**Dear Editor Dr. Benjamin A Nault,**

**Thank you very much for handling our manuscript. Below, we provide our detailed responses to the reviewers' comments along with the revised manuscript, which includes all the changes made in response to the reviewers' suggestions. We have addressed every comment raised and incorporated the necessary revisions.**

**Thank you for your consideration.**

**Sincerely,**

**Shuai Li et al.**

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**Report #2**

**Comment [2-1]:** The authors have addressed the reviewer comments appropriately. I have minor comments to further improve the clarity of the paper.

**Response [2-1]: We thank the reviewer for the positive and valuable comments. All of them have been implemented in the revised manuscript. Please see our itemized responses below.**

**Comment [2-2]:** L46: change air stagnancy to air stagnation and remove enhanced before ventilation since the sentence refers to phenomena rather than the direction of change.

**Response [2-2]: Thank you for pointing it out. We have corrected them accordingly.**

**Comment [2-3]:** L68: it would be helpful to be consistent throughout the manuscript on the terminology: relationship versus sensitivity

**Response [2-3]: Thank you for pointing it out. In this study, we aim to use the ozone-temperature**

**relationship to represent the overall association between ozone and temperature, including their correlation ($r_{\Delta O3\text{-}\Delta Tmax}$) and ozone-temperature sensitivity ($m_{\Delta O3\text{-}\Delta Tmax}$). In contrast, ozone-temperature sensitivity specifically refers to $m_{\Delta O3\text{-}\Delta Tmax}$. We have carefully reviewed and corrected the use of "relationship" and "sensitivity" throughout the manuscript to avoid any potential misunderstandings.**

**Comment [2-4]:** L91: please clarify if AQS dataset provides "surface" temperature measurements.

**Response [2-4]: We change the description: "<u>The AQS dataset also provides surface temperature measurements that could be ideally used in quantifying the ozone-temperature relationship at individual sites.</u>"**

**Comment [2-5]:** L157-159: NOx may have low solubility but wet deposition of its oxidation products (e.g., HNO3, PAN) have implications for ozone.

**Response [2-5]: We agree. Accordingly, we have revised the relevant description in the manuscript as follows: "<u>$NO_x$ and ozone have low solubility, but wet deposition of $NO_x$ oxidation products may further influence ozone. We do not separately consider temperature's indirect influences on ozone through wet deposition processes in this study.</u>"**

**Comment [2-6]:** L160-161: The trend analysis period is 1990-2021 while the simualtions are done for 1995-2017. Please provide a specific reason or the shorter time priod for model simulations.

**Response [2-6]: Thank you for pointing this out. We have added the following text in Line 157 "<u>We do not extend the simulation to earlier or later years due to lack of reliable anthropogenic emission inventory by the time when this study was designed.</u>"**

**Comment [2-7]:** L162: replace closed with close

**Response [2-7]: We have corrected them accordingly.**

**Comment [2-8]:** L179: "others" communicates vagueness. Please be specific.

**Response [2-8]: We change the relevant text in the manuscript: "<u>including natural emissions of BVOCs and soil $NO_x$, the chemical kinetics, dry deposition, and other mechanisms that may have minimal impacts on ozone.</u>"**

**Comment [2-9]:** L198: 1995E-AVOCs should be 1995EAVOCs. Figure 3b: Spatial is misspelt in the title near the top.

**Response [2-9]: We have corrected them accordingly.**

**Comment [2-10]:** Figure 5: How are the NOx emissions reported - kg NO or kg NO2?

**Response [2-10]: Thank you for pointing this out. We use kg NO to describe NOx emissions, and to avoid any confusion, we have changed the unit of NOx emissions to kg N in Figure 5.**

**Comment [2-11]:** L354: which version of CEDS? Is it the same used in this study?

**Response [2-11]: To ensure consistency in the study, all anthropogenic emission inventories used in this study are based on CEDS v2021_04_21 (O'Rourke, 2021).**

**Reference:**

O'Rourke, P. R., Smith, S. J., Mott, A., Ahsan, H., McDuffie, E. E., Crippa, M., Klimont, Z., McDonald, B., Wang, S., Nicholson, M. B., Feng, L., & Hoesly, R. M. (2021). CEDS v_2021_04_21 Release Emission Data (v_2021_02_05) [Data set]. Zenodo. https://doi.org/10.5281/zenodo.4741285

**Report #3**

**Comment [3-1]:** The authors have provided reasonable responses to all the questions and comments raised during review and I find the manuscript suitable for publication.

**Response [3-1]: We thank the reviewer for the positive and valuable comments. All of them have been implemented in the revised manuscript. Please see our itemized responses below.**

**Comment [3-2]:** I would request two further small clarifications on the description of the spin-ups.
1. The text is not perfectly clear about the initial condition for all the simulations that start on June 1. Does the text on lines 156-157 mean that the IC for every June 1 simulation (for every year) is July 1 of 2005? If so, could the authors please specify that the same initial condition is used for every simulation?

**Response [3-2]: In this study, all simulations use the same initial conditions. To clarify this, we have added the following description in Section 2.4 of the manuscript: "The initial chemical fields are close to conditions for July 2005 (the same initial fields used for each set of sensitivity experiments)"**

**Comment [3-3]:** 2. Line 165: while the average difference was small, Figure S2 shows some regional increases and decreases of ~+/-1 ppb in O3 and +/-0.3 ppb/K in O3/T. These are modest, but not negligible differences and the authors should clarify that the 2.3% average difference includes more significant regional increases and decreases.

**Response [3-3]: Thank you for pointing it out. We agree that a shorter spin-up time increases uncertainty for specific regions. To clarify this, we have added the following description in Section 2.4 of the manuscript: "The average differences between the two simulations were only 0.3% for ozone concentrations and 2.3% for $m_{\Delta O3\text{-}\Delta Tmax}$, with high spatial consistency ($r > 0.99$). This confirms that using a 1-month spin-up time for the simulation should not affect the analysis and conclusions. However, for specific regions, more noticeable differences in ozone concentrations and $m_{\Delta O3\text{-}\Delta Tmax}$ exist between the two simulations. A longer spin-up time is favorable for generating global chemical fields when sufficient computational resources are available."**