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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating three patterns of new particles growing to cloud condensation nuclei size in Beijing's urban atmosphere" *by* Liya Ma et al.

Anonymous Referee #1

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This study investigated patterns and characteristics of atmospheric new-particle formation events in Beijing. The authors categorized these observed events into three classes based on the number size distributions of the newly-formed particles. Further, by combing the size distribution with the speciation of measured or modelled gas- and particle-phase pollutants, the authors discussed the contribution of organic and inorganic compounds to particle growth during different type of events.

The manuscript focus on the topic of new-particle formation in the urban atmosphere, trying to address critical questions that whether or not the newly-formed particles can grow to the CCN size, and what conditions/species control the grow process. The

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scope of the manuscript is thus suitable for ACP, and the data the authors presented are ample and interesting. However, the interpretation of some key results is questionable and leads to unrigorous conclusions. Major revisions and improvements are needed before this manuscript can be considered for publication as an ACP paper.

Major comments:

1) The authors defined three classes. “Class I was characterized by no apparent particle growth” makes sense, this class might indicate either a lack of supersaturated condensable vapors so that particles don’t grow, or a too high condensation sink so that small particles don’t survive. But is there a better way to classify the rest events? Particles are larger than 50 (or 75) nm doesn’t necessarily mean they are good CCNs; and there are so many factors (chemical, physical or meteorological) that can determine whether or not the particles grow over 50 (or 75) nm. Classifying the events just based on the “cut-off” size doesn’t really help modelers or lab experimentalists to understand the real atmosphere. Please justify the classification or improve it.

2) From the surface plot of these NPF events (e.g. Fig 2a, e; Fig 3a, e; Fig 4a), I don’t see any significant band of pre-existing particles. Were these events all observed in very clean days? Or is it because the linear color scale veil the background particles? Please do change to the log color scales.

3) The author stated that many growth events lasted for over 10 hrs or even a whole day. Was there any primary emission mixing with the newly-formed particles, e.g. from vehicles, restaurants or factories? Is it true that there was only condensational growth without mixing during the whole period? Please discuss this and also show O:C from the AMS measurement to verify the statement.

4) About the AMS measurements, the sampling site is 8 km away from Peking University, how long does it take for an air parcel transport from one site to the other? Roughly one hour maybe? How well does the AMS result represent the particle composition at Peking University? I think this question need to be better addressed in order to discuss

the spatial heterogeneity.

5) AMS measured the bulk PM_1.0, how well does the chemical composition in PM_1.0 represent the species drive the sub-100 nm particle growth? Were there any aerodynamic diameters measured by AMS at the same time? Discuss more about the uncertainty here.

6) Page 4, line 20, Equation 4, I don't find the exact same equation in the references the authors cited here. Using averaged particle number concentration over the whole growth period can bring in large uncertainties. Newly-formed particle are prone to coagulational loss; this means particle number concentration at D_pg1 will be much higher than that at D_pg2, and the Mass_required will be overestimated. Please justify the equation, or calculate the particle mass concentration for each FMPS scan and also take the coagulation into account.

7) Was there a special reason to sum up O3 and NO2 instead of discussing them separately? NO2 is not always associated with O3, it could come from primary emission such as vehicle exhaust.

8) The authors briefly mentioned seasonal variation, but didn't dive into the details in, for example, wintertime events. A recent study (Wang et al., 2020, Nature 581 (7807), 184-189) show that NH4NO3 could help the newly-formed particles grow and survive in winter. So it's intriguing to know that the authors observed that the newly-formed particles didn't grow during wintertime events, but it would be more important to understand why they didn't. Was it because of a lack of supersaturated condensable vapors, or a too high condensation sink?

9) Please discuss more about the model uncertainty, sensitivity test, etc.

10) The authors used many sentences describing the particle growth/shrinkage processes vs time, e.g. "However, the shrinkage occurred as early as 15:20-17:20 on 11 June.". Yet these sentences contain very limited information. I would suggest the

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authors go through the whole manuscript and reword these sentences by discussing more deeply about the environmental conditions or the causes of these different types of growth.

Minor comments:

1) Page 1, line 17: “11/27” (and hereinafter) should be something like “11 out of 27”.

2) Page 1, line 22: “. . . in the remaining NPF events”, please add the number here.

3) Page 2, line 26: “survival probability ratios”, “survival probability” would be better.

4) Page 3, line 9: should the coefficient be size-dependent?

5) Page 3, line 13: “During the other observational periods. . .”, please specify the date.

6) Page 6, line 8: “to a negligible level; in Scenario 2”, the semicolon should be period.

7) Page 6, line 10: “. . . may not represent two NPF events occurring in one day.”, why? They look very much like two events.

8) Page 6, line 14: “. . . associated with wind direction changes in the late afternoon or nighttime.”, please specify the wind directions, and discuss if the sources of pollutants changed.

9) Page 7, line 14: “. . . from 9-22 nm to 23-69 nm. . .”, please reword it.

10) Page 8, line 29: “uparticulate” should be “particulate”.

11) Page 9, line 19: “. . . need to confirm this.”, should be “. . . are needed to confirm this.”

12) Page 9, line 30: “stopped the growth”, should be “stopped growing”.

13) Page 10, line 17: “The observed concentrations of OM and NO3- largely oscillated and had no increasing trends after 21:00, although Dpg increased from 60 nm to 75 nm in one and half hours.”, explain this.

14) Page 12, line 5: “The slope further suggests that an increase of 10 ppb in Ox likely causes an increase of 5 nm in Dpgmax.”, I would suggest removing it.

15) Page 12, line 23: “When the estimated CS were compared, the averaged value was 1.8±2.0×10-2 s-1, 2.1±1.5×10-2 s-1 and 2.0±1.2×10-2 s-1. . .”, the deviations are too large to provide detailed information, I would suggest removing it.

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