

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-245

Anonymous Referee #1

Referee comment on "Reconciling the total carbon budget for boreal forest wildfire emissions using airborne observations" by Katherine Hayden et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-245-RC1>, 2022

This paper describes extensive measurements of organic species in two plumes from one boreal wildfire (WF). There is a significant amount of detail and the paper, and its Supplementary Information, are quite long. The truly unique aspect of the paper is the total organic carbon measurement, which to my knowledge has never been done before. I think the paper is acceptable for publication once the following general and specific comments/questions are dealt with.

General comments;

The total organic carbon measurement and associated budget is one of the most interesting aspects of the paper, but there is not a lot of detail about it. I'd like to see some close-up plots of what the total signal and the $\text{CO}_2 + \text{CO} + \text{CH}_4$ signals looked like inside and outside the plume. This would give a sense of what the variabilities are, and ultimately what the uncertainties are in the quantity NMOG_T . I assume that the numbers presented are by Carbon, but that should be specified somewhere.

Several places the authors mention the absence of emissions data on boreal WFs. Simpson et al., (2011), present emissions data from the boreal Canadian WFs and only get mentioned towards the end of the paper. There are several other places that the Simpson et al., work should be mentioned, and the results of this work could be used to comment further on some of the detailed conclusions of Simpson et al., which were limited by the fact that they had to rely on whole air canister samples.

Specific Comments

Lines 25-27. The description of the NMOG_T budget appears incorrect. As it reads, the 7.4% attributed to I/SVOCs was part of the 46.2% NMOG, when later on in the paper it reads that the 7.4% is in addition to the NMOG. That would make sense because 100-46.2-7.4 is the 46.4% that the paper says is unidentified. In addition, these numbers should have uncertainties.

Lines 77-80. The Simpson et al., 2011 work should be mentioned here.

Lines 116-117. Here it would be good to have a bit more information on the catalyst system, e.g. amount of Pt, geometry, flow rate through the catalyst.

Lines 119-120. As noted above, this method is worth describing in more detail, perhaps with its own section in the SI.

Lines 136-138. The lack of coverage of N-containing compounds in this method is an issue and certainly bears on whether or not these measurements can be considered comprehensive. There are certainly significant N-containing I/SVOCs in WF emissions, see for example Tomaz, et al., (2018).

Line 169. Should be "depositional"

Line 214. I don't think Liu et al., 2022 is referenceable as a paper since it hasn't even been submitted. It would be a personal communication.

Line 255. Here is a place where more details on the NMOG_T measurement would be helpful. Was 100ppbv the detection limit for the NMOG_T measurement?

Line 280-281. I assume these numbers are as Carbon, not by weight?

Section 3.3.2 This section needs to address the effect of not measuring N-containing species.

Lines 341-344. What is the impact of the lack of background samples on these calculated ERs? Did you correct for CO outside the plume? Does this mean you essentially assume the VOC concentrations outside the plume are zero?

Line 347. What is the nature of this variation in sampling efficiency? How large was it, what caused it?

Line 367-368. These numbers need to have uncertainties and a description of how they were arrived at.

Line 442. These numbers have too many significant figures, 3 would be the most that are warranted.

Line 531. This is confusing, doesn't similar average MCEs imply similar fires stages were sampled?

Lines 647-648. This doesn't make any sense; these ERs differ by almost a factor of 10.

Line 651. I think at most 3 significant figures are warranted here.

Figure 8b, Did you really measure peroxyntic acid? Could this be an artefact due to IO^- ions reacting with HNO_3 in the IMR?

Table A1. This is a long table. It would be helpful if there was a heading bar on each page to make it easier to read the details.

Supplementary Information

Lines 40-41. Wouldn't the heated inlet line for NH_3 volatilize NH_4NO_3 ? How might that impact the measurements?

Line 60. The Li et al., references are not in the SI ref list.

Line 82. Do you mean it provides a stronger signal at higher masses to provide a better

mass calibration?

Line 188. Does the 3000 particle/s limit bias the measurements?

Line 192. Is this a composition-weighted proportional density?

Figure S1. It is impossible to read much of the text on the map.

References

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