We appreciate the referee's insightful comments. Below, we present the reviewer's remarks in red italics, followed by our responses in regular text.

Reviewer 2

*The study presented in this manuscript helps to bridge a critical gap in our understanding of contrail climate effects. It is essential that we examine how contrails evolve in the presence of background cloudiness. The authors have provided a well thought-out and thorough study in this regard. However, their arguments are muddied by their presentation of their results, which is somewhat confusing to follow. As such it is unclear how their results demonstrate that "**mitigation strategies based solely on atmospheric thresholds are insufficient to reliably predict contrail evolution or their climate impact**". Additionally, there are a few cases of awkward phrasing or text that is too conversational that detracts from the point the authors are making (a few examples are highlighted below). These factors combined with the fact that the authors do not provide a discussion of their results to provide context on how they **compare to other studies**, **implications for potential contrail mitigation**, or **how their model assumptions could influence their results**, I suggest a major revision to this manuscript. Below I have listed each section with my main comments.*

We thank the reviewer for their detailed assessment and constructive suggestions. We appreciate their recognition of the manuscript's relevance and have carefully addressed all concerns raised. These revisions have substantially improved the clarity, coherence, and overall quality of the manuscript. In summary, our

simulations show that contrail growth in cloudy ISSRs depends not only on relative humidity and the Schmidt–Appleman criterion, but also on the **time-varying structure of the atmospheric environment**, particularly:

- the temporal supply of moisture,
- the vertical structure and depth of the ISS layer below flight altitude, and
- the degree of atmospheric stability.

Accordingly, we have revised our statement on mitigation strategies from: Our findings indicate that mitigation strategies based solely on atmospheric thresholds are insufficient to reliably predict contrail evolution or their climate impact.

To the more precise: Our findings indicate that relying solely on persistent-contrail threshold criteria is insufficient, as the time-varying background conditions strongly influence how far contrails grow and develop.

For clarity, we have also added explanatory text to better contextualize the analysis (lines 249-251 in the revised manuscript).

Introduction

■ *Overall this section is well written and clear. It provides a good background overview.*

■ *Line 54-55: "Merged contrails produce less total extinction compared to individual contrails due to competition for available humidity". Could the authors make this clearer that it's the competition of the ice crystals within the contrails?*

Thank you for the suggestion. We have revised the sentence to clarify that the competition occurs among the ice crystals themselves (see line 55-56 of the revised manuscript).

■ *Paragraph beginning on Line 58: I suggest the authors also cite Krämer et al. 2016 and 2020 as they provide a great overview cirrus properties that would help this section.*

Thank you for the suggestion. We have revised the relevant sentences to incorporate the recommended citations (see line 59 of the revised manuscript)

Methodology

■ *Line 141-142: I wouldn't use terms like "accurately capture" in terms of modeling. Instead simply "represent" or "simulate".*

We appreciate the reviewer's comment. The wording has been revised accordingly, and we have also incorporated related feedback on vertical resolution from Reviewer 1 (see lines 144–149 of the revised manuscript).

Can the authors explain why they did not choose to use the default cloud cover scheme and instead go for the binary (0 or 1) scheme? Are there implications on their results that they could discuss further in the manuscript?

We thank the reviewer for raising this question regarding the choice of cloud-cover scheme. In our LES with 154 m horizontal resolution, clouds are explicitly resolved, negating the need for subgrid parameterizations like the default diagnostic PDF scheme (used in coarser models for unresolved variability). The binary (0 or 1) approach is standard for LES, directly indicating resolved hydrometeor presence without extra assumptions. We have clarified this rationale in the Methods section (see lines 153-155 of the revised manuscript).

■ *Line 154: "pre-existing" ice usually refers to ice that was advected into the grid box that formed by some other process (e.g., mixed phase or ice in a convective cloud). I would cut this terminology for clarity.*

We thank the reviewer for noting the potential ambiguity of this terminology. To improve clarity, we have removed the term "pre-existing" and revised the sentence accordingly in the revised manuscript (see lines 156–159).

■ *I would* ***combine*** *Sections 2.3 and 2.4 to avoid repetition. The authors can explain their contrail formation criteria in a single section. As an example, in section 2.3* ***while describing the SAC, it is typical to include the aircraft and engine types for readers who may have more knowledge on how these may influence contrail formation thresholds****. I found the information in Section 2.4, so to reduce this repetition of information I suggest combining these sections.*

We thank the reviewer for the suggestion. Sections 2.3 and 2.4 have been merged into a single subsection titled "2.4 Contrail Formation and Initialization in ICON" (see lines 195–239 in the revised manuscript). This restructuring removes repetition and brings together the description of the Schmidt–Appleman Criterion, aircraft and engine parameters, and the initialization procedure into one coherent section. We also moved the key inputs used in the critical temperature calculation from the appendix into this subsection, as it is standard to provide these details here for readers familiar with how such parameters influence contrail formation thresholds.

■ *Line 203-204: "This selection aligns with reported values for the west coast of the United States". Can the authors provide references for this information?*

We thank the reviewer for this comment. We have clarified this point in the revised manuscript and now explicitly reference Bier and Burkhardt (2022) (see lines 220-225 in the revised manuscript).

■ *Finally, I would add a section on simulation "setup", i.e., what cases were tested and why? Instead of including a table in the Appendix, can the authors layout clearly what control cases were run (and again, why they were chosen). This will help readers immensely when interpreting the results. It is unclear from reading the results what weather conditions are present in each case.*

We thank the reviewer for this helpful suggestion. In the revised manuscript, we have added a dedicated subsection—Section 2.3: Simulation Case Selection and Background Conditions (see lines 178–194)—in which we clearly outline the motivation for selecting the eight simulation cases. This subsection now provides a quantitative characterization of each background state, including tables and relevant time series, to make the meteorological conditions and the rationale behind each case explicit. This addition clarifies the simulation setup and helps readers interpret the results more easily.

■ *Line 235: "While contrail formation does not require atmospheric saturation".* ***This statement is incorrect****. Contrail formation requires at least saturation with respect to liquid water to form cloud droplets that then freeze. Overall in Section 2.5, this appears to be missing in the discussion. For example, on Line 230 the authors state "flying aircraft within saturated regions alone does not necessarily lead to formation of contrail", which is correct if they are referring to* ***ice saturation.*** *Again, contrail formation needs at least liquid saturation.*

We thank the reviewer for pointing out this important inconsistency in our description of contrail formation. We have revised the text in Section 2.5 to correctly reflect the Schmidt–Appleman Criterion and to clearly distinguish between plume conditions and ambient atmospheric conditions (see lines 260–265 in the revised manuscript). This revision corrects the earlier misleading wording and explicitly emphasizes that contrail formation requires at least liquid-water saturation in the plume, while persistence depends on ambient ice supersaturation.

■ *Figure 3: Please label all colorbars for clarity.*

We thank the reviewer for this suggestion. The figure (Figure 4 in the revised manuscript) has been revised accordingly, and all colorbars are now clearly labelled in the updated version.

Results

■ *Line 269: Can the authors consider using a term other than "water-vapor thirsty"?*

We thank the reviewer for pointing this out. We agree that the term "water-vapor thirsty" is too informal for a scientific manuscript. In the revised version, we have replaced it with the more precise phrasing "strong moisture sinks" (line 392), which conveys the intended physical meaning while maintaining scientific rigor.

■ *Line 316: Please indicate the Ni anomaly is relative to each control case*

We thank the reviewer for noting this ambiguity. The text has been updated to explicitly state that the Ni anomaly is defined relative to each control case (see lines 326 in the revised manuscript).

■ *Line 393: Can the authors provide some quantification of how small the ice crystals remain. Another figure is not necessary here.*

We thank the reviewer for this comment. While we agree that providing quantitative ice-crystal sizes could be informative, doing so at this point in the text would raise additional questions—for example, how sizes compare across all cases, how they vary between the contrail core and its edges, or how they evolve within the fallstreak. Addressing these points would require adding substantial additional analysis that would shift focus away from the main narrative. For this reason, we opted to clarify the wording rather than introduce detailed size statistics or additional figures (see lines 381-383 in the revised manuscript).

■ *Line 427: Please revise the text in the brackets to something like, "this vertical extension is clearer in the IWC anomaly plots below".*

We thank the reviewer for the suggestion. The bracketed text has been removed in the revised manuscript, so this clarification is no longer needed.

■ *Figure 9 is unnecessary, at least in the main text as the authors only use it to illustrate that Case 4 is unstable.* ***The authors could consider simply quantifying it in absolute terms or relative (i.e., _% smaller)****. If this figure is kept, please move the legend to outside the axes.*

We agree with the reviewer. The former Figure 9 has been removed from the main text, and the relevant absolute values are now reported in Table 1.

■ *Figure 5: please include "****Case X****" as titles at the top of each column, also please make the axis labels larger for legibility and use the same format for longitude and latitude that was used in Figure 1.*

We thank the reviewer for these suggestions. The requested changes have been implemented in the revised version (Figure 6 in the revised manuscript).

■ *Figure 6:* ***It would be useful to include the horizontal coordinate on the x-axis, in I assume meters***

We thank the reviewer for this suggestion. We originally chose not to include the horizontal coordinate because quantifying contrail width is not the focus of this analysis. In our non-idealized setup, the contrail width varies both across cases and throughout each individual simulation, making a single horizontal scale potentially misleading. For this reason, we avoided entering into width diagnostics (this has been clarify in the revised manuscript line 296-299) and instead focused on the consistent microphysical contrast observed across all cases: the contrail core consistently maintains much smaller ice crystals than the surrounding natural cloud, even after 90 minutes of evolution (See Figure 7 in the revised manuscript).

■ *Figures 7, 10, and 11: Please include the y-axis label on each row and the units. Similarly for the x-axis label*

We thank the reviewer for the suggestion. The suggestions are applied accordingly in the revised manuscript (Now Figures 8, 10, 11 in the revised manuscript)

■ *Figure 8: by using the same latitude and longitude formatting as in Figure 1, the authors wouldn't need to include the labels on the axes here. Additionally, a more colorblind friendly colormap is preferred.*

We thank the reviewer for the helpful suggestions. The change to the axis formatting consistent with Figure 1 has been applied. Regarding the colormap, we agree that colorblind-friendly palettes are generally preferable. However, for this particular figure, the selected colormap provides the necessary contrast to clearly distinguish subtle variations in ice-crystal number concentration across both the contrail and the surrounding cloudy background. For this reason, we have retained the existing colormap while ensuring that it remains interpretable (Figure 9 in the revised manuscript).

■ ***Figures 13 and 14:*** *a single legend would make this figure easier to understand, with subplot titles indicating each case. Similarly,* ***I don't recall whether the numbers above the black lines are ever explained.***

We thank the reviewer for these helpful suggestions. These have been applied in the revised manuscript (Figures 12 and 13 in the revised manuscript).

References

Bier, A. and Burkhardt, U.: Impact of parametrizing microphysical processes in the jet and vortex phase on contrail cirrus properties and radiative forcing, Journal of Geophysical Research: Atmospheres, 127, e2022JD036 677, https://doi.org/10.1029/2022JD036677, 2022.