

Referee comment on “Seasonal variations in composition and sources of atmospheric ultrafine particles in urban Beijing based on near-continuous measurements” by Li et al., <https://doi.org/10.5194/egusphere-2023-809>

Li et al., presented an extensive study on the chemical composition of ultra fine particles in an urban area in Beijing. The measurements were performed over the four seasons, which makes this study very robust. The chemical composition of particles was measured by a thermal desorption chemical ionization mass spectrometer (TDCIMS). The authors found that the particles measured in Beijing are dominated by organic compounds. CHO organic compounds are the main constituents of particles during summer, while organic particles containing sulfur, nitrogen, nitrate, and chloride are more abundant in winter. A Positive Matrix Factorization (PMF) analysis was performed in order to determine the sources of these particles. A 5-Factor solution was suggested, and the authors related these factors to cooking, vehicle emission, photooxidation formation, and aqueous/heterogeneous sources.

Besides the chemical composition, the authors reported nucleation, growth, and emission rates. Thus, the highest particle number concentrations were found in winter. The authors explain this fact due to the highest primary particle emission, the highest new particle formation, and the lowest growth rates in winter.

The study by Li et al., is highly valuable and certainly contributes to the understanding of urban ultrafine particles. I appreciate the way in which the manuscript is written. I very much enjoyed the reading. Furthermore, I would recommend it to be published on EGU sphere after addressing the following comments.

Specific comments:

Lines 17, 19, and along the manuscript: I would recommend using either UFPs or UFP to make the manuscript uniform.

Could the authors define CHO in the abstract? once in the abstract and once in the introduction.

Why the collection efficiency on the filament decreases with the particle size?

Is the  $O_2^-$  chemical ionization technique more sensitive towards low and/or high oxygenated organic compounds?

At which temperature is the filament heated up? Is this done gradually, how long does it take to evaporate the sample? Is the filament heated directly, or is there any heated carrier flow? Is the highest temperature enough for desorbing all the compounds collected?

How do the authors determine that a large fraction of the compounds below 100 m/z were produced by thermal decomposition?

In line 141: is this the normalized signal (by the reagent ions) or is the raw signal?

In Eq. (1) is the GR the net condensation growth term or is the condensation growth rate of the particle?

In line 106 is written that the TDCIMS performed the measurements in the “bulk collection mode”, with this I assume the TDCIMS does not select previously the size before collection and evaporation. In this sense, can the authors clarify how the size-dependent composition then is done? Is this analysis base on the observed size distribution measured by the PSD and SMPS? Or the PSD and SMPS were coupled to the TDCIMS allowing the collection of particles with known size?

If the PSD and/or SMPS were not coupled to the TDCIMS. Did the authors observe size distributions with 2 or 3 modes? How did the authors isolate the effect of the big particle on the small particles?

What are the possible losses that the TDCIMS can experience?

In lines 218 and 219, what does slow desorbed compounds mean, in terms of temperature? And why their appearance is related to the highest solar radiation and low NO<sub>x</sub>? Could the authors clarify how slow and fast desorbed compounds are defined in terms of temperature before explaining further their characteristics?

In line 220: what does “occurs later” mean here?

Lines 237-240: can the authors add connectors or make these sentences shorter?

Page 10: I very much appreciate the interpretation of the PMF analysis. I have a couple of questions about this. How do the residuals look like? Do the authors observe any factor related to background, or was the background removed from the data?

Did the authors apply any complementary technique for fully identifying the compounds described in Section 3.2? Is it possible that any of these compounds, for example,  $C_3N_3O_3H_2^-$ , and  $C_6H_9O_5^-$  are affected to some extent by thermal decomposition and not fully correspond to cyanuric acid and deprotonated levoglucosan? Do the authors characterize thermally these compounds, so they can certainly claim that these are cyanuric acid and deprotonated levoglucosan?