PAN) is rather clear, the application and discussion of the so-called “commonality anal-

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ysis” is rather confused. I would suggest to describe more in details this methodology in the Section 3, and the providing results in Section 4. Basically, it is not clear what this “commonality analysis” is and how it is performed, actually. References to other study can help the reader. It is not clear how the “unique” and “shared” contributions have been calculated.

A second point that deserve more discussion by the authors is related to the spatial dependence of the agreement between model and observations (figure 4) . How these evident disagreements affect the final results and their interpretation/robustness?

**\*\*SPECIFIC COMMENTS\*\*** METHODOLOGY The model resolution cannot prevent some wrong representation of processes (e.g. emission near urban area or coastal regions). Please comment on this and if available provide references about comparison of model performance with near-surface observations.

Did you perform any basic data check on AQS or AirBase database (e.g. detection of outlier)? Did you try to categorize (especially for the comparison with model) the measurement stations as a function of their altitude, sub-region (e.g. South Europe vs Northern Europe) and type (e.g. urban, rural, remote)? All these attributes can be important when discussing O3-related processes

Pag 5: The description of the methodology from line 9 to 14 is not clear .

RESULTS AND DISCUSSION Pag 6: what ” OLS” is?

Figure 4. Significant mismatch exist between model and stations (e.g. over central US).Please better discuss if (and how) you considered this when commenting your results. For Europe, it is almost impossible to appreciate the model results due to the high coverage of stations. Is there any dependence of bias or correlation between model output and observations as a function of station type/elevation?

Figure 9: in the text a “grey” fill is mentioned. In the plots only blues and black are visible. Please improve the colors. Also make the caption more clear: it’s difficult to

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couple the figure with the text. In general the discussion of Figure 9 is confusing for the reader. Please, start describing what the figure reports (double check with caption) then comment results.

Pag.10: line 20-22: something appears missing in this sentence.

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