Review Report for ACP

Contrail formation for aircraft with hydrogen combustion – Part 1: A systematic microphysical investigation

Zink, Unterstrasser & Burkhardt (EGUsphere preprint, 2025)

The authors perform a comprehensive suite of Lagrangian Cloud Model (LCM) simulations to study ice nucleation on ambient aerosols in hydrogen combustion plumes. They quantify the negligible influence of coarse-mode aerosols, propose a weighted-mean scaling to approximate multi-population behavior, and derive a conservative boundary (Eq. 8) to determine when HDN can be neglected. Furthermore, they identify a parameter subspace (temperature $\leq$ 225 K, 10–100 nm aerosol size) where ice number formation ($N_{\mathrm{ice},f}$) becomes insensitive to aerosol properties, thus facilitating parametrization.

General comments

The manuscript is scientifically valuable, presenting a thorough microphysical modelling study of contrail formation for hydrogen combustion and explores important conceptual simplifications for parametrization development. However, issues regarding the structure, novelty, model validation, assumptions, and clarity of applicability need to be addressed before publication.

Specifically, the work is well executed, carefully documented, and useful for the contrail/aviation community. However, it mostly reports comprehensive application of an existing particle-based Lagrangian Cloud Module and a large parameter sweep; it does not introduce a clear, novel physical mechanism in atmospheric chemistry or cloud microphysics. As such, in its present form it reads as an important and careful modeling/data paper but falls short of the level of novel atmospheric physics/chemistry unless the authors: (a) clarify and amplify the particular microphysical insight(s) that are genuinely new; (b) add further validation/uncertainty quantification; and (c) improve the argument that the identified insensitive subspace is a new physical result rather than an empirical property of this model/setup. As a consequence, the authors should note that splitting the research into 3 manuscripts is acceptable only if each part contains a clear, stand-alone novelty claim. As written, Part 1 is a thorough model-based analysis, and may be regarded as insufficiently novel for ACP on its own.

We elaborate this general comment with further specific comments below:

Specific comments [6pt]

1. Scope and Contribution: While the manuscript is scientifically valuable and the discussion of results is thorough, the novelty appears circumscribed to systematically running an existing model (Bier et al., 2024). It feels more like a follow-up study rather than a substantial, standalone modelling contribution. The authors mention splitting the analysis into three parts. I am unconvinced that the current contribution is sufficient to justify a trilogy of papers. I strongly suggest the authors consider compressing the work into one or two meaningful papers. For instance, the third part (building an AI-based surrogate model) seems to offer the distinct modelling contribution that is currently lacking in this first part; integrating these findings could significantly strengthen the publication.

2. Introduction and Motivation: The introduction is currently too long and fragmented. The critique of the state-of-the-art is not sufficiently connected to the specific scientific contributions of this study. Furthermore, the motivation needs to be more sharply focused on the aviation industry's context.

3. Outdated Context: The reliance on the Airbus (2020) reference for motivation feels outdated. In 2020, the target for service entry was around 2035. However, as of 2025, timelines have shifted towards 2040–2045 due to infrastructure challenges (e.g., lack of hydrogen infrastructure at airports). Indeed, I have the feeling that the H_2 aircraft is not a priority anymore... In any case, please, try to reflect the current industrial reality to ensure the motivation is robust and up-to-date.

4. The study is purely model-based, with no quantitative validation. Although hydrogen contrails are not yet observed, consistency checks with kerosene contrails or known ranges of $N_{\mathrm{ice},f}$ would be recommended. Alternatively, explicitly state the limits of observational validation for hydrogen combustion and discuss implications for model uncertainty.

5. Include sensitivity runs or describe variability when other subsets or entrainment efficiencies are used (now, the entrainment efficiency seems to be fixed in the paper).

6. Criterion for HDN relevance (Eq. 8) should be further discussed. The conservative criterion ($J_{\mathrm{HDN}}, n_{\mathrm{aer}} = 0 = 10^6\,\mathrm{m}^{-3}\mathrm{s}^{-1}$) is useful but requires clearer justification. Specifically, it is recommended to explain the physical rationale for this threshold and demonstrate how outcomes vary if it changes by an order of magnitude.

7. Further discuss/quantify how neglecting vapor depletion or droplet interactions may overestimate HDN frequency. The exclusion of lubrication-oil or NOx-product aerosols may limit generality. In this respect, it is recommended to discuss how their presence could modify the current conclusions, especially the negligible coarse-mode effect.

8. The paper neglects the coarse-mode particles without quantified depletion analysis. The manuscript asserts these particles can be ignored when they are orders of magnitude less numerous, but provides no order-of-magnitude vapor budget demonstrating when coarse particles materially affect plume microphysics (a simple calculation comparing potential water uptake per coarse particle to plume-available vapor is recommended).

9. Weighted-mean scaling is presented without conditions for linear applicability. The linear weighted-mean reconstruction around Eq. (3) is supported empirically but without a mathematical criterion (e.g., small-depletion limit, timescale separation, or inequality) that explains when nonlinear vapor-competition effects can be ignored.

10. The use of an isoline ($\max(J_{\mathrm{HDN}}) = 10^6\,\mathrm{m}^{-3}\mathrm{s}^{-1}$) in the HDN discussion remains qualitative because J (which is a rate) is not integrated into expected droplet number or vapor removal over plume lifetimes.

11. When mentioning the planned neural-network parametrization (Part 3), briefly discuss how physical constraints (e.g., monotonicity, conservation) will be preserved.

12. To facilitate usage by third parties and accelerate the community's understanding of the problem, could the authors clarify the availability of the model used for these simulations (in the original papers, it is stated that the data are available upon request to the corresponding author)? Are there plans to release the model as open-source? I'm saying this because the paper rests on the running of a model. If the model would be open-source, then it is accessible to the entire scientific community, facilitating the reproduction and intercomparison of results. The authors mention about the availability of the data upon request to the author, which is fair, though current practices within Horizon Europe and national science programs are moving into the direction of publishing the data and making them findable, accesible, interoperable, etc. Please, consider these aspects. In the end, this enhances the impact of the research and supports the sharing of knowledge.