

Responses to Reviewers' Comments on Manuscript ID egusphere-2026-4269

(New particle formation contribution to aerosol mass: regimes and implications for future urban environments)

We thank Prof. Imre Salma for handling this manuscript and the reviewers for their efforts and comments that help to improve this manuscript. The reviewers' comments are addressed in the following paragraphs and the manuscript has been revised accordingly.

The comments are shown as **sans-serif blue texts** and our responses are shown as serif black texts. Changes are **highlighted** in the revised manuscript and shown as **“quoted underlined texts”** in the responses. References are given at the end of the responses.

Reviewer #1

The manuscript entitled “New particle formation contribution to aerosol mass: regimes and implications for future urban environments” by Runlong Cai and coauthors presents an analytical approach to infer the mass contribution of nanoparticles formed by gas-to-particle conversion to aerosol loading under varying particulate pollution levels. To this end, three different regimes based on competing growth processes have been identified. In this context, condensation sink, mass accommodation coefficient, vapor source strength and number concentration of newly formed particles seem to be key parameters. The analysis suggests that under pristine background conditions like in boreal forest NPF does contribute substantially to aerosol mass, whereas in heavily polluted environments typical for East Asian megacities NPF does typically not play a pronounced role in aerosol mass concentration. However, due to the competition between condensation sink from background particles and emerging condensation sink related to the growing new mode this may eventually also allow particles from NPF to become a relevant contributor to aerosol mass. In view of decreasing primary emissions this effect may thus become important under future clean atmospheric conditions. Overall, my assessment of this manuscript is quite positive as it is well presented and the topic clearly fits the scope of the journal ACP. A few points mentioned below should, however, be considered before final publication.

Response: We thank the reviewer for nicely summarizing the main findings of this study and providing constructive comments.

One of the conclusions drawn, and also highlighted in the abstract, refers to the need of reductions in NPF precursors in order to achieve sustained reductions in aerosol mass loading. Since biogenic precursors will also contribute substantially to NPF it would be good to address pathways how such reductions in NPF precursors could be realized.

Response: Thank you, this is a very important input. Biogenic precursors can make substantial contributions to NPF. Here we focus on polluted urban environments where anthropogenic precursors are key and potentially controllable sources of NPF. These include H₂SO₄ formed from SO₂ oxidation, amines and ammonia from anthropogenic activities, and anthropogenic organic and inorganic precursors that governs new particle growth. Accordingly, reductions in

anthropogenic amines and ammonia may suppress nucleation, whereas reductions in anthropogenic organic and inorganic precursors may reduce the GR of new particles and their subsequent contribution to aerosol mass. We also acknowledge that reducing H₂SO₄ concentrations may be an effective method to suppress nucleation. However, the relationship between long-term SO₂ reductions and atmospheric H₂SO₄ concentrations is not necessarily direct (Li et al., 2020).

For instance, H₂SO₄ derived from anthropogenic SO₂ and high-concentration amines and ammonia from anthropogenic sources can efficiently form new particles, and OOMs derived from anthropogenic VOCs is an important source for new particle growth. According, we recommend potential pathways to reduce the NPF mass contribution by reducing anthropogenic precursors. This includes the reduction of amines, ammonia, anthropogenic VOCs, etc. The reduction of H₂SO₄ would be significant in terms of reducing nucleation intensity. However, as it has been reported the change in H₂SO₄ concentration appeared to be insensitive to the long-term SO₂ reduction (though SO₂ reduction also reduces the formation of sulphate), here we focus on the reduction of amine concentrations in terms of reduction nucleation intensity.

Revised text:

Abstract: “This indicates that control of anthropogenic NPF precursors may represent an effective strategy for present and future particulate matter pollution in urban environments, which warrants further investigation.”

Page 10, lines 24-26: “Potential solutions for urban environments include reducing anthropogenic NPF precursors including amine and ammonia to decrease N_{np0} and reducing anthropogenic organic and inorganic precursors (Q_v) to decrease the GR of growing new particles.”

On page 7, lines 17-23, a somewhat more detailed discussion of the meaning of α_{bg} would be appreciable. This is partly due to terminology reasons but also because of some physically controversial behavior. On the one hand the authors use α_{bg} as a parameter in their calculations for mass accommodation coefficient, on the other hand they refer to effective uptake coefficients substantially below unity. What is the relation between accommodation and uptake coefficient? Furthermore, while I can follow the arguments for reduced vapor uptake due to unfavorable surface composition, the size effect is quite counterintuitive. Kinetic effects play a much more important role at smaller (~10 nm) particle sizes than at ~100nm where particles are in the transition regime and will also (at least partly) benefit from diffusion. What justifies the assumption of such low values of α_{bg} for 100 nm particles? Have some of the papers from the referenced literature (or other literature) seen such size dependent effects from experiments (decreasing mass accommodation coefficient with increasing size)?

Response: The reviewer addressed an interesting topic on the underlying reason of the α_{bg} below unity. First, on the relationship between mass accommodation coefficient (α) and uptake coefficient (γ), they characterize related though different aspects of vapor uptake. Their relationship can be expressed as (Kulmala and Wagner, 2001):

$$\gamma = \frac{4}{3} \cdot \frac{p_{v,\infty} - p_{v,a}}{p_{v,\infty}} \cdot Kn \cdot \beta_m(\alpha)$$

where β_m is the transition regime correction factor. γ is always no larger than α , and it deviates from α for small Kn values (i.e., large particles) where gas-phase diffusion also suppresses vapor uptake.

In the revised manuscript, we have replaced “effective uptake coefficient” with “ α_{bg} ” to avoid unnecessary confusion, and explained the physical meaning of α_{bg} used herein to avoid potential confusions. Actually, interfacial mass accommodation was used in the cited reference (Zaveri et al., 2022) instead of uptake coefficient, though it is focused on the interfacial mass transfer step and is defined differently from α_{bg} used in this manuscript. This revision does not affect the correctness of this sentence and improves its clarity among the content.

Second, on the reason of α_{bg} below unity, we provide a plausible explanation that particle-phase diffusion may impose an additional resistance to net condensational growth (Zaveri et al., 2014), and this is reflected as an ineffective mass accommodation. As particles grow large, semi-volatile vapor that condense onto the particle surface cannot rapidly mix within the viscous bulk, leading to a higher surface concentration than the bulk-average concentration and thereby retard vapor uptake. For sub-25 nm particles, atmospheric measurements have shown that the average size-dependent GR has a positive correlation with particle size, which is consistent with the Kelvin effect. For particles larger than 25 nm, measurements at different sites show that particles grow slower than the rate expected with a constant mass accommodation coefficient (see Fig. R1 below, corresponding to a power of 0 for GR versus d_p). Such a behavior has not been explicitly reported and discussed in literature, and we have an ongoing study to address this behaviour from both experimental and theoretical aspects.

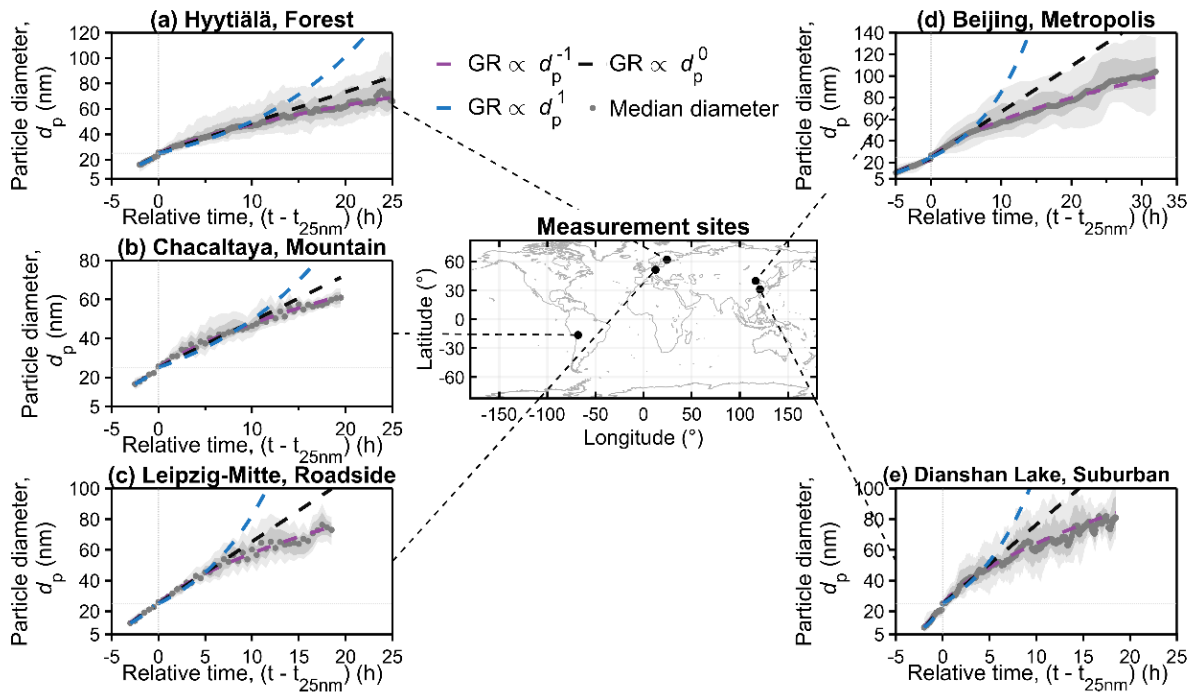


Figure R1. Dependence of particle growth rate on particle diameter. For kinetic-limited condensation processes, GR is expected to be constant in the size range of interest (i.e., $GR \propto d_p^0$). However, atmospheric measurement from different environments indicates that the average GR decreases as particles grow large.

In the revised manuscript, we added discussions to address the potential cause of $\alpha_{bg} < 1$.

Page 8, lines 15-17: “This may reflect an ineffective vapor uptake of large background particles characterized by α_{bg} below unity, i.e., vapor molecules colliding with the particle surface are not effectively incorporated into the bulk.”

For instance, the importance of particle-phase diffusion increases with particle size and may retard vapor uptake of large particles (Zaveri et al., 2014)."

Conclusions, page 8, line 17: To me the relation of high mass from new particles and CCN fraction is not clear. The impact of NPF on CCN is usually argued via number concentration. I guess a significant mass contribution under conditions representative for Hyytiälä is rather due to low CS/background. Some clarifying sentence here might help the reader in better understanding this point.

Response: Thanks. We have added a sentence to explain the relation between high mass fraction and high CCN fraction. The main reason is that both CCN and aerosol mass require new particles to grow and survive to large sizes against coagulation scavenging. The underlying reason is that this high contribution requires a low background aerosol concentration (in number, CS, and mass), as the reviewer has pointed out.

Revised text

Page 9, lines 18-20: "This is qualitatively consistent with previous estimates that NPF accounts for a substantial fraction of cloud condensation nuclei in boreal forest environments (Tunved et al., 2006; Gordon et al., 2017; Petäjä et al., 2022), where low background aerosol concentration favors the growth and survival of new particles to sizes that are relevant to cloud condensation nuclei."

Editorial remarks/suggestions:

Abstract, page 1, line 14: "...NPF events in urban Beijing occur..."

Page 2, line 5 and page 6, line 2: I guess the word "competence" should be replaced by "competition"

Introduction, page 1, line 30: "...particle mode, after sufficient growth, and background..."

Introduction, page 1, line 31: The authors refer to "a prevailing hypothesis". Is there a reference for this hypothesis?

Section 2.2, page 3, line 30: "covert" --> "convert"

Section 2.2, page 3, line 37: "... contributes negligibly due to..."

Section 3.1, page 4, line 21/22: "...reaching sizes (e.g., ~100 nm) that significantly contribute to aerosol mass after growth..."

Figure caption Fig. 1, page 5, line 10: model input parameters are presented. Are these values representative of a certain environment (e.g., Hyytiälä)? Or what were the reasons to choose these numbers?

Figure caption Fig. 2, page 7, line 1: model parameters are mostly very similar to values in Fig. 1 except for N_{np0} which is higher by one order of magnitude. Is this correct or a typo?

Page 7, line 30: "Event" --> "Even"

Page 7, line 33: "which whether" does not make sense. Please reformulate.

Figure caption Fig. 3, page 8, line 5: “Statistical results of measured NPF events in relation to N and CS.”

Conclusions, page 8, line 15: would there be a reference to support the statement: “success in previous control of aerosol mass by reducing primary emissions”?

Conclusions, page 8, line 24: “... feedback is suppressed by the..”

Response: We thank the reviewer for these editorial remarks. The manuscript has been revised accordingly. We have checked the spelling and grammar of the whole manuscript and revised other typos.

For “a prevailing hypothesis” in Introduction, it is proposed by a number of colleagues during personal discussions and peer-review processes. We therefore remove the word “prevailing”.

For the input values for Fig. 1, we have added a sentence to clarify that “The value of N_{np0} , CS_{bg0} , and the derived GR represents typical values at Hyytiälä.”

For the caption of Fig. 2, it was a typo and has been corrected.

For conclusion, we have added Zhang et al. (2019) to support that statement “success in previous control of aerosol mass by reducing primary emissions”.

Reviewer #2

The manuscript "New particle formation contribution to aerosol mass: regimes and implications for future urban environments" by Cai et al. proposes a conceptual regime framework for quantifying the contribution of new particle formation (NPF) to atmospheric aerosol mass, using a zero-dimensional aerosol dynamic model constrained by long-term observations from two different locales: urban Beijing and the Finnish boreal forest site at Hyytiälä. This work is important because it addresses a long-standing and genuinely open question of whether NPF events meaningfully contribute to aerosol mass or whether condensable vapors would simply partition onto background particles regardless. As such, it offers a useful framework that maps NPF mass contributions across regimes governed primarily by the background condensation sink.

The authors' transparency about the simplifying assumptions underlying their model, including the constant vapor source hypothesis and the choice of mass accommodation coefficient, is appreciated and adds clarity to the scope of the work. However, in several key places the conclusions, particularly those with direct policy implications, are stated with a level of confidence that is difficult to fully justify given the acknowledged limitations of the modeling framework. Overall, this study presents a valuable conceptual contribution that merits publication, but requires major revisions to better align the strength of its conclusions with the evidence provided, as described in detail below.

Response: We thank the reviewer for summarizing the main findings of this study and providing fair and constructive comments. The manuscript has been revised to better clarify the scope of this study and address the uncertainties of the results. Point-to-point responses are given below.

1. The abstract currently states that "control of NPF precursors represents an effective strategy for present and further particulate matter pollution." This is a direct policy recommendation derived from a model using a constant vapor source that the authors explicitly call "not supported by atmospheric measurements." The authors must qualify this statement, for example, framing it as a hypothesis warranting further investigation rather than a prescriptive conclusion. Similarly, the claim in the conclusions that "ongoing reductions in primary aerosol emissions, if not accompanied by equivalent reductions in NPF precursors, may disproportionately amplify NPF contributions to particle mass" should be clearly framed as model-dependent and conditional, not as a robust projection.

Response: We revised the abstract and conclusions to better clarify that the conclusions of this study was derived from a model and warrants further investigation. As the reviewer noted, this study is aimed to provide a framework mapping NPF mass contributions and regimes. Measurements constraints from different sites and 3-dimensional model simulations are needed in further studies to provide stronger conclusions.

Revised abstract and conclusions:

Abstract: "Our simulations suggest that NPF mass contribution in urban environments with a declining CS_{bg} will shift toward regimes I and II. This indicates that control of anthropogenic NPF precursors may represent an effective strategy for present and future particulate matter pollution in urban environments, which warrants further investigation."

Conclusion , page 10, lines 21-24: "This finding derived from our simulations suggests that ongoing reductions in primary aerosol emissions, if not accompanied by equivalent reductions in NPF precursors, may disproportionately amplify NPF contributions to particle mass. Further studies with more observational constraints are needed to better assess the NPF mass contribution and its importance in different environments."

We would like to clarify the context of the statement "not supported by atmospheric measurements". This statement refers to the constant vapor source assumption itself, which does not affect the main conclusion of this study. We used a constant vapor source as an unfavorable/conservative scenario for NPF mass contribution. Under this scenario, the growth of new particles is suppressed due to the consumption of condensable vapors by large background particles. However, our measurement results show that new particle growth rate appears to be insensitive to atmospheric environments, as explained by the sentence immediately following that statement. Therefore, the main conclusion of this study was not derived from a constant vapor source.

2. The default value of $\alpha_{bg} = 0.1$ is used throughout most of the main simulations, yet laboratory measurements cited in the paper itself span 0.01 to 1.0, and Liu et al. (2019), which is also cited, reports near-unity values for diverse aerosol types. Figure S11 shows clearly that the regime boundaries and NPF mass contribution ratios shift substantially across this range. The authors should either provide a stronger atmospheric justification for $\alpha_{bg} = 0.1$ as a representative default, or present results more symmetrically across the full measured range rather than treating 0.1 as the primary scenario. As it stands, the default choice systematically favors larger NPF contributions relative to what higher α_{bg} values would produce.

Response: Thanks. Our choice of $\alpha_{bg} = 0.1$ as a default value is based on atmospheric measurements. Specifically, our long-term atmospheric measurements show that large background particles tend to grow more slowly than newly formed particles. This is also consistent with some 3-dimensional modeling studies that use low α values to reproduce atmospheric

measurements (e.g., Zaveri et al., 2022). Therefore, we consider $\alpha_{bg} = 0.1$ to be a representative default for atmospheric conditions, while recognizing that higher values are possible. To clarify this, we added the following sentence to the revised manuscript:

Page 8, lines 19-21, “Considering that our atmospheric measurement results show that background particles grow slower than new particles, we take $\alpha_{bg} = 0.1$ as a representative default input for simulation and explore its influences on the mass contribution.”

While we are not aware of published studies that explicitly report on the slower growth of larger particles, this behavior is supported by our long-term observations. Here we present preliminary results from an ongoing study to support this statement.

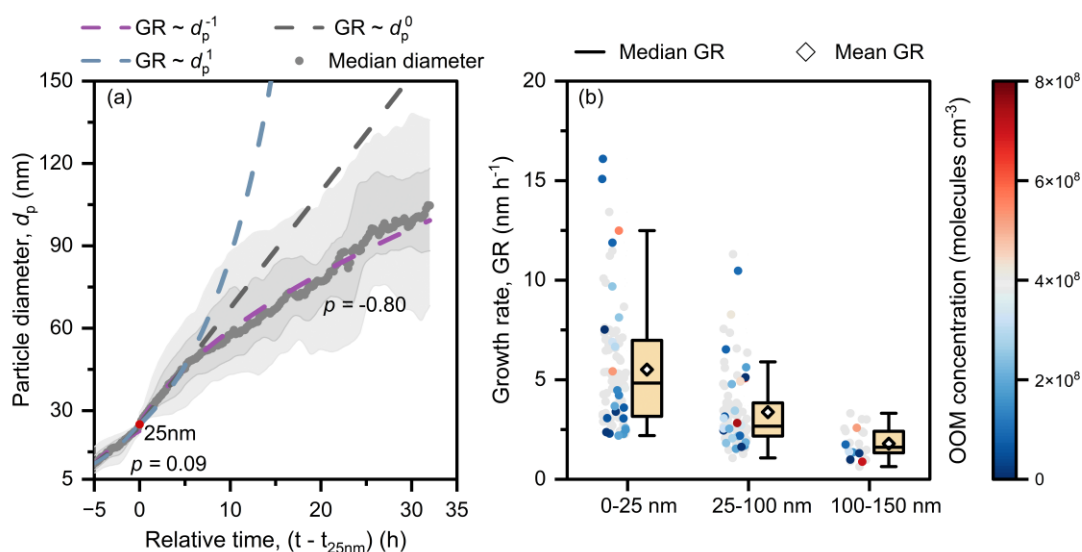


Figure R2. a) Average growth trajectory and b) size-dependent particle rate measured from urban Beijing. If particle growth is kinetically limited by gas-phase diffusion (i.e., vapor-particle collision rate), the growth rate should be near constant ($GR \propto d_p^0$) in the sub-100 nm size range. However, measurements shows that larger particles tend to grow slower than smaller ones.

3. In Section 3.1, the authors report average mass concentrations of 39.7 $\mu\text{g}/\text{m}^3$ for the growing mode in Beijing, with values approaching $\sim 100 \mu\text{g}/\text{m}^3$ during severe haze, and use these figures to motivate NPF's significance to aerosol mass. However, just a few sentences later they acknowledge that "particles in the observed growing mode also originate from primary particle emissions and transport." These two statements are in direct conflict. The authors should more explicitly caveat the observational statistics in Figures S1 and S2, clarifying that these mass concentrations cannot be cleanly attributed to NPF alone. Presenting them as upper-bound estimates of NPF-relevant particle mass, rather than as direct NPF mass contributions, would be more scientifically defensible.

Response: We thank the reviewer for pointing out this ambiguity in our presentation. We have added sentences to both the main text and the SI figure captions to clarify that the numbers reported here should not be taken as the contributions from NPF alone.

Revised text:

Page 5, lines 10-11, “While particles in the observed growing mode also originate from primary particle emissions and transport, i.e., the number and mass contributions reported above should not be taken as from NPF alone, these observations confirm that NPF events, under favorable conditions, can produce particles with sizes that are relevant to aerosol surface area and mass.”

Figure S1, “Note that particles in this observed growing mode also originate from primary particle emissions and transport, and hence the statistical results represent upper-bound estimates of NPF-relevant particle number and mass concentrations.”

4. The authors use the scatter in Figure S4, which spans roughly three orders of magnitude in growth rate, to conclude that “the rate of gas-to-particle conversion is not majorly limited by the competence of vapor between the two modes.” This interpretation is then used to justify the constant vapor source assumption that underpins the entire model. However, the large scatter in Figure S4 is equally consistent with day-to-day variability in vapor sources, photochemical activity, or primary emission influences on particle sizing. The authors should acknowledge these alternative explanations and avoid using this ambiguous dataset as primary justification for a core modeling assumption.

Response: We understand your argument in this comment. We have added clarifications to the revised SI to acknowledge that the variation in other parameters could be reasons for the weak correlation between the measured GR and CS. We now state that this weak correlation indicates that the competition of vapor between the two modes is not the governing factor for the rate of gas-to-particle conversion. That is, this does not exclude the importance of the competition of vapor between two mode, yet its shows that this competition does not overwhelm the importance of other factors. We have note the relative uncertainty of a low GR ($< 0.1 \text{ nm h}^{-1}$) is expected to be large, yet we prefer to keep Fig. S4 in logrimathic instead of linear scales for better visualization of events with small GR values.

Revised text:

Page 6, line 19, “Figure S4 shows that there is no strong correlation between new particle growth rate and the background CS, indicating that the competition of vapor between the two modes is not the governing factor for the rate of gas-to-particle conversion.”

Figure S4 caption, “Other factors, such as day-to-day variability in vapor sources, photochemical activity, and primary emissions, also influence the growth rate.”

5. The thresholds of $\sim 0.001 \text{ /s}$ and $\sim 0.01 \text{ /s}$ separating Regimes I, II, and III are discussed throughout the paper as though they are generalizable atmospheric benchmarks. However, Figure S11 demonstrates clearly that these boundaries shift substantially with α_{bg} and Q_v . The authors should more prominently and consistently qualify these thresholds as being specific to the parameter choices used in the main simulations, and caution against their direct application to other environments without recalibration.

Response: The manuscript has been revised as suggested to address the influence of other factors on the threshold values. This influence has been proposed in the original manuscript, yet we try to improve its clarity in the revised version. We have also clarified that the threshold values may need re-evaluation when being applied to other environments.

Revised text:

Abstract: “Among these influencing factors, the initial CS_{bg} governs NPF mass contribution under the simulation conditions; NPF contributes significantly to aerosol surface area and mass at an initial CS_{bg} of $\sim 0.001 \text{ s}^{-1}$ (regime I).....”

Page 8, line 35-39, “Note that although CS_{bg0} governs the threshold separating these three regimes in the simulation results, Figure S11 shows that the threshold values are also affected by Q_v and α_{bg} . While Fig. S12 validates this three-regime structure for conditions with a constant vapor concentration (rather than a constant Q_v), the threshold values may need re-evaluation when applied to other conditions and environments.”

6. Currently, limitations are scattered throughout the text -- the unsupported vapor source assumption appears briefly in Section 3.1, the α_{bg} sensitivity is discussed in Section 3.2, and the mode separation problem appears in the Introduction. A dedicated limitations section would consolidate these acknowledgments and help ensure readers appropriately contextualize the conclusions. This section should explicitly address: the zero-dimensional model's inability to capture spatial heterogeneity and boundary layer dynamics; the constant vapor source assumption and its known departure from observations; the single-vapor representation; the fixed 48-hour simulation window with constant inputs; and the restriction of observational validation to just two sites.

Response: Thanks. This study aims to provide a framework to address whether NPF may contribute to aerosol mass concentration, and our main findings are not affected by these limitations. However, we agree with the reviewer that adding a new section explicitly stating these simplifications and assumptions improves the clarity of this manuscript and helps readers appropriately contextualize the conclusions. Hence, we have added a new section 2.3 to synthesize the uncertainties and limitations.

Revised text:

“2.3 Uncertainties and limitations

The simulation results are derived based on a series of simplifications and assumptions. While these simplifications do not affect the proposed regime framework, they may lead to uncertainties in the predicted NPF mass contributions. Here we summarize the limitations of the model.

The zero-dimensional model assesses NPF processes within a hypothesized well-mixed air mass with uniform particle and vapor concentrations. It cannot resolve the influences of spatial heterogeneity and the diurnal evolution of the boundary layer on the NPF mass contributions. The vapor source Q_v is assumed to be constant over the simulation period, whereas it is expected to vary diurnally and from day to day due to the variations in primary emissions, photochemical activity, and transport. Condensable vapors are represented by a single hypothetical species, which may not capture the synergistic effects of multicomponent condensation, volatility-dependent vapor uptake, and the possibility that different vapors dominate growth at different sizes or growth periods. The simulations use a fixed 48-h window, while the window for new particle growth during each episode varies with events. The observation results used to validate the input parameters for simulation are limited to two sites, which represent polluted and relatively clean environments but do not span the full range of atmospheric conditions and emission regimes. Extrapolating the results to other environments (e.g., marine, free-tropospheric, or polar atmospheres) should be made with caution. Overall, these uncertainties and limitations do not eliminate the main conclusion that NPF can contribute non-negligibly to aerosol

surface area and mass under favorable conditions, but the reported mass contributions may need to be refined by more observational constraints and three-dimensional simulations.”

7. Given that the constant vapor source is explicitly acknowledged as a "conservative lower bound" and "not supported by atmospheric measurements," the authors should include at minimum a systematic sensitivity analysis testing how the regime boundaries and NPF mass contributions change under a range of vapor source scenarios, including temporally varying sources more representative of real urban diurnal cycles. This would help quantify the uncertainty introduced by this assumption and give readers a clearer sense of whether the conclusions are robust.

Response: We have added a sensitivity analysis in Fig. S12 to test the robustness of the three-regime structure. We agree with the reviewer that temporally varying sources are more representative of real urban environments than a constant vapor source. However, we do not expect adding including such variability would substantially alter the three-regime structure or its threshold values. Actually, we emphasize the “constant” vapor source on its limitation on the total vapor availability (rather than the variability), while such a limitation is supported by atmospheric measurements of particle growth rate. To test the uncertainty associated with this assumption, the original manuscript already included Fig. S5, which shows a significant NPF mass contribution under a constant vapor concentration. In the revised manuscript, we have added Fig. S12 to show that the three-regime structure also holds under the assumption of constant vapor concentration. With these sensitivity test, which lead to a large difference in simulated/input vapor concentrations under the two assumptions, we consider the three-regime structure to be robust, although the threshold values may needs re-evaluation when applied to different environments, as discussed in our response to previous comment.

To test the uncertainty associated with this assumption, the original manuscript already included Fig. S5, which shows a significant NPF mass contribution under a constant vapor concentration. In the revised manuscript, we have added Fig. S12 to show that the three-regime structure also holds under the constant-vapor-concentration assumption. Because these sensitivity tests span a large range of simulated and input vapor concentrations, we consider the three-regime structure to be robust, although the thresholds may need re-evaluation when applied to different environments, as discussed in our response to comment 1.

Revised text:

Page 8, line 35-39, “Note that although CS_{bg0} governs the threshold separating these three regimes in the simulation results, Figure S11 shows that the threshold values are also affected by Q_v and α_{bg} . While Fig. S12 validates this three-regime structure for conditions with a constant vapor concentration (rather than a constant Q_v), the threshold values may need re-evaluation when applied to other conditions and environments.”

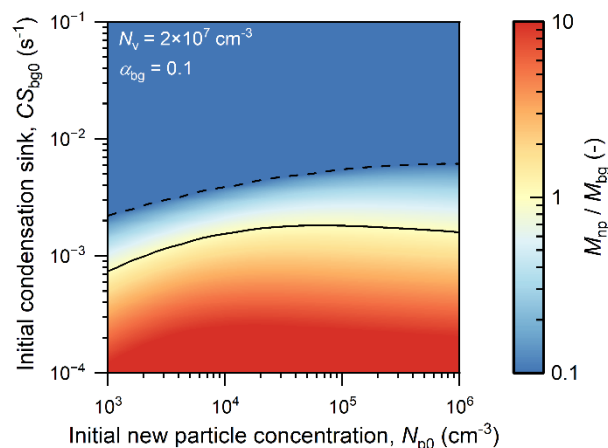


Figure S12. Contribution of the new particle mode to particle mass concentration after 48 h with a constant condensable vapor concentration. Different from Fig. S11, which uses a constant vapor source, a constant vapor concentration is used here.

8. The conclusions and Figure 4 extrapolate the regime framework broadly to "future urban environments" including East Asia and South Asia. However, the observational constraints come from only two sites -- urban Beijing and the Finnish boreal forest -- which represent very specific atmospheric regimes. The authors should either explicitly limit their geographic claims to environments similar to those studied, or provide some discussion of whether the regime boundaries and framework could be expected to hold under the substantially different emissions, chemistry, and meteorology of South Asian cities, European urban environments, or tropical regions.

Response: We have limited the discussion to East Asia, as the measurements reported and cited in this study are mainly from East Asia cities. In terms of future urban environments, we have added discussions to address that other environments needs more measurement constraints. In terms of the three-regime structure, we consider is to be valid for various regions as it is not sensitive to the underlying differences in emissions and chemistry (as indicated by the sensitivity analysis of Q_v/N_v , α_{bg} , and N_{p0}). In terms of future environments, it is reasonable to expect lower anthropogenic emissions in the long-run, while the response of NPF to emission reduction and the importance of anthropogenic sources to NPF may vary with environments.

Revised text:

Page 10, line 4 "These results carry direct implications for interpreting the importance of NPF to air quality under different atmospheric conditions (Fig. 4). In present-day polluted environments such as those in East Asia,....."

Page 10, lines 20-25, "As a result, the NPF contribution in these environments will shift from regimes II-III towards I-II, offsetting the decrease in total particle mass..... Further studies with more observational constraints are needed to better assess the NPF mass contribution and its importance in different environments."

Minor errors that need correction:

Abstract, line 14: "NPF events in urban Beijing occurs mostly" should be "occur mostly."

Abstract, line 16: "events at a Finnish forest site falls in regimes I and II" should be "fall in regimes I and II."

Abstract, line 17 (and repeated in the Conclusions): "an effective strategy for present and further particulate matter

pollution": "further" should be "future."

Section 2.1, p.2: "Measurements in Beijing was conducted" should be "were conducted." The same error appears one sentence later: "Measurements at the Finnish boreal forest was conducted" again should be "were conducted."

Section 2.1, p.2: "provide constrains on typical conditions" should be "constraints."

Section 2.1, p.3: "characterized by a dramatical change in the mode diameter": "dramatical" should be "dramatic."

Section 1 (Introduction), p.4: "vapor uptake is kinetically influenced process" is missing an article; should be "is a kinetically influenced process."

Section 1, p.4: "vapor uptake by large background particles if often retarded": "if" should be "is."

Section 2.2, p.6: "we do not peruse a closed-formed analytical solution": "peruse" should be "pursue," and "closed-formed" should be "closed-form."

Section 3.2, p.7: "Event with the most favorable α_{bg} (0.01)": "Event" should be "Even."

Section 3.2, p.7: "Under such condensations, new particles are rapidly removed": "condensations" should be "conditions."

Sections 3.1 and 4 (pp.6 and 8, appearing twice): "not majorly limited by the competence of vapor between the two modes": "competence" should be "competition."

Section 4, p.8: "this feedback is suppressed the dominant CSbg" should be "suppressed by the dominant CSbg."

Section 4, p.9: "may not decrease with the same extend to that in CSbg0": "extend" should be "extent," and "to" should be "as" ("the same extent as that in CSbg0.")

Figure 2 caption (main text) and Figure S10 caption (supplementary): "Accomodation coefficient" should be "Accommodation"

Supplementary Figure S3 caption: "Measured evolution of atmospheric particles at on Sep. 8-10, 2019" should be "on Sep. 8-10, 2019."

Response: We thank the reviewer for these editorial remarks. The manuscript has been revised accordingly. We have checked the spelling and grammar of the whole manuscript and revised other typos.

References:

- Cheng, J., Su, J., Cui, T., Li, X., Dong, X., Sun, F., Yang, Y., Tong, D., Zheng, Y., Li, Y., Li, J., Zhang, Q., and He, K.: Dominant role of emission reduction in PM_{2.5} air quality improvement in Beijing during 2013–2017: a model-based decomposition analysis, *Atmos. Chem. Phys.*, 19, 6125–6146, 10.5194/acp-19-6125-2019, 2019.
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