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Atmospheric Chemistry and Physics Discussions
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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 #2

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This study investigated seasonal variations of new particle formation (NPF) events in Beijing by using observations of particle size distributions and chemical compositions of aerosols and numerical model simulations. The authors found no apparent growth of new particles in winter whereas the growth of new particles to CCN size (50 or 75 nm) was often observed in summer. The three patterns of NPF events during the summertime were discussed in terms of secondary aerosol formation, evaporation of semi-volatile species, and spatial heterogeneity of NPF events.

The scope of this manuscript is well suited to ACP, and the data obtained by the authors are valuable and important to understand the mechanisms of NPF events in urban

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Discussion paper

atmospheres. However, the current manuscript needs substantial revisions before the manuscript is considered as a publication of ACP as shown below.

1) Page 1, Line 17:

“11/27” should be revised. For example, “11 new particle formation (NPF) events out of 27 events” may be better. Other parts written similarly in the text should also be revised.

2) Page 2, Lines 21-28:

The authors described what they did in this study. However, it is not clear to me which parts of this manuscript are scientifically new. There are many previous studies on NPF in Beijing and other urban areas. The authors should summarize these previous studies and describe what are well understood and what are poorly understood in Introduction. Then, the objectives of this study should be described more clearly.

The sentence at Lines 18-20 (Thus far, which chemicals drive the growth..) is a point poorly understood, but I don’t think the understanding on this point was improved by this study.

3) Page 4, Line 3: Equation (2)

Please add descriptions on the uncertainty of this equation.

4) Page 4, Line 11:

The SPR analysis (section 4.5) is not meaningful. It is hard to quantitatively estimate the survival fraction of new particles from this equation because the SPR values can be greater than 100% in many cases (Table 1). I think the authors may be able to calculate the loss rate of new particles during each NPF event from CS.

5) Page 4, Line 13:

Please clarify why 3 sigma was chosen.

6) Page 4, Line 20: Massrequired

The authors compared Massrequired with the changes in mass concentrations of organic and nitrate aerosols, but the latter is generally controlled by accumulation mode particles, not nucleation mode particles. The comparison between Massrequired (changes in aerosol mass for nucleation or Aitken mode particles) and the changes in mass concentrations of organic and nitrate aerosols (mainly controlled by accumulation mode particles) is therefore not so meaningful (in sections 4.1-4.3).

7) Page 5, Lines 1-2:

Please provide some brief descriptions on model setups.

8) Pages 5, Lines 8-9:

Please describe on model evaluations more clearly (e.g., the degree of agreement with observations, chemical species evaluated).

9) Page 5, Lines 23-27:

The unit of number concentrations in this paragraph is probably not correct.

10) Page 6, Lines 2-19:

Please clarify why Class II was subclassified to 4 scenarios. What is the purpose of this?

11) Page 8, Line 3: the contribution of <2%

Please clarify how the authors estimated this contribution.

I think the authors have sulfate data observed by AMS. The data can be shown like OM and nitrate in Figures 2-4.

12) Page 8, Line 7: 13 ug m-3

Please clarify how the authors estimated this value. Did the authors consider the spread of particle size distributions? (like 3 sigma in equation (3)).

13) Page 8, Lines 6-8:

As I described above, the comparison between the required mass (13 ug m-3) and PM1 enhancement (15 ug m-3) is not so meaningful because the former focuses on nucleation/Aitken mode particles but the latter is usually dominated by accumulation mode particles.

I think what the authors can do here is to calculate mass concentration changes for sulfate, nitrate, ammonium, and SOA and to discuss which changes are the largest during the growth periods of new particles.

14) Page 8, Line 7:

OM can be divided into HOA (POA like) and OOA (SOA like) by using m44 and m57 signals. Only OOA can contribute to the growth of particles.

15) Page 9, Lines 3-4:

I don't agree with this authors' description. The simulated OA and nitrate cannot be used to interpret the data unless the authors evaluate the simulations with observations.

16) Page 10, Line 16

Delete "(ON)".

17) Page 10, Line 28:

OM can be divided to HOA and OOA as I described above.

18) Page 11, Lines 5-6: "Repartition of the . . ."

This part should be removed because no data can support this sentence.

19) Page 12, Line 19

“then in”, “transience”: they should be corrected.

20) Page 12, Line 26: Section 4.5

As I described above, this section is not so meaningful and should be removed.

How did the author consider the contribution of primary particles in this analysis?

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