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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics, primary sources and secondary formation of water soluble organic aerosols in downtown Beijing" *by* Qing Yu et al.

Anonymous Referee #1

Received and published: 15 September 2020

Yu et al reports observations of organic aerosol, both primary and secondary, collected on filters for different seasons/time periods of 2017 in Beijing, China. They report water soluble organic carbon (WSOC), its hydrophobic and hyrdophilic portions, water soluble ions, total PM2.5, total organic carbon (OC), and total elemental carbon (EC). Further, they report tracers associated to different sources (levoglucosan, cholesterol, phthalic acid, 4-methyl-5-nitrocatechol, 2-methylerythritol, 3-hydroxyglutaric acid, and cis-pinoic acid). They use the tracers to differentiate sources of OC and WSOC during the seasons via "CO-scaled" concentrations, day and night ratios, correlation coefficients with various meteorological and chemical properties of aerosol ("acidity" and liquid water content), and positive matrix factorization. They find that aqueous chem-

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istry explains a large portion of the secondary organic carbon during most seasons except summer, where photochemistry explains an important biogenic portion. They also find differences in the sources between the seasons (biomass burning vs dust vs vegetation). Overall, the paper is important and of interest to Atmsopheric Chemistry and Physics community as there is general overall uncertainty in the sources of organic aerosol in urban environments, especially during all seasons and high pollution events. The paper will be of value once the authors address the comments below.

Section 2.1 Sampling: Further information is needed here for the readers to have a better understanding of how the aerosol was collected–Was there a drier in-line prior to be collected on the filters? Was there a denuder to scrub gases prior to the filter to minimize gas-particle partitioning? Was there an impactor or cyclone for size selection?

Further, of importance, was there any analysis of potential reactions that occurred on the filters prior to sampling?

Line 121: It is unclear what the standards curves were of (the tracers reported throughout paper or other standards), and what is meant by "standard curves with five to seven concentration gradients were re-established." What was re-established?

Line 122: What authentic standards? What company/purity?

Section 2.3: Please describe or cite the PMF software used. PMF 5.0 is not enough to understand how positive matrix factorization was actually conducted.

Line 140: I highly recommend the use of "aerosol acidity," as defined in this line, due to the discussion from Pye et al. 2020 (https://acp.copernicus.org/articles/20/4809/2020/). The ratio here does not define acidity, and is analytically challenging to say if it is defining the amount of hydronium ions in the aerosol phase, as the hydronium ions may be a very low detection limits that cannot be quantified due to propagation of uncertainty.

Section 3: Though an important and valuable aspect of this whole manuscript is that the

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filters were collected during different seasons, I highly recommend the authors soften the language throughout that the results "reflect" a specific season or are similar or different to other studies. Since it's only for one year and approximately 2 weeks for each season. The limited data makes it hard to say how typical the results are and this should be discussed/emphasized throughout (instead of general statements that in fall this is what is observed/happens).

Another area I suggest the authors be careful in their discussion of r values, as majority of the values they report lead to Rˆ2 values less than 0.5 (thus explaining less than 50% of the variability observed).

Line 195: Since the authors are comparing OC from emissions inventory to Fig. 2, I would recommend converting the emissions to OC-to-CO ratios. Also, I would recommend adding these ratios, if possible, to Fig. 2, for direct comparisons with observations.

Line 199: Is it possible to get emission inventory values of residential biofuel combustion and coal combustion to compare with the OC from open biomass burning?

Line 207: It is unclear how aromatics form SOA to impact WSOC during winter, as the photochemistry is greatly reduced. Could the authors provide more discussion concerning this?

Line 249: It is surprising that the authors are saying that gas-phase photoxidation was not the dominant formation mechanism of secondary organic carbon. I can see maybe WSOC, but seeing all secondary organic carbon is a big statement. Especially, since the authors go on in line 254-55 to say photochemistry plays a role.

Line 339: Source 3 did not show the highest contribution in winter....highest contribution of what?

Line 416-419: I would recommend caution here, as other hypothesis have been stated for reasons in differences between chamber SOA and ambient SOA, including losses

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of vapors to the walls and autoxidation (which has been shut down in chamber experiments due to too high NOx levels and/or too high aerosol loadings).

Table 1: I would recommend somehow highlighting which values show statistical differences between day and night and between seasons. Also, I would recommend including average CO mixing ratios.

Fig. 1: I would recommend including a line that shows the average and standard deviation for the WSOC/OC values. Currently, eyeballing the values in Fig. 1, they look fairly similar in all seasons.

Minor: Line 30: replace takes up with composes Line 117: replace entirely dryness with either "entirely dry" or "entire dryness" Line 121: replace T The with The Line 248: replace did not appear any with did not have any Line 280: replace association with correlation Line 294: replace appeared with showed Line 340: replace except with beyond Line 377: remove Nevertheless Line 399: remove of after Both Line 408: believe C is missing after SO Line 427: replace was in consistent with was consistent

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