

General Comment:

The authors of the MS titled “Chromophores and chemical compositions of brown carbon aerosol before and after photooxidation of combustion emissions” have made a commendable effort to address my comments and have strengthened the overall quality of the article. However, the small sample size and lack of replicates weaken my confidence in the study findings (considering that the variability of each measurement is largely undefined). I suggest that the authors consider how to address this issue; perhaps a “study limitations” discussion in the conclusion section? After the authors address this primary concern and the specific comments listed below, I would recommend the MS for publication in ACP.

Specific Comments:

Lines 46-48: I am pleased to see that the authors have provided more context concerning the Feng et al. study; however, some of this information could be condensed. Specifically, I would simplify the statement: “global distributions of aerosol concentrations were simulated with a chemical transport model with a horizontal resolution of $2^\circ \times 2.5^\circ$ and 26 vertical layers from the surface to the top of the atmosphere” and merge it with the following sentence.

Line 53: I’m afraid my original question has not been answered. What do they authors mean by this statement? The use of “primarily” suggests that primary and secondary pathways are not the only sources of atmospheric BrC.

Lines 64-65: Needs rephrasing. Perhaps something like “molecules capable of absorbing and reflecting specific wavelengths of electromagnetic radiation”.

Lines 76-77: Reword. There is an unnecessary “the” in this sentence. Also, “burning combustion” is somewhat redundant. Do the authors mean biomass burning, or combustion in general?

Lines 110-115: This section has improved but still requires clarification. I would suggest something like: “In this study, we investigated ... secondary BrC. Within a laboratory setting, a selection of fuels (list) was combusted to generate primary particles and gases; these emissions were oxidized within a reaction chamber to simulate the photooxidative aging process. The primary and secondary emissions produced through these experiments were analyzedetc.”

Lines 134 – 135: The phrasing in this opening sentence is confusing. Are the authors stating that straw and beech burned in the open cylinder were representative of waste burning and forest fires, respectively? I would also suggest moving this sentence below the following sentence.

Lines 154-155: Were these blanks used to correct the sample data? I’m assuming the blanks were low?

Lines 157-159: “However, due to sufficient particle mass loading on the filters, the potential contribution of this effect can be considered as minor”. This assumption makes me a little nervous. The (S)VOCs co-emitted with BB aerosols are similarly high in concentration – and could impart a high loading to the gas-adsorbing filter as well.

Lines 211-212: The authors mention high methanol extraction efficiency but provide no further elaboration. Was this tested in the lab? A brief quantification of extraction efficiency would be helpful.

Line 231: If the authors measured the UV-Vis absorbance of the filter extracts, why was this information not included in the MS? These data can be very useful when characterising brown carbon.

Lines 264-266: The mass fraction of $C_6H_{10}O_5$ was ~35% in the plastic burn sample - why is this not mentioned/explored here?

Lines 267-268: Can you confidently state that these mass fractions are substantially different? These values are fairly close - and we do not know how much these measurements vary, due to the lack of replicate samples.

Line 335: Since there are only 8 samples in total, I would suggest presenting the individual EEM scans in the supporting information section. Direct visual inspection may reveal subtle differences between the fuel types – differences that may not be observable in a small sample size PARAFAC model.