

Interactive comment on “Effects of ozone-vegetation coupling on surface ozone air quality via biogeochemical and meteorological feedbacks” by Mehliyar Sadiq et al.

Anonymous Referee #1

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INTRODUCTION:

This paper discusses the impacts of ozone on ecosystems in the framework of a modelling study. An ozone plant damage parametrization is implemented in CESM and fully coupled to the land surface and atmospheric composition components. The consequences of ozone ecosystem damages is investigated with regards to ozone surface concentrations, i.e., from an air quality perspective. A systematic analysis of ozone surface concentrations, dry deposition velocities, stomatal resistances, surface temperatures, latent heat fluxes, etc. is presented.

GENERAL REMARKS

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The effect of ozone on ecosystems and its consequences for ecosystem productivity, dry deposition fluxes and ozone surface concentrations has been a hot topic for some time. This is mainly due to the sizeable impacts that are expected on air quality and climate. Ozone can have a substantial control over the removal and/or emission of key atmospheric compounds such as biogenic VOC, ozone itself or water vapour. In the latter case, the impact on evapotranspiration is critical to the climate on a local to regional scale and the short term. This is compounded by the control of ozone over the carbon cycle through damages to photosynthetic productivity which has consequences for climate on the large scale and the long term. For these reasons I consider the paper a useful and interesting contribution to the current discussion. It may not be the absolute first study to apply fully coupled ozone parametrizations (c.f., e.g., Pacifico et al., 2015) but it is certainly amongst the pioneering studies and looks at it from a particular angle.

Largely, the paper is well written, accessible and nicely structured. In general, the figures are quite helpful and complement the text. There are a few changes I would recommend, though: 1) In case of figures 3, 5, 6 and 7 I would prefer to see relative changes rather than absolute changes. Maybe these figures could just be amended by plots of relative changes. 2) figure 4 does not seem to be discussed anywhere in the text. 3.) In case of figure 6 b I am not quite sure I understand the diagnostic. It seems to me this is some kind of semi-offline approach. Why is the "change in ozone concentration per unit change in dry deposition" required from another study? Is it not possible to diagnose the ozone change directly from the model simulations? At least, this should be explained more clearly.

For each experiment about 15 years of data are available, yet the authors only discuss means. There is a good opportunity to study inter-annual variability and add some statistics with regard to statistical robustness and significance of the findings. This is a somewhat serious omission in the current version of the paper which is the main and almost only reason why I recommend major revisions. However, this should be quite

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easy to do.

SPECIFIC COMMENTS

p2l66-69: the number for the direct ozone RF of 0.89 W m⁻² to me seems excessively high. If I recall well, the IPCC Assessment Report 5 quotes an ozone RF +0.35 (0.15–0.55) W m⁻². Can you please clarify this number.

p4l146: "... , and W_e is the export-limited rate is W_e."; please revise

p4l158: the "cumulative ozone uptake" is abbreviated with CUO, however, I would find COU the more immediate choice. Or, if CUO is preferred then describe it as "cumulative uptake of ozone".

p5l162: "computed from CAM-Chem at every time step"; please revise

p5l164: "... when vegetation is most vulnerable to ..."

p7l238-239: the authors argue that "Given the sound theoretical and empirical basis of ozone damage on vegetation, this further highlights the urgency to revise other model processes and modules relevant for ozone simulations." However, I think this high confidence in our understanding of the ozone-ecosystem effects is not yet justified and I would not be prepared to modify other process models based on these findings. I think much more research on process understanding is needed and I would like to see this statement removed.

p8l275-277: As mentioned above in connection with figure 6, I don't quite understand the line of argument here. Why can the ozone impact on its own deposition not be diagnosed directly from the model simulations? Please explain further.

RECCOMENDATION

I recommend to reconsider the paper after major revisions. As I already stated above, this is only to include some analysis of the results with regard to statistical significance and robustness and/or some uncertainty analysis.

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