

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2020-1264

Anonymous Referee #2

Referee comment on "Indirect contributions of global fires to surface ozone through ozone-vegetation feedback" by Yadong Lei et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1264-RC2>, 2021

This paper examines the effects of fires on surface ozone pollution and the subsequent feedback effects that may further enhance ozone. This runs along the excellent work this group of researchers have done demonstrating the importance of ozone-vegetation interactions in atmospheric chemistry modeling and air quality projections. While the idea of ozone-vegetation feedbacks is not new by now, this paper presents a new perspective by focusing on fires, which distinguishes itself from previous ozone-vegetation papers that focused on anthropogenic emissions. There are however several aspects which I believe need to be addressed, and revisions need to be made, before this paper can be published. Please see below for my comments and suggestions.

P4 L68-74:

First of all, it should be "vegetation damage" that would influence the sources and sinks of ozone via various "feedbacks". Second, the authors mentioned the distinction between "biogeochemical" and "biogeophysical" feedbacks, but it needs to be explained further. What are the distinctions? In particular, in the following few sentences, only "biogeochemical" processes are considered, but the "biogeophysical" pathways are not mentioned at all.

In general, the whole introduction lacks a thorough illustration of the detailed feedback pathways and the distinctions between the biogeochemical and biogeophysical effects of vegetation on air quality (and thus feedbacks after ozone damage). I suggest having a separate paragraph detailing first how vegetation processes affect ozone air quality, distinguishing between the biogeochemical (i.e., BVOC emissions and dry deposition) and biogeophysical (i.e., transpiration and the subsequent changes in meteorological

environment) pathways. A paper that can be referenced on these is Wang et al. (2020). After such an introduction, the feedback effects can be explained much more clearly.

P5 L89-90:

In Sadiq et al. (2017), much of the positive feedback is due to “biogeophysical” effects, i.e., reduced transpiration leading to higher surface temperature and thus higher isoprene emissions, then higher ozone. Reduced dry deposition velocity is roughly only half of explanation. In general, in this whole paragraph, the distinctions in methodology or pathways included should be explained more clearly. E.g., Zhou et al. (2018) and Gong et al. (2020) only considered biogeochemical effects, because in their models, climate was not dynamically simulated, whereas Sadiq et al. (2017) considered both effects because their model dynamically simulated climate. Moreover, a fourth study (Zhu et al., 2021) that focused on China is currently under review.

P5 L100:

A better justification is needed here to illustrate why this is important to look at. It’s unquantified, but do we really expect the ozone-vegetation feedback via fires is really gonna be important? Any justification for this expectation (and thus the motivation of this paper)? Any comparison with previous work regarding the magnitude of the potential effects?

P8 L160:

The setting of this model using prescribed meteorology needs to be emphasized and contrasted with fully coupled climate-chemistry-vegetation models such as CESM. It should also be emphasized that this model setting only addresses the “biogeochemical” effects, not “biogeophysical” (referring to the points made above).

P9 L181:

Does YIBs actually simulate a multi-layer canopy, instead of a big-leaf canopy? This needs to be clarified. If a multi-layer canopy is represented, the number of layers and other canopy parameter setting needs to be clarified. If not, this line here should be corrected.

P11 L230-241:

An obviously missing element in their model setting and experiments is that fires also damage LAI and canopy height directly, which may only happen only where fires happen but would be the dominant effect (other than ozone damage on plants) there. Fires also influence the long-term recovery and growth of the forests, which of course would also influence ozone. I understand that such an effect is more localized to the forested areas while the ozone-vegetation feedbacks can occur downwind of the forests, the lack of consideration of this necessary pathway should be explained upfront early on. Indeed, this should also be discussed as early as in the introduction.

P13 L285:

It should be clarified that the reductions are consistent with studies/models that used the same ozone damage scheme. It should also be mentioned that some other studies, using other ozone damage scheme, e.g., the Lombardozzi scheme (Zhou et al., 2018; Zhu et al., 2021), may find quite different ozone-induced reductions in GPP.

P14 L303:

The reduction in stomatal conductance mainly follows reduced photosynthesis – this should be clarified. This is obviously missing some newer physiology that people have found recently, e.g., the sluggishness of stomatal responses after ozone damage (Huntingford et al., 2018) that may cause the stomata to be more open under ozone exposure than otherwise. Such missing element needs to be discussed.

P17 L354:

Why does fire emission cause larger ozone-vegetation feedbacks than non-fire sources? It needs to be explained.

P17 L359-363:

The rationale behind needs to be explained in greater detail as well.

P19 L406-409:

This is also related to my comments on P11 L230-241 above. Fires do not only affect BVOC by burning vegetation, it also reduces LAI and the long-term recovery and growth of the forests, thus affect the whole ozone-vegetation interactions in the long term. When a forest is burned, the reductions in LAI, dry deposition, transpiration and BVOC emissions can have effects that last for many years, and this temporal perspective is entirely missing from the current discussion. A more thorough discussion on this missing element, and the implications on the validity and significance of this paper's results, is warranted.

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