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Comment on acp-2022-484

Anonymous Referee #1

Referee comment on "Survival probabilities of atmospheric particles: comparison based on theory, cluster population simulations, and observations in Beijing" by Santeri Tuovinen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-484-RC1, 2022

The authors compared the survival probabilities of atmospheric particles using theory, simulations, and observation data in Beijing. I find the the comparison with observation data especially valuable, and this work can be nice contribution to NPF studies. However, questions below need to be addressed before the manuscript can be published.

- Line 183: 'Evaporation in the cluster formation from sulfuricacid and dimethylamine has been observed to be negligible'. This sentence is somewhat misleading and 'has been' is very vague. The evaporation rate is only close to negligible when the base concentration is very high. Jen et al. (2014) listed the nucleation rates in their Table 1. I would rather claim the simulation is a reference in the kinetic limit for comparison with the analytical formulae or observation.
- Line 270: 'the loss rates of the clusters can also be considerably affected by the cluster-cluster collisions inaddition to the GRs if the background CS is small and the cluster concentrations are high'. This sentence seems to suggest growth will cause the cluster to be lost, which is incorrect.
- Figure 2. I would suggest adding simulated rates with cluster-cluster coagulation to the right y axis of figure 2. Since simulation with cluster-cluster coagulation is extensively shown in subsequent figures, this is important information.
- I find Figure 5 challenging to read as there are too many cases presented in a single figure. I suggest reducing the number of cases presented in this figure. For instance, one fitting method can be presented while the other may go into supplemental.
- The simulation is extensively compared to the observations. However, I wonder if the simulation is a good representation of atmospheric particle growth: in the simulation there is only one condensing vapor, while in the atmosphere the condensable vapor concentration increases with particle size. This is especially the case for growth between 1.5 nm to 3 nm when the Kelvin effect is very strong. For larger particles it might be alright because the 'condensable' concentration is no longer a strong function of particle size; using a single vapor to model the growth rate may coincide with the real atmosphere. Therefore, how is it justifiable that the simulation is directly comparable to atmospheric observations, especially for Figure 7? (The authors sort of discussed this matter in section 3.4.3 when the uncertainty of GR is discussed, but I wonder why the participation of other species on growth is not explicitly discussed) Overall, the role of the simulation needs to be better defined/clarified.

- Line 361: what is the meaning of 'median NPF event'? Median of what? The word 'median' is extensively used in section 3.4.1. I found many of them really confusing.
- Section 3.4.2: The sequence of presentation is non-linear: the authors first briefly discuss Figures 7-9 briefly and then discuss Figures 7 and 8 again with more depth. I wonder if it's better to just sequentially discuss Figures 7-9.
- Line 455: I have doubts regarding uncertainty of J_5. The measurement uncertainty decreases with increasing particle size, hence I assume the uncertainly of $J_{1.5}$ is larger than that of J_3. Is it possible that $J_{1.5}$ is actually systematically underestimated, as a result of which $J_3/J_{1.5}$ is larger than reality? From what I know sub 3 nm particles often seem under detected in the observed PSD, does this have an effect?

Technical corrections:

- The rate constant is missing in the third term on the R.H.S. of Eqn. (8).
- In the top panel of Figure 6, tick labels are missing on the left y-axis. Also, can the color scale be changed for this plot to make NPF event more visible?