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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Photochemical environment over Southeast Asia primed for hazardous ozone levels with influx of nitrogen oxides from seasonal biomass burning" *by* Margaret R. Marvin et al.

Anonymous Referee #1

Received and published: 18 November 2020

Recommendation: Return to author for minor revisions Journal: ACP Title: Photochemical environment over Southeast Asia primed for hazardous ozone levels with influx of nitrogen oxides from seasonal biomass burning Author(s): Margaret R. Marvinet al. MS No.: acp-2020-886

General comments The study explores impacts of seasonal biomass burning on ozone production using GEOS-Chem along with the OMI data and the emission and fire inventories. The fire inventories reveal two interesting biomass burning regimes. Three indices derived from the GEOS-Chem model are used to investigate the mechanisms

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of ozone formation with a focus on the two seasonal biomass burning regimes. Uncertainties associated with the inventories and assumptions are properly discussed. The presented results show interesting findings in terms of anthropogenic and biogenic emission interaction and its impact on air quality. If there is a weakness, it is my opinion that it relates to the calculation and interpretation of the indices. I believe this concern can be addressed through moderate revision.

Specific comments 1.-Ozone production rates. Can you explain what RO2 are included in Equation 1?

2.-Ozone sensitivity to NOx and VOCs. The organic nitrates can be important in an isoprene-dominated area under high NOx conditions. Can you roughly estimate the impact of organic nitrates? In addition, the organic peroxide can also be a sink for HOx radicals. Can you also quantify the contribution from the organic peroxides if possible? Bates et al. (2019) updated the isoprene mechanism in GEOS-Chem, which includes important updates on isoprene nitrates. It might be helpful to test it with the new isoprene chemistry, but probably not essential for this study.

3.-Ozone formation potential. The authors did not filter the OFP for potential NO-limited regions and acknowledged the associated uncertainties. In Figure 9, how different it would be if you exclude the potential NO-limited regions?

4.-PBL ozone. PBL-averaged values of the three indices are used here. However, the simulated PBL-averaged O3 is not justified, as OMI provide tropospheric O3 and the ground measurements do not seem to cover the two regions of biomass burning. Is there a way to justify that the PBL O3 is well represented by the model in the two regimes of interest?

5.-The three indices are focused on the chemistry of ozone production in the PBL. However, other physical processes could be important for PBL O3 too as pointed out by the authors in the last Section. Figure 6 shows a glimpse of those processes for tropospheric O3. I wonder their relative importance to chemistry for PBL O3. To be

specific, I know convection is an important source of surface O3 in Amazonian region due to the active MSC. Therefore, I wonder if that's true for this tropical region too and how it could potentially affect the results of O3 chemistry in this study.

Minor comments 1.-Can you add latitudes & longitude for Figure 1b-e

2.-Line 212. Can you clarify the source region of the transport?

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