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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Organic pollutants from tropical peatland fires: regional influences and its impact on lower stratospheric ozone" *by* Simon Rosanka et al.

Anonymous Referee #1

Received and published: 31 December 2020

Rosanka et al., 2020 presented a modeling study to quantify the impact of Indonesia peatland fire burning on atmospheric chemical composition. Overall, I found this an interesting study with well-designed model simulations. Use model sensitivity simulations to address the impact of aqueous phase chemistry of fire emissions is a somewhat new area that haven't been discussed much in literature. However, the paper in its present form needs some major improvements, including better figures, more in-depth analysis and uncertainty discussion. These concerns need to be addressed before the paper be accepted for publication in ACP.

1. Title of this paper, in my view, is not accurate in describing the content of what's

presented in this work: i) this study primarily targets the Indonesia peatland fires, the use of word “tropical” implies the entire equatorial tropical band, which is misleading to some extent even though I understand a large fraction of the tropical peatland fires are from SE Asia, ii) most of the results from this work focused on the impact of peatland fires on tropospheric VOC, NOx, OH, O3; the impact on lower stratospheric ozone is just a minor component of this work and I am not convinced the impact is important nor statistically significant compared to the potential dynamical variability. Hence the use of “. . . its impact on lower stratospheric ozone” is not appropriate.

2. The paper is in general well written. However, I did find many grammatic and editorial mistakes during reading. I tried to include most of these down below, but I am sure there are more places need correction. I would strongly encourage the authors have a full editorial read of the manuscript before resubmission.

3. L68-L90. I think this is a very helpful paragraph and a useful discussion in putting the Indonesian biomass burning in the context of other biomass burning emissions regions around the globe. But I am not sure it belongs to an “Introduction” section. Is it possible to move it to the main results section? If you are strongly inclined to keep it in the introduction, this section will probably flow much better if you move this paragraph before para 2 (Asian Monsoon).

4. Figure 4 and the related discussion of model bias in simulated HCN in Section 3. The minimum and maximum values used for the color scales not adequate for readers to assess model performance. For example, IASI column saturates at 2x10ˆ16 while the model FIR run high bias over the same region also saturates at +2x10ˆ16, and the model REF run have negative biases that also saturate at -2x10ˆ16. You need to increase the saturation values before the readers can reasonably assess the model performance and understand the magnitudes of emissions biases we are looking at over Indonesia. Without a properly done Figure 4, I find the discussion in section 3 hand waving and not convincing. With that said, with the difference between REF and FIR roughly two times the observed HCN amount over Indonesia, I strongly suspect

the emissions and/or emission factors used for biomass burning being highly biased. Although CO is not a unique tracer to biomass burning, since (a) remote sensing measurements of CO is available from multiple sensors and retrievals are available with much higher precision, (b) SE Asia during biomass burning season is most likely dominated by CO from biomass burning, I would strongly encourage the authors to check the simulated CO from these two simulations and how do they compare with satellite measurements, e.g. from IASI, to see if such high bias issues existed for CO as well.

5. I think Section 4 has a lot of interesting results regarding the impact of the 2015 Indonesian peatland fires. I strongly suggest the authors consider summarize these results into a schematic diagram that illustrates (1) the direct impact on primarily emitted gases, e.g. C2H2, C2H6, (2) changes in species such as phenol and methanol, due to direct and secondary production from primary VOC oxidation, (3) the subsequent impact on OH, NOx, NO3, O3. You have the quantitative numbers calculated and given in the text already. It will be much more useful for readers if these information are assembled into an easy-to-understand diagram and will definitely improve the paper.

6. My biggest concern with this analysis is I am not convinced that the impact of Indonesian peatland fires on lower stratospheric O3 are as significant as the authors claimed. More thorough analysis is needed before one should jump to such conclusions. Here are a few of my reasons for saying so:

i) The FIR-REF difference (ïĄĎO3) for simulated O3 at 50 hPa in the tropics is on the order of 10 ppb (max = - 12 ppb). Compared to typical O3 concentrations of 1-2 ppm in this region, this is a change of about 0.5-1% even in the maximum change region. Consider the high uncertainty and likely high biases in VOC emissions from peatland burning (as pointed out in my comment #4) and large uncertainties in aqueous phase chemistry, even this small impact could be a high-biased impact.

ii) From Figure 14 and the related discussion in Section 6, I am not sure how did you calculated and arrived at the -0.38 DU change in lower stratospheric O3. There are large dynamically-driven and chemically-driven variabilities of tropical lower stratospheric O3, at minimum you need to performance a statistically robust trend analysis, describe what you did, show your trend analysis results and discuss the results. In addition to just showing the differences, I would strongly encourage you compare the simulated total lower stratospheric O3 from these two simulations, analyze this change in the context of dynamical-driven variabilities in O3 before concluding such change is significant and the related aqueous process need to be considered in chemical modeling.

iii) By looking at Figures 8, 13, 14, with the documented ozone abundance and variability in previous literature, the change in tropospheric and lower stratospheric O3 are too small, in my opinion, to suggest VOC emissions from Indonesian fires can change atmospheric O3 in a significant way, except in the small localized regions near Indonesia.

Minor comments:

L12. hydroxyl radicals (OH) → the hydroxyl radical

L13-14. Suggested rewording: While an increase in ozone is predicted close to the peatland fires, simulated O3 decreases in eastern Indonesia due to particularly high phenols.

L16. “the impact of such extreme pollution events.” The impact on what? Please clarify.

L18. Not really appropriate to use “high” destruction. May be substitute it with “large” or “efficient”?

L20-22. What’s the statistical significance of such decrease compared to the variability of ozone in this region? See above major comment.

L26. “,” missing before “resulting”

L30. Please specify which months are the dry season.

L33. → Gaveau et al. (2014) estimated

L39. Delete “this” before “smouldering fires”

L41. → A major fraction . . . is (instead of “are”)

L41. → which comprise a large variety of species and can influence atmospheric . . .

L48. “This almost stationary globally prevailing meteorological pattern” – too many adjectives! And it is not global either. How about “This semi-stationary large-scale meteorological pattern”?

L58. What do you mean by “usual systems”? Consider use “other meteorological systems”

L76. → where the most of peatland is burned.

L76. How about change “result in” to “have”?

L77. “Indonesia is characterized by a unique emission footprint”. It is helpful to readers if you can elaborate the uniqueness of the Indonesian fire emissions, instead of implying it is unique. What’s unique about it? You may0 consider move the sentence from L83-84 to here. L103. Suggest use “simulated results with”, instead of “prediction to . . .”

L109. → provided

L119. Within this . . . → For this . . .

L119. Suggest change “modelled” to “computed” to avoid redundancy with submodels

L125. You may consider specify “linear hydrocarbons” for readers that are not familiar with this term

L127. I would suggest use “primarily emitted” instead of “heavily emitted”

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L150. It is not 100% accurate to say biogenic and biomass burning emissions are the dominant sources of VOCs, consider the importance of anthropogenic emissions. How about change to “two major sources . . .”

L152. “MESSy submodel Model of . . .” reads awkward and redundant. How about use “The MESSy submodel uses Model of . . .”

L243. Delete “even”

L399. Change “O3 sinks” to “an O3 sink”

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