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Comment on acp-2022-484

Anonymous Referee #2

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-RC2, 2022

The manuscript deals with survival properties of freshly nucleated particles while they grow and scavenge. Simple parameterizations and an aerosol dynamic model are compared against observations in Beijing, where the high survival rates have puzzled researchers because background aerosol concentrations are so high that the sink they cause should (theoretically) result in lower survival rates than what is observed. The usefulness of the manuscript, from my point of view, lies in the uncertainty analyses presented and the discussions related to potential causes of the above-mentioned discrepancy. The manuscript is definitely suitable for publication in ACP, however, some issues have to be clarified prior to this.

Major comments:

- Several of the authors here are authors in two other papers/manuscripts, also dealing with the topic of survival rates in Beijing: Cai et al. (ACP 22, p. 11529, 2022) and Cai et al. (ACPD, acp-2022-476, 2022). In the first one of these, the authors state in the conclusions: "The low theoretical survival probabilities of new particles contrasting to the new particle formation events observed at high coagulation sinks should be caused by underestimated growth rates". In the latter one, the authors claim that experimental survival probability for 'Beijing-type' events should be determined by comparing pointwise dN/d(logdp) values and with this approach "the measured and theoretical survival probabilities are on average consistent with each other". In this manuscript, under review, the analysis is based on studying what kind of uncertainty is needed in the estimated values for GR and CS for the theory/model to match experimental findings.

The three papers/manuscripts thus approach the concept of survival from slightly different angles, and a short discussion (with references) should be written about the differences/similarities in these. And, if possible, some speculative conclusions would be nice about the combined results of these studies, e.g. have we so far analyzed experimental survival incorrectly (as implied in acp-2022-476) or have we analyzed GR somehow incorrectly or are the measurements so uncertain that observational survival results with some reasonable uncertainty range actually agree with theory/modeling.

- As also detailed aerosol dynamics modeling has been utilized, it would be nice to see some more details about the results. With this I mean, for example, contour (or banana) plots of the growing nucleation mode for an average NPF day.

Minor comments:

- Page 2, line 46: Here the survival probability is defined in terms of fluxes ("ratio of the formation rates of particles of diameters d1 and d2") while in the other paper by the authors (acp-2022-476) it is defined in terms of concentrations. Are these the same? If not, which one is correct?
- Page 2, line 48: Doesn't GR also depend on particle phase chemistry?
- Page 5, equation 7: What are the assumptions inherent in equation 7 if there are any? Is CoagS some kind of average in the interval? And, shouldn't the last term, if written exactly, be GR x n at the upper edge of the interval?
- Page 6, lines 147-151. When estimating GR using the appearance time method, is some kind of fitting or smoothing applied to the concentration vs. time data of each channel before the peak value is stored?
- Pages 6-7, description of the model: The description is a little bit confusing. You mention that ACDC is used, but then state that source, loss and evaporation terms are set to zero. Aren't you then just solving the discrete coagulation equation? Wouldn't it be clearer to also write it that way? Also, how realistically do you think this describes events in Beijing, when Kelvin- and Raoult-type effects are neglected?
- Page 8, line 203: how do you calculate the cluster flux past a certain size?
- Page 10, line 250 and Fig. 1: For a single component system, If C_mon is constant, and Kevin-effect is neglected, condensation-theory tells that GR should be constant in the free-molecular regime, apart from a region at the smallest particle sizes where molecular dimensions also play a role. This means that the result in Case 1 in no surprise. In the other paper by the authors (acp-2022-476), however, the authors apply a size dependent growth rate (increases with size) in their simulations. Why these different approaches when studying essentially the same thing? One important question is also, if the current simulations with constant GR result in a narrowing size distribution when plotted in dN/d(logdp) form in log-scale.
- Page 10, lines 260-263: if in the high-CS cases the clusters disappear very rapidly, is I then reasonable to assume a constant monomer concentration in the simulations?
- Page 10, lines 281-282: J1.5 is almost always smaller if determined from eq. 7 than based on fluxes. Do you know why this is so?
- Page 11, line 288 and fig. 3: Please add base-case values to figure caption.
- Page 11, line 290 (and many other places): CS/GR is given as unitless? The (SI) unit of CS is 1/s and of GR m/s so how can their ratio be unitless?
- Page 13, line 361: What is a median event day? How do you determine it?
- Page 13, line 363: Should it be "…based on Eq. 13 and Eq. 14." ?
- Page 14, lines 403-405. Do van der Waals forces affect CoagS also?
- Page 15, lines 435-438: One thing important to mention about self-coagulation is also

that as a process it also effectively decreases the number concentration. If particles of diameter 3 nm grow to 6 nm by self-coagulation alone, their volume increases by a factor of 8 so that their number concentration also would decrease by the same factor.