RE: A point-to-point response to editor's comments

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Title: Organic condensation derived from different volatility parameterizations and its implications for ambient nanoparticle growth

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Dear James,

Thank you very much for your comments. We have addressed all your comments in detail and specified the corresponding revisions made in the revised manuscript. In the following response, your comments are reproduced in *italics*, our responses are provided in normal font, and all changes in the revised manuscript are highlighted in blue.

We thank you again for your time and consideration!

Yours sincerely,

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Comments:

1. The authors have addressed the reviewers comments and I am happy this can be moved to publication, however I first request that the conclusions be reformatted slightly in order to meet the journal's style guidelines (https://www.atmospheric-chemistry-and-physics.net/policies/guidelines_for_authors.html). These specify that the conclusions should be 'concise' however in its current form I feel it it too long. Some of the discussion of the research outlook in particular goes beyond the immediate implications of the work presented, so would be better suited in the previous section.

Response:

We sincerely appreciate your positive feedback of our manuscript. The “Conclusions” section has been reformatted to comply the journal's style. In addition, this section has been revised and condensed to more clearly represent the main findings, limitations and implications of our work and to provide a cautious outlook based on our resluts.

In our revised manuscript, the “Conclusions” section reads as follows (lines 369-399):

“Conclusions and outlook

This study examined whether condensation of H_2SO_4 and OOMs alone can account for the rapid growth of freshly nucleated nanoparticles. Using a laminar flow reactor that cleanly separates particle–particle coagulation from OOM condensation, we benchmarked seven widely used volatility parameterizations directly against observed growth. Across diverse OOM precursors (α-pinene and toluene), seed nanoparticle sizes (3-5 nm), and NOx regimes, six of the seven overestimated nanoparticle growth-most severely under the high-NOx conditions typical of polluted urban air-whereas only a parameterization constrained by ambient organic-aerosol volatility (i.e., Zhang et al., 2026) reproduced our observations.

The overestimation arises because most existing parameterizations were derived from, or tuned to, biogenic, low-NOx oxidation systems dominated by CHO compounds, and they tend to assign lower volatilities to the OOM populations generated under high-NOx, anthropogenically influenced conditions. Such conditions favour nitrogen-containing products (e.g., organonitrates) and fragmentation pathways that shift the volatility distribution toward higher volatility, so that schemes calibrated on biogenic reaction systems overstate the condensation flux of organics. This interpretation is supported by the fact that the Zhang et al. (2026) parameterization, which is constrained by directly measured volatilities of ambient urban and suburban particulate OOMs, alone reproduces our observations. The fundamental insight is therefore not that any individual parameterization is inherently deficient, but that the volatility-composition relationship is sensitive to the chemical regime in which it is calibrated, particularly the prevailing NOx level.

Whether the underprediction we observe at Lake Tai extends to other environments cannot be settled by a single campaign, and the performance of any volatility parameterization is expected to be regime-dependent. Our laboratory results nonetheless argue against a fixed low bias in the Zhang et al. (2026) scheme: under low-NOx α-pinene + O_3 conditions, it slightly overpredicted rather than underpredicted growth (Fig. 3a and 3c), so a systematic underprediction in a clean, BVOC-dominated environment such as Hyytiälä is not anticipated. Conversely, the scheme was derived from ambient particulate OOMs collected in a polluted region of eastern China and is best supported for NOx-rich, mixed anthropogenic–biogenic conditions; its extrapolation to pristine biogenic or marine environments remains to be tested. The residual growth at Lake Tai is therefore more plausibly a property of the site (i.e., elevated NOx, abundant anthropogenic precursors and reactive gas–particle chemistry) than a generic feature of the parameterization.

These caveats notwithstanding, the results caution against applying volatility frameworks calibrated largely on biogenic, clean-environment chemistry to polluted, high-NOx atmospheres, where such frameworks may overstate the organic contribution to early growth and thereby bias estimates of CCN abundance and aerosol–cloud radiative forcing. Establishing a transferable, mechanistically complete description of nanoparticle growth will require extending these co-evaluated laboratory and field analyses to diverse polluted atmospheres, alongside direct volatility measurements and explicit kinetic treatment of particle-phase heterogeneous reactions."

2. On more minor points, I spotted the typo "applyingthis" in the conclusions, and also the data can now be made available and the doi added.

Response:

This typo "applyingthis" has been corrected. The data associated with this manuscript have been made available online, and the dataset DOI has been added.

In our revised manuscript, the “Data availability” section reads as follows (lines 400-403):

“Data availability

The data used to support the conclusions in this study are available at a public data repository of Zenodo via https://doi.org/10.5281/zenodo.23737227. Additional data related to this paper can be requested from the corresponding authors (lei_yao@fudan.edu.cn; lin_wang@fudan.edu.cn).”