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Need observational constrains on the simulated ozone-vegetation-meteorology interactions

Anonymous Referee #2

Referee comment on "Effects of ozone–vegetation interactions on meteorology and air quality in China using a two-way coupled land–atmosphere model" by Jiachen Zhu et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-165-RC2, 2021

Major comments:

1. The study use a regional chemical transport model (WRF-Chem) with a revised ozone-damage scheme to explore the sensitivity of meteorology and ozone air quality to ozone-vegetation interactions, specifically, ozone damage. The authors discussed that most of the model sensitivity results presented in this study are broadly consistent with the results from the earlier studies (e.g., Sadiq et al., 2017). It is not clear to the referee what the novelty of this specific research article is compared to the earlier studies. Furthermore, the ozone-vegetation interactions and meteorological responses discussed in this study appear to be purely based on model sensitivity experiments, thus missing critical observational constrains. The authors conducted some evaluation of surface meteorology, ozone and related chemical tracers averaged over entire China from their base (?) simulation, but there are no evaluation and discussion regarding how the introduction of ozone-vegetation interactions in the model improves the simulation of ozone air quality and surface meteorology. The model sensitivity results will be much more trustworthy if the authors could demonstrate that the new model with ozone-damage substantially improves the simulation of observed ozone interannual variability and mean distributions, at least over the areas where the ozone-vegetation interactions are largest.

2. From Table 4, it appears that the model not only has large mean-state ozone biases and but also have difficulty simulating the observed ozone interannual variability. For example, observations are lowest in JJA 2014 and highest in JJA 2017. The model does not capture this variability at all. It is not clear from the text and table captions as to which model they are evaluating, the old model without ozone damage, or the new model with ozone damage? Does the new model with ozone damage better simulate the observed high-ozone summer and extreme events? If not, why shall we care all the sensitivity results discussed in the paper? Also in Table 4 and Fig.8, are you showing JJA average of 24-hour mean ozone or daily maximum 8 hour average ozone (MDA8)? Since the effects of ozone damage via stomatal uptakes are expected to be largest during daytime, the analysis should focus on daytime or MDA8 ozone, not the 24-hour average.

3. From Figure 3, it appears that changes in vegetation properties due to ozone damage are most prominent in areas with sparse vegetation, such as north and northwest China. Why? The authors report the large percentage change in the abstract, but this could be misleading, as the large percentage change could be the numerical artifact from dividing a small value.

Other comments:

1. Lines 50-70 and 95-115: there are a few recent papers demonstrating the significant impacts of reduced ozone removal by drought-stressed vegetation on observed surface ozone trends and extremes. These papers can be discussed here for a complete literature review:

Huang, L., McDonald-Buller, E. C., McGaughey, G., Kimura, Y. & Allen, D. T. The impact of drought on ozone dry deposition over eastern Texas. *Atmos. Environ.* **127**, 176–186 (2016).

Lin, M. et al. Sensitivity of ozone dry deposition to ecosystem–atmosphere interactions: a critical appraisal of observations and simulations. *Glob. Biogeochem. Cycles* **33**, 1264–1288 (2019).

Lin, M., Horowitz, L.W., Xie, Y. *et al.* Vegetation feedbacks during drought exacerbate ozone air pollution extremes in Europe. *Nat. Clim. Chang.* **10,** 444–451 (2020). https://doi.org/10.1038/s41558-020-0743-y

2. Lines 155-160, clarify you are using monthly mean chemical boundary conditions from MOZART?

3. Simulation years should be clarified in Section 2.1

4. Section 2.2:

(1) This section should include information on the fraction of sunlit and shaded leaves as well as the fraction of dominant vetation types considered in the model. Fig.4 fits better in this section.

(2) It is not clear from the text whether the authors implement a new ozone dry deposition and damage/feedback scheme in the WRF-Chem model. Does the simulated stomatal resistance respond to soil moisture deficits? According to several recent papers listed above, stomatal closure induced by soil moisture deficits can substantially increase surface ozone concentrations; this process is an important part of the ozone-vegetation interactions. The default Ball-Berry scheme does not include the effects of soil moisture. The default Wesely dry deposition scheme used in WRF-Chem does not consider the effects of soil moisture, neither (e.g., Rydssa et al., 2016).

Rydsaa, J. H., Stordal, F., Gerosa, G., Finco, A. & Hodnebrog, O. Evaluating stomatal ozone fluxes in WRF-Chem: comparing ozone uptake in Mediterranean ecosystems. *Atmos. Environ.* **143**, 237–248 (2016).

The role of soil moisture should be clearly discussed and clarified in the manuscript.

5. Tables 3 and 4. The evaluation should be done by the different parts of China, according to ozone pollution conditions, meteorological regimes, and vegetation types., and tied closely to the model sensitivity experiments, as discussed in my major comments.