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Comment on acp-2021-159

Anonymous Referee #2

Referee comment on "Chemical composition, optical properties, and oxidative potential of water- and methanol-soluble organic compounds emitted from the combustion of biomass materials and coal" by Tao Cao et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-159-RC2>, 2021

The manuscript by Cao et al. titled, "Chemical composition, optical properties and oxidative potential of water- and methanol-soluble organic compounds emitted from the combustion of biomass materials and coal" describes the characterization of various physicochemical properties of aerosols emitted from biomass and coal combustion. While the work here is important, as it provides necessary data that is currently missing in the literature, from BrC light absorptivity and oxidative potential values with corresponding chemical composition information, there is critical information missing that hinders the ability to review this manuscript at this time. As such, major revisions to the manuscript need to be done before it can be further reviewed for publication. Also, a more in-depth discussion of the author's observations is warranted to clearly highlight the novelty of this work.

Specific Comment:

One of the major conclusions re: the DTT assay work is that the ROS activity is "weaker" compared to previous studies, likely due differences in chemical composition. This point should be further supported by comparing the chemical composition observation from this current study and other cited studies of ambient aerosol. This adds to the novelty of this study.

Detailed information about the methodology re: the TOC content analysis was supposed to be in the supplementary, but in the supplementary file, this "detailed measurement method is provided in the SI file" (line 133-134 of SI). This information needs to be presented, as some of the results presented are mass-normalized. However, it is unclear if "ug" is referring to the mass of carbon, or the mass of the WSOC, or the mass of the PM (e.g., the conversion of the mass of OC to organic PM concentration uses a conversion factor (Turpin and Lim, Aerosol Science and Technology, 2001, 35(1) 602-610). This lack

of information makes it difficult to assess if the direct comparison of mass-normalized results as reported in the current manuscript (e.g., Figure 6) is applicable.

L184: For the ^1H NMR work: What does it mean that the BrC fractions (e.g., dissolved in water, recovered from the SPE cartridges with solvent, and methanol extracted) were dissolved in D_2O ? Effectively, this was a liquid-liquid extraction? I am not sure if all the MSOC components dissolved in the D_2O (and which from my understanding is the portion of the sample that leads to the ^1H NMR signal), and as such, I am uncertain if the authors can equate the proton-NMR data are measurements of BrC in the MSOC BrC fraction. Why not use deuterated methanol instead?

L224: How is HULIS a hydrophobic fraction of *water-soluble* organic carbon? Perhaps you mean it is less polar components of the WSOC?

L125: It is not clear as to how these blank filters can be used to correct the mass of smoke, optical signal, and DTT consumption by BrC. Were these blank filters placed into the filter collection system behind the first filter that contains most of the aerosol (e.g., this is typically done for quartz filter for breakthrough, such as correction for semi-volatile organic carbon).

Minor comments:

L218: It is not clear which parameter was used to infer "average contribution of WSOC to wood smoke $\text{PM}_{2.5}$ " from Table 1 (is it WSOC-C/PM)?

Figure 5: I recommend the authors abbreviate "typical biomass burning" as something else other than "WS" as it is confusing when "WS" is commonly used to refer to water-soluble.

It is also useful to provide the DTT values that are normalized by volume of air, these values would be useful for the calculation of exposure. Can the authors provide these analogous values in the supplementary?

The volumes and concentrations of reagents used in the current study are not as described by most of the papers cited by Bates et al., 2019 and Verma et al., 2012. The general approach is certainly identical, but the specific details are not. I highly recommend the authors to specify the differences, as this may be important for comparison to other literature value.

L56: The Lin and Yu, as well as Ma et al. papers, looked at the generation of ROS by HULIS - these paper did not actually report whether the HULIS is also BrC (e.g., it is still not known if all HULIS can be considered as BrC and vice versa). On a related note, the next sentence is written in a manner that suggests that all HULIS is BrC.

Technical Corrections:

The Ma et al. reference has some typos/weird characters.

L77: I don't understand the following sentence: "However, these studies only focused on the BrC fractions emitted from BB or CC, and therefore the comprehensive characterization and full understanding of the BrC fractions from combustion processes are still required." Is the author referring to the fact that the cited studies only focused on certain fractions of BrC (e.g., water-soluble BrC?).

L138: the Cheng et al. (2016) reference was included twice.

L206: It is not clear what the acronym DTPA stands for, it has not been explained prior.