Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1027-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Carbonaceous aerosol composition in air masses influenced by large-scale biomass burning: a case-study in Northwestern Vietnam" *by* Dac-Loc Nguyen et al.

Anonymous Referee #1

Received and published: 20 December 2020

This authors report ground-based measurements of speciated OA, OC, EC, and various gas-phase components in northern Vietnam. The measurements are divided into three categories, including low, medium, and high biomass burning periods based on different tracers (including levoglucosan). The manuscript is well written, and the data analysis provides a reasonable way to source apportionment the sampled emissions. The manuscript merits publication in ACP after the authors address the below comments. I have two main concerns with the manuscript.

1) The authors do not really emphasize the importance of the work. My impression after reading the manuscript was that the main motivation for the work was the lack of mea-

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surements in the area, which significantly weakens the quality of the measurements reported and the effort that was put in the analysis of the data. The results presented are not placed in a greater context. For instance, how are such measurements useful to the community? What did the authors learn? This seems largely missing.

2) The section reporting the molecular composition of aerosol constituents (section 3.2.2) is very linear and, at times, doesn't converge to any meaningful conclusions. For instance, in the first paragraph of that section (anhydrosugars), the only useful information is reported concentrations. The rest of the discussion is not relevant to the overall message of the paper and seemed to belong to methods and not results. The same is true of the PAH discussion. Here, the authors discuss in detail the trends of various indicators (e.g., BaP/BeP+BaP) but the main take away from that section is not clear to me. To illustrate this, the authors mention the following: "Altogether, PAH diagnostic ratios and PAH pattern are not useful in this data set to elucidate their emission sources and could not linked to BB, which agrees with the relatively large distance of PAH to BB markers such as anhydrous sugars and methoxyphenols". Why is this interesting? Is it because other studies have used these ratios for source apportionment, and that these diagnostics are not valid here? If so, this needs to me clearly motivated and better discussed. If not, then this should be omitted from the result section. In summary, section 3.2.2. needs to be tightened and focused.

Minor comments:

Line 21: "but OC composition studies are missing in the scientific literature." Change to: "but OC composition studies from this site are missing in the scientific literature".

Line 53: "...may change (Nordin et al., 2015; Gilmour et al., 2015)." This is disconnected from the sentence. Please adjust.

Line 65: define PM2.5

Line 67: define PAH and o-PAH

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Line 75: adjust to: “Aside from the measurements. . .”

Line 79: 579,000 and not 579’000

Line 82: adjust to: “in the vicinity of station.”

Line 86: adjust to: (fine inhalable particles <2.5 um diameter)”

Line 97: remove “from” after Pyrolytic OC (OP)

Line 173: replace “distinctly different above one” with “larger than unity”

Line 175: add space between EC1apparent and OP for clarity

Line 204: the authors offer no explanation as to why nitrophenols behave so distinctly from the rest of the categories. Is it because nitrophenols are secondary and the rest are primary emissions? This needs to be expanded.

Line 217: replace “quantified OA” with “speciated OA”

Section 3.2.2. see major comment #2

Line 236: adjust to” appeared in the range of”

Line 255: replace “dominating” with “significant”

Figure 6: the arrows in the graphs are confusing and I am not sure what the authors are trying to point at

Line 376: the differences in behavior for CO, CO2, CH4 and O3 is also due to the fact that CO, CO2, CH4 are mainly of primary origin whereas O3 is not.

Line 401: “, and rose again. . .”

Line 404: “As for the $\Delta O3/\Delta CO$, both CO and CO2 are close to the chosen background. Consequently, at CO2 mixing ratios below 410 ppm, the uncertainty of MCE exceeds 0.04.”. These sentences seem unrelated to me, and the first sentence makes no sense.

Line 407: I am surprised that Figure S5 is buried in the SI. To me, Figure S5 is a central finding of the paper. The manuscript first discusses timeseries of multiple speciated OA components and their variability. The authors then discuss the variability in terms of three emission categories (low, med, and high BB), but MCE seems like a more intuitive metric. Also, MCE is easily obtained from emission inventories and the parameterizations presented in Figure S5 can be useful to modelers. MCE explains part of the measured variability for primary components, as already noted in the text, and (to a lesser extent) for secondary components. The results are robust given that multiple sources influence the sampled air.

Line 407: "The absolute values of the slopes are similar to the intercepts obtain from the regression (Table S1), giving reasonable concentrations of organic close to detection of ideal combustion with MCE of 1.". This sentence is not clear to me. I think what the authors are trying to say is the following: to have null concentrations of OA at MCE = 1 (perfect combustion), the slope of the regression should be equal to minus the intercept. I get that. Is this right? Anyway, please re-phrase.

Line 409: "PAHs and -alkanes, which cannot be formed by atmospheric aging, show the best agreement with the regression function, whereas the fit for nitrophenols is clearly dominated by the datapoints of the high-BB days, indicating formation by secondary processes.". I have trouble with this. If I use the same rational the authors previously used, if nitrophenols (for instance) are likely SOA, then I would not expect a value $\sim$0 near MCE = 1 (since it is not a primary combustion pollutant and therefore not directly driven by changes in MCE). However, the values of the slope and intercept for nitrophenols are very close (see Table S1). Wouldn't you expect in this case a significant difference between the slope and y-intercept of the regression line (so as to not get a value of 0 at MCE=1)? I understand that there is substantial variability in the measurements, but the fit is consistent with a value of near zero at an MCE of unity, suggesting the same behavior as combustion derived POA. Please explain. Also note that there are 2 tables in the SI named Table S1. Adjust in the SI and in the manuscript.

Line 426: “A main difference between the more polluted cluster (high) and the medium polluted cluster (medium) seems to be a recirculation over land area for the medium cluster as compared to a more westerly advection for the high cluster.” This also explains the lower $\Delta O3/\Delta CO$ ratio for high BB event compared to the medium BB event.

Line 426: The significantly higher concentrations of nitrophenols (presumably SOA) during the high BB event compared to the medium BB event seems in disagreement with the fact the high BB plume is fresher. Clarify.

Line 443: replace “comprehensive” with “extensive”

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