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Interactive comment

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 #2

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This manuscript investigated the impacts of biomass burning on the air quality especially ozone pollution in Southeast Asia. Using GOES-Chem model as well as satellite observations, the authors found that VOC-limited ozone production exists in the burning areas, while outside the burning areas NOx-limited regime dominates. This study also estimated the impacts on public health due to the biomass burning emission.

The text is concisely written and well documented. The topic is applicable for the Atmospheric Chemistry & Physics journal. However, the current manuscript lacked detailed discussion and needed more analysis (please see the remarks below). My main concern is, the tropospheric ozone pollution is determined by multiple factors such as surface temperature, precipitation, and emissions (anthropogenic, biogenic, and pyrogenic for Southeast Asia). The current manuscript identified two major episodes when pyrogenic emissions could play an important role in two regions, i.e., peninsular mainland in March and Indonesia in September. As discussed in the remarks below, the high ozone concnetrations are found not only in and near the biomass burning areas, but also in other regional far away from the fire activities. So it is difficult to conclude that the high ozone events are caused by the pyrogenic emissions. One sensitivity experiment without GFED emissions is necessary to simulate the ozone pollution without biomass burning, and the differences between these two GEOS-Chem runs can better demonstrate the impacts of fire activities.

In summary, the current manuscript shows important results but need further work. Major revisions as indicated in the comments and remarks below are needed before consideration of publication in ACP.

Detailed Remarks/Suggestions for Revision

Line 206: I am confused about the Figure 5 and the bias correction with respect to an ozonesonde ensemble. My understanding is that the authors applied the OMI averaging kernels (AK) to sonde profiles to calculate ‘profiles’ retrieved by OMI. Te GEOS-Chem model should generate true concentration profiles, so I don’t think it is proper to compares these two. Should the OMI AKs be applied to the GEOS-Chem profiles and them be compared with OMI retrievals?

Line 213: Any details about how the flux especially transport is calculated? Through the lateral boundaries of the nested high resolution GEOS-Chem domain?

Line 214-218: Is the ‘mean annual’ value calculated through averaging the domain? Can the authors show the annual cycle in a table or a figure?

Line 219-225: From Figure 7, it is hard to conclude that the GC simulated high ozone columns in the high latitude, i.e., peninsular mainland, are correlated to the biomass burning. For instance, over the ocean area between Hainan Island and Taiwan, there are also high ozone columns in both GC simulations and OMI observations in March. To me, the high ozone columns in the high latitude are more regional nature of chemistry and climate, and cannot be explained by the biomass burning. If the biomass burning dominates, it cannot explain why oceans in both side of the peninsular mainland have high ozone when only one side is the downwind regional of the biomass burning. The Sept case has the similar results. GC did not simulate the high ozone columns over or near the locations of biomass burning (Fig. 2d). Lastly, the difference plots show mixed high and low bias, both of which are large. Does it mean GC has problem to simulate the ozone columns in this region? A control run without GEFD emissions may be helpful here to identify the contribution from biomass burning.

Line 373: What kind of population data were used in this study? Since the GEOS-Chem simulated concentrations at gridded domain, it may be more proper to apply the gridded population products such as the gridded NASA SEDAC global population products (https://sedac.ciesin.columbia.edu/data/collection/gpw-v4).

Line 387: Is possible to add the GC simulated ozone concentrations as background in a) and b)? Hard to tell if GC capture the pattern of ozone when comparing Figures 10 and 11 which do not have the same map ratio.

Figures

Figure 2. '(b-e)' should be '(b-c)' for Spatial distribution of dry matter emissions?

Figure 7. What are the resolution of GC and OMI? If they are gridded, why not show contours? The current plots look like there are lots of gaps.

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