

# ***Interactive comment on “Polar semi-volatile organic compounds in biomass burning emissions and their chemical transformations during aging in an oxidation flow reactor” by Deep Sengupta et al.***

## **Anonymous Referee #1**

Received and published: 22 March 2020

This study quantified a number of compounds produced by burning six different fuels, most of them peat, and examined the effect of aging of the biomass burning smoke by reactions with OH (and to some extent ozone) in an oxidation flow reactor (OFR). The authors report emission factors (EFs) for all the compounds as well as the change in the effective EFs after OFR.

Major comments 1. The reported dataset is comprehensive but it is not very clear to the reader what one can do with these data. While the decrease in the EFs can likely be modeled with known OH rate constants, the increasing EFs are hard to interpret

Printer-friendly version

Discussion paper



without knowing whether additional products came from gas-ozone chemistry or from heterogeneous oxidation of particles by OH. There are some hints of this discussion in the paper, for example, on line 554. It would be good to expand this discussion.

2. I always find it surprising how small the mass fraction of quantified compounds is despite clearly sophisticated analytical chemistry methods. Would it be possible to discuss what the rest of the compounds could be? Are there giant peaks in the GC data that are not reported because of the lack of standards?

Minor comments Lines 126-133, 144-154: These sentences belong to either introduction or discussion. In the experimental section, it is better to focus on the specifics of the fuels used (such as water content, carbon content, etc.), not on the general importance of peatland ecosystems and eucalyptus forests. Line 165: it would probably help to know which molecule comes from which company Line 215: I do not think it is a sufficient argument. Reactions with OH are limited to a short time the mixture spends in OFR. In contrast, residual ozone comes out from OFR and has a chance to react with compounds on filter for the duration of sampling. Therefore, secondary oxidation of BB compounds on filter by ozone is quite possible and can only be ruled out if the residual ozone is low. The authors discuss these effects later in the paper on line 760. Line 287: MW should be 124.14 g/mol Line 360: missing right parenthesis Line 492: replace )( with a semicolon Lines 568, 660: you already used LMW on line 448, it would be better to define it there, or not use this abbreviation at all, as it is used only a few times Line 664: error in  $2.4 \pm 0.001$  appears to be unrealistically small considering other errors reported in this paper Figure 4c: two panels have only one number on the X-axis, and ticks are too small making it hard to estimate the values from the figure (the tick comments applies to all similar figure). Figure 5: the text (on line 736) suggests that similar figures exists for other fuel types, however, they are not presented in the supporting information section

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-1179>, 2020.