

# ***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 #2**

Received and published: 9 January 2021

This paper presents modeling studies of several aspects of the strong Indonesian biomass burning in 2015, including the impact of VOC emissions, the in-cloud oxidation of VOCs, and the transport of VOCs to the lower stratosphere by the Asian monsoon. This work uses the ECHAM/MESSy model for a series of sensitivity tests, including two horizontal resolutions (1 deg and 2.8 degrees), and a comparison of aqueous-phase mechanisms. A new satellite product, HCN from IASI, is described in the appendix and used for model evaluation. A model experiment where all VOC biomass burning emissions are excluded is the basis for much of the analysis in the paper. There are a number of interesting results and new results, such as the aqueous chemistry impacts, presented, but I agree with the other reviewer that a major revision of the paper

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is required before publication.

The paper includes a broad set of topics - tropospheric composition, health impacts, aqueous chemistry, UTLS impacts - which make the paper seem a bit disjointed. As the other reviewer pointed out, the impact on stratospheric ozone appears to be insignificant and I recommend leaving that out.

The paper discusses VOCs quite generally in many places, but does briefly summarize in Section 4.1 various categories of VOCs, and describes their differences. A bit more focus is placed on aromatics, and particularly phenols, which are certainly significant in biomass burning, but a number of other HCs and OVOCs are also important and should be discussed.

The discussion of HCN does not really fit with the reactive chemistry analysis and I do not find the use of it to evaluate the biomass burning emissions in the model very convincing. This paper seems to be an opportunity to describe the new IASI retrievals of HCN, but it seems like that discussion might be appropriate for a short AMT paper, with comparisons to other satellite retrievals of HCN and validation with in situ aircraft observations. Evaluation of the model with the established and validated CO satellite retrievals, from IASI and other platforms, would provide greater confidence in the model simulations.

Here are some of the technical corrections that are needed:

I.110: in the resolution label, L90 apparently indicates 90 levels; explain 'MA'.

I.123: 'capable to represent' should be 'capable of representing'

I.193: 'one simulation exists, in which all ...' -> 'in one simulation all ...'

I. 339: 'destruct' -> 'destroy'

I.348: "80's" -> "1980s"

I.396: relative -> relatively

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I.405: rewrite "which even enhances by in-cloud ..."

I.407: 'in the same order to SOA formation' -> "to SOA formation on the same order"

I.431: reference for La Nina strengthening AMSA?

I.431: 'strengthens' -> 'has strengthened'

I.433: extend -> extent

I.456: 'appoint' is not the right word here. suggest? hypothesize?

I.467: 'our study' - does this refer to this manuscript, or previous work? If this paper, where is this shown?

I.506: deletion -> depletion

Table 1: column 2 would be better labeled 'Dominant fuel type' (fire type implies to me flaming or smoldering, for example).

Table 2: What does 'ScSta' mean? Also, define JAMOC here.

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