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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Enhancement of nanoparticle formation and growth during the COVID-19 lockdown period in urban Beijing" *by* Xiaojing Shen et al.

Anonymous Referee #2

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This study presents observational results of particle and ions number concentration in Beijing, China during Jan. 24 - Feb. 14, 2020. This period represent the Chinese New year and the COVID-19 lockdown period, therefore suitable for understanding the influence of reduced emission to nanoparticle formation and growth. Although the data set is novel and the main research question it wants to discuss sounds interesting, there are too many important values and discussion in the text which are based on guessing. Overall, I don't recommend this paper to be considered to be published before major reconstruction.

1.I would say that the current evidence (No measured or modelled VOCs) does not

allow the major conclusion on VOC contribution of particle growth of particles of 10nm$\sim$100nm. The only mentioning of VOCs is that reduces by $\sim$45% in the BTH area, but then they say that low volatility products increased with a VOCs oxidation capacity factor of $\sim$1.3. But if we do a simple estimation, 1.3*0.55=0.71, which would mean the oxidation products would not increase. I'm not questioning the conclusion that oxidation products could increased, but there's just far too less data in this study to support this.

2.Furthermore, it seems to me at least that authors does not understand the concept of ELVOCs or HOMs correctly. It should be noted that the concept of "ELVOCs" should be used when we don't have information of the volatility, but only use "HOMS" when we are discussing oxidized VOCs. However, using k1*[OH]*[VOC]+k2*[OH]*[VOC] is certainly not acceptable as a proxy of HOMs. First, this represents the first generation product of oxidized VOCs, and even for a-pinene oxidation which is the most studied HOMs production pathway, this only produces very volatile OVOCs, and they won't contribute substantially for the growth of Aitken mode particles. Secondly, while this proxy was developed for an a-pinene rich boreal forest, the main VOCs in Beijing are aromatic, alkenes, and even the main BVOC are not a monoterpene but isoprene. So there needs to be far more discussion to settle down which is the main OVOC contributing as low volatile products, and multi-generation products instead of first-order products should be considered. Last but not least, in the wintertime in Beijing, the night is very long, and oxidation by NO3 should not be ignored.

3.The authors claim that nucleated particles can grow to CCN size and contribute to particle mass during haze events. But overall, there is very less discussion of investigation of the particle growing form nucleation mode to accumulation mode, which means that quantitative understanding is lacking. For Fig7., if we take 6th Feb for example, it seems that the growth from aitken to accumulation mode comes from growth of pre-existing particles. And even though the growth of particle number concentration seems to terminate by 7th Feb, it seems that the PM2.5 mass still increases rapidly. At noon 8th Feb, it looks like there is a new polluted air mass coming, leading for stronger pollution.

4.I think the paper can be resubmitted by putting more effort on nucleation and early growth by sulfuric acid. The NAIS measurement seems to work good, and could be discussed more in depth. To explain the growth driven by oxidized VOCs, either support by measurement of chemical composition or a chemical mechanism model is needed.

Minor comments:

1.The fitting coefficients for H2SO4 proxy should not used the same as the measurement in Finland. For Beijing, there's a paper by Lu et al (2019). Note that the effect is nonlinear and will effect trend in Fig 4-5. And if Global Radiation is used instead of UVB for H2SO4, it should be stated as it was done for OH. UVB is a fraction of Global Radiation, therefore a new coefficient should be used. Lu, Y., Yan, C., Fu, Y., Chen, Y., Liu, Y., Yang, G., Wang, Y., Bianchi, F., Chu, B., Zhou, Y., Yin, R., Baalbaki, R., Garmash, O., Deng, C., Wang, W., Liu, Y., Petäjä, T., Kerminen, V.-M., Jiang, J., Kulmala, M., and Wang, L.: A proxy for atmospheric daytime gaseous sulfuric acid concentration in urban Beijing, Atmos. Chem. Phys., 19, 1971–1983, https://doi.org/10.5194/acp-19-1971-2019, 2019.

2.I didn't found the OH proxy used here in Petäjä et al(2009). Please make sure the right reference is cited. 3.There are spelling mistakes and grammar errors, try to find an english expert to fix them all, eg: The number concentration of Aitken mode particles (∼25-100nm) should also decreased as expected-> The number concentration of Aitken mode particles (∼25-100nm) decreased as expected or The number concentration of Aitken mode particles (∼25-100nm) decreased as they should.

Use the term oxidizing capacity consistently, replace all "oxidization capacity"

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