

We thank Reviewer 1 for the additional comments and constructive suggestions. We also thank Reviewer 3 for serving as a new reviewer and for the valuable remarks provided. We have addressed all comments and revised the manuscript accordingly.

In this response document, reviewer comments are reproduced in black. Our replies are provided in green, and references to the manuscript are indicated by '*green italicized text in quotation marks*'.

Reviewer 1:

I would like to thank the authors for taking time to carefully address each of my specific comments in detail. Concerning my general comments, the authors have opted to restructure the manuscript by relocating various parts into a dedicated results section. This now more clearly differentiates the model description from the findings obtained using the model and is an effective change.

Nevertheless, I have one additional (general) suggestion that I believe would further improve the manuscript which concerns the presentation of the results.

To be most useful to the wider scientific community, I find that the most applicable findings could be stated more clearly in the results section. As an example, my understanding is that the key result in Sects. 3.11 and 3.12 is that (when simulating contrails formed by hydrogen combustion) the complexity in the prescribed ambient aerosol particle size distribution can be reduced straightforwardly by: (a) neglecting contributions from coarse mode aerosol and (b) preparing estimated ice crystal numbers using a weighted mean approach. These are very useful observations for future modelling studies, however I find that while these are justified at length in the text, their general applicability is not emphasized in the discussion/results section.

Note that while I have chosen the case of Sects. 3.11 and 3.12, I find that the same is true of the results presented in Sect 3.2 and 3.2.2. Therefore, I would advise that the authors consider: (i) placing a sentence at the end of each results section with the key result and (ii) emphasizing (potentially in the conclusions) that their findings are more generally applicable to all contrail simulations for hydrogen combustion (see the bullet points in Sect. 5), if this is indeed the case.

We thank the reviewer for this additional comment. In the first review round, we already undertook substantial revisions to place greater emphasis on the physical interpretation of our results and to improve the clarity of the model–result separation, including the addition of new figures illustrating key processes. This restructuring also explains the comparatively detailed discussion in some sections, as we aimed to highlight the governing microphysical mechanisms rather than present purely empirical model outcomes. We are convinced that the results presented in Sects. 3.1.1 and 3.1.2, in particular, provide general insight into the microphysical processes of contrail formation for hydrogen combustion and should therefore be interpreted as physically grounded findings rather than model-specific observations. While these results ultimately allow a reduction of parameter space complexity, this simplification is physically motivated.

We note that the key findings identified by the reviewer are already explicitly stated at the end of the respective results sections. We have revisited these passages and made only minor adjustments to further clarify their physical basis and general relevance, but found that the main conclusions were already formulated accordingly.

At the end of Sect. 3.1.1:

'Therefore, if the coarse mode particles are two or more orders of magnitude fewer in number than another particle mode -as is typically the case in the UT (see Sect. 2.2)- we can safely neglect them in the ice crystal formation process.'

At the end of Sect. 3.1.2:

'The success of the weighted-mean approach –attributed to the fact that the total number of entrained aerosol particles primarily drives water vapor depletion– implies that only the dependence of $N_{ice,f}$ on the properties of a single population needs to be represented. The effect of the coexistence of multiple populations can then be emulated by this approach. This reduces the dimensionality of the parameter space and avoids the curse of dimensionality (Bellman, 1957) without loss of generality.'

At the end of Sect. 3.2:

'If a p_a - T_a pair lies to the right/below of this boundary (white region in Fig. 7b), we can be very sure that HDN does not play a significant role as the nucleation rates on the ambient aerosols are substantially higher regardless of the ambient aerosol number concentration. In the gray region of Fig. 7b, it remains uncertain whether HDN may significantly contribute to droplet formation. This depends on the aerosol number concentration and requires an explicit, time-resolved simulation of relative humidity and temperature evolution, along with the kinetics of droplet cluster growth/evaporation.'

At the end of Sect. 3.3.2:

'All together, this systematic investigation shows that the number of ice crystals is almost independent of ambient relative humidity, ambient aerosol size, and hygroscopicity for the parameter subspace defined by $T_a \leq 225$ K and 10 nm $\leq r_{d,aer} \leq 100$ nm.'

Furthermore, to emphasize the physical basis of our findings, we have added clarifying statements to the conclusions (Sect. 5), including:

'Coarse-mode particles typically do not need to be accounted for, as their much lower number concentrations compared to co-existing nucleation, Aitken, or accumulation mode particles result in a negligible depletion of the available water vapor.'

and

'The success of this weighted-mean approach is physically explained by the fact that the total number of entrained aerosol particles largely governs the competition for the available water vapor.'

Specific comments:

Line 44: although the authors have made efforts to justify this sentence by introducing several additional references in lines 32 – 38, I find that the phrasing of line 44 is still too definitive. Specifically, use of the term: “great potential”.

The wording has been revised to adopt a more cautious tone. The phrase *'has a great potential'* has been replaced with *'could play a role'* to avoid overly definitive language.

Line 59: I would suggest that the authors clarify whether they are referring to the depth/vertical extent of the contrail when using the term “vertical direction”.

We agree that the wording was ambiguous. We have replaced *'contrails grow in vertical direction'* with *'contrails increase in vertical extent'* to clarify the intended meaning.

Line 132: Note that theta (G) is used by e.g., Kärcher et al., 2015 and Ponsonby et al., 2025, to represent the temperature at which the mixing line contacts (tangentially) the saturation vapour pressure above water. Theta (RH) is then used by Kärcher et al., 2015, for the ambient temperature at which this occurs. The use of symbols in this manuscript is instead aligned with Bier et al., 2024. Although this is entirely valid, I recommend that the authors introduce a section on notation/variable use to prevent any potential confusion that may arise with this choice of naming convention.

We thank the reviewer for pointing out the potential for confusion regarding the notation. We have added a short clarification as follows:

'Note that in some previous studies, e.g., Kärcher et al. (2015) and Ponsonby et al. (2025), Θ_G denotes the threshold temperature assuming ambient air at water saturation ($RH_{\text{wat},a} = 100\%$). Here, it refers to the threshold temperature for the actual ambient relative humidity, following the naming convention of Bier et al. (2024).'

Lines 443 – 446: I appreciate that the authors have introduced this comment to clarify my previous question. However, I am still unclear why the authors choose to present these results if their earlier analysis implies that they cannot confidently rule out homogeneous droplet nucleation. I suggest that the authors consider removing these data points altogether when presenting results in Fig. 9 and in the later discussion. This change would also strengthen the narrative of the article, since the findings in Sect. 3.2 (the boundary outside which homogeneous droplet nucleation is unexpected to contribute) would then directly impact the the choice of parameter space for model initialization (Table. 2).

The aim of Sect. 3.2 is to provide an estimate of the uncertainty associated with our assumption that ambient aerosols are the sole source of ice-forming particles. Our analysis indicates that homogeneous droplet nucleation (HDN) may only become relevant at very low temperatures and can otherwise be safely neglected. As this estimate is intentionally conservative, it remains an open question whether HDN may play a role in the parameter regime where it cannot be fully ruled out. Addressing this question requires online modelling approaches with fully coupled microphysical processes; such investigations are currently being explored for fuel-cell scenarios in dedicated box-model studies and will be presented in a separate publication.

We have revisited whether removing the corresponding data points from Fig. 9 and the subsequent discussion would provide additional clarity. However, in our view, retaining all data points does not alter the interpretation or conclusions of the study and does not deteriorate the representation of contrail formation on ambient aerosol particles presented here. The assumption that only ambient aerosol particles are considered as ice-forming particles is clearly stated in the manuscript, and the results are interpreted consistently within this framework.

Lines 511 and 513: I suggest that the authors reference recent in-situ measurements performed behind a hydrogen combustor during the Blue Condor campaign and address any statements concerning “validation”.

We have added the sentences

'A step toward obtaining measurement data under hydrogen combustion conditions has been taken with the first demonstrator experiment (Blue Condor). Still, the very limited measurement database constitutes a clear validation gap. Despite this limitation, the conclusions derived in this study are grounded in physical process understanding rather than being specific to the particular model setup.'

References:

- Bier, A., Unterstrasser, S., Zink, J., Hillenbrand, D., Jurkat-Witschas, T., and Lottermoser, A.: Contrail formation on ambient aerosol particles for aircraft with hydrogen combustion: a box model trajectory study, *Atmos. Chem. Phys.*, 24, 2319–2344, <https://doi.org/10.5194/acp-24-2319-2024>, 2024.
- Kärcher, B., Burkhardt, U., Bier, A., Bock, L., and Ford, I. J.: The microphysical pathway to contrail formation, *JGR Atmospheres*, 120, 7893–7927, <https://doi.org/10.1002/2015JD023491>, 2015.
- Ponsonby, J., Teoh, R., Kärcher, B., and Stettler, M. E. J.: An updated microphysical model for particle activation in contrails: the role of volatile plume particles, *Atmos. Chem. Phys.*, 25, 18617–18637, <https://doi.org/10.5194/acp-25-18617-2025>, 2025.

Reviewer 3:

The manuscript explores simulated contrail properties relevant for a no-particle-emitting engine that emits a water vapor plume that mixes with ambient aerosol-laden air. The water vapor can then either condense heterogeneously on these ambient particles or homogeneously nucleate liquid water droplets (HDN). Both processes form liquid droplets that then freeze homogeneously to form contrail ice crystals. The simulations are interesting and well thought out. The results of the study show that ambient temperature and aerosol number concentration are the drivers of contrail ice crystal number, which is not particularly surprising. The scientific value of the paper comes from insights of how the plume maximum supersaturation might vary with ambient particle microphysical properties for the limiting case of a no-particle-emitting aircraft engine. Overall, the manuscript is well written and appropriate for ACP. My major concern relates to the assumption that aircraft with hydrogen-powered gas turbines do not emit any aerosols that contribute to contrail formation, and I'd recommend that 2 scenarios be included that account for near-field oil emissions alongside ambient aerosols as potential cloud condensation nuclei for contrail formation.

Specific comments:

1) Hydrogen-powered gas turbines require lubrication oil that has been shown to form considerable numbers of particles (US NASEM, 2025; Decker, 2024; Yu et al., 2010; Yu et al., 2012). The size of these oil particle depends on how they are emitted from the engine. For example, recent testing with low-soot CFM engines that vent oil vapor at same location as the hot combustor core flow indicate that the oil vaporizes and subsequently forms new particles of several nanometers in diameter. Other engines vent the oil vapor in the engine bypass or outside the nacelle, where there is likely to be a mixture of primary-emitted Aitken- or accumulation-size-mode droplets and semi-volatile vapor that can subsequently condense on existing particles or form new particles. While the influence of future engineering design choices makes the particle emissions characteristics of future hydrogen-burning gas turbine engine highly uncertain, it seems unlikely to me that these engines will be truly no-particle-emitting as is explored in this study.

I recommend that two additional scenarios be explored that consider how continuous co-emission of semi-volatile particles, for example:

-- Case 1: A hydrogen gas turbine engine with breather vent location at the engine tail cone (like a CFM engine), where the hot oil vapor nucleates a 10-nm-diameter size mode that mixes with the water vapor plume

-- Case 2: A hydrogen gas turbine engine with breather vent location outside the engine nacelle (like a Rolls-Royce engine), where primary emissions of 100-nm size mode oil droplets mixes with the water vapor plume

I think it will be really interesting to see if the co-emission of even modest numbers of volatile particles are enough to completely dwarf the contribution of background aerosols.

Alternatively, the title of the paper could be changed to remove references to hydrogen combustion and instead focus solely on the idealized case of a theoretical no-particle-emitting engine.

We are completely in line with the comment and cookbook of how to analyze the role of lubrication oil particles. In our previous study, we explored exactly these scenarios. See the paper by Zink, Unterstrasser & Jurkat-Witschas (2025) "On the Potential Role of Lubrication Oil Particles in Contrail Formation for Kerosene and Hydrogen Combustion" (<https://doi.org/10.1029/2025jd043487>).

This study has been cited in the introduction of the present manuscript, but apparently it was not mentioned prominently enough. Therefore, we included a new paragraph to describe in more detail our previous findings and also motivate why we felt it is too early to include lubrication oil effects in the envisaged parametrization of N_{ice} .

In Sect. 4 we added:

‘Section 3.2.2 presents a comprehensive analysis of ice crystal number assuming formation on entrained ambient aerosol particles. An additional particle source may arise from lubrication oil released from engine breather vents (Yu et al., 2010b, 2012; Decker et al., 2024), with its impact depending strongly on where the oil is injected into the exhaust plume. If venting occurs close to the hot core flow, high temperatures promote evaporation and subsequent renucleation in the cooling plume. This produces a nucleation-mode particle population (Ungeheuer et al., 2022) that typically forms well before contrail formation begins. These particles can activate into liquid droplets and subsequently freeze into ice crystals (Ponsonby et al., 2024). In contrast, if the breather flow is emitted farther from hot engine sections, the oil is expected to be entrained into the water-vapor plume only after the exhaust has substantially cooled. Under these conditions, the resulting particles are expected to remain relatively large (hundreds of nanometers) and low in number, contributing little to the total ice crystal number.

Recent work by Zink et al. (2025) demonstrates that even very small lubrication oil emissions (on the order of a few milliliters per hour or even less) can generate substantial particle numbers when evaporation and renucleation occur. In many scenarios they investigated, ice crystal formation is dominated by oil particles rather than by entrained ambient aerosol. Lubrication oil emissions therefore represent a potentially important but highly configuration-dependent particle source whose relevance for contrail formation may vary substantially across engine designs. Technical design choices may thus play a key role in determining their importance for contrail formation in hydrogen combustion cases.

In the conclusion (Sect. 5), we added:

‘The microphysical investigations presented here are complemented by the study of Zink et al. (2025), which explores the potential role of lubrication oil particles in ice crystal formation.’

2) Line 10 and Line 486: The word "solubility" is used here, but the model (and elsewhere in the manuscript) focuses on hygroscopicity.

Many thanks for this comment. We have revised the text accordingly and replaced ‘solubility’ with the appropriate terminology throughout the manuscript to ensure consistency with the hygroscopicity-based framework.

3) Line 138: It's not just that these particles are abundant at cruise altitude, but that they are co-emitted with the water vapor and mix into the early engine emissions plume where the contrail forms. We agree with the reviewer. This aspect is reflected in the study of Zink et al. (2025) and has also been made clear in the newly added text in the revised manuscript.

Lubrication Oil References:

US NASEM, 2025, <https://doi.org/10.17226/29073> (pg. 28)

Decker et al., 2024, <https://pubs.acs.org/doi/10.1021/acsestair.4c00184>

Yu et al., 2012, <https://pubs.acs.org/doi/10.1021/es301692t>

Yu et al., 2010, <https://pubs.acs.org/doi/10.1021/es102145z>