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

Reviewer 1

Review of the revised version of
Investigating the Development of Perisitent Contrails in Ice Supersaturated Regions with Cloudy Background Using ICON-LEM
by S. Marjani et al.

As this is already the revised version, my review will just mention some points for improvements.

We thank the reviewer for their evaluation of the manuscript and for highlighting several important issues. We have addressed all reviewer comments in detail, and we believe the corresponding revisions have substantially improved the overall quality of the manuscript.

There is one major issue and several minor ones.

Major issue:

*It is not clear to me what exactly is meant with the statement (page 11, line 231) "we approximate that each cloudy grid cell is predominantly occupied by either natural cloud ice or contrail ice". What does "**predominantly**" mean here? It seems to indicate that a grid box can only have either cloud ice or contrail ice, but not both in a mixture. If this is the case, then this should be stated (in this case "predominantly" would be wrong). In any case, it is necessary that **the consequences of this choice are discussed**. Unfortunately, this issue does only appear again in the conclusion section after line 566 (that is, there is no discussion of the consequences) and I find this paragraph not convincing. Instead of playing with the resolution to assess the effects of this choice, wouldn't it be straightforward to just **let both ice categories mix in the grid, of course with keeping track of their sources**.*

Thank you for raising this important point. In the present high-resolution LES configuration of ICON-LEM, the two-moment bulk microphysics scheme represents six hydrometeor categories (cloud droplets, rain, ice, hail, snow, and graupel), but only a single prognostic category is used for ice particles. As a result, contrail ice and natural cloud ice are not tracked as separate source-tagged classes. Instead, the model predicts the evolution of the total bulk ice population within each grid cell and its interaction with neighbouring grid boxes.

Therefore, we do not assume that a grid cell contains exclusively contrail or natural cloud ice. Rather, once both are present within the same grid cell, their individual source contributions cannot be distinguished, and only their combined bulk evolution is represented.

Our study focuses on the post-vortex dispersion phase of contrail evolution, after the jet and vortex dynamics have ceased. In this stage, contrails form a narrow plume characterized by very high ice crystal number concentrations, while their subsequent evolution is mainly controlled by ambient thermodynamic conditions through depositional growth, sublimation, and mixing. Since the nucleation and early jet phases are not considered here, initializing the contrail as a narrow line of high ice crystal number concentration is sufficient for addressing the objectives of this study.

During the analysis, cloudy grid cells that exhibit a clear response to the perturbation relative to the control simulation are labelled as contrail grid cells, while unaffected cloudy grid cells are considered natural cloud. This classification is diagnostic and should not be interpreted as a separate representation of ice categories within the microphysics scheme. These have been clarified in the revised manuscript in section 2.4 (lines 230-240) and conclusion (lines 578-586).

Minor issues:

1) *Table 1 should also list the wind shear at the position of the contrail, ideally with the angle between contrail direction and shear vector. The text mentions the widening of the contrail sometimes, and wind shear is important for that.*

Thank you for this suggestion. We have added to Table 1 (of the revised manuscript) the perpendicular component of the local wind shear relative to the initial contrail axis, $S_{\perp}$, evaluated at flight altitude at the contrail initialization location. This quantity is included as a simple indicator of the initial dynamical tendency for lateral contrail spreading.

2) *Page 13, Section 3.1, Figure 5: It seems that the contrails remain quite narrow, which is surprising. Please give the reader a hint of the lateral dimensions of the contrails by* ***including scales*** *in Figure 5. What are the spreading rates? Are those smaller than typical values for contrails in clear air?*

Thank you for this helpful suggestion. First, we clarify that Figure 5 shows the horizontal extent of the contrail at flight altitude only (see Figure 5 caption at the revised manuscript). It therefore does not include the additional horizontal spreading associated with the vertical development of the contrail across multiple levels, which can lead to a larger overall extent (see also lines 273-275 of the revised manuscript),.

To provide a clearer visual reference, we have added scale bars to Figure 5 in the revised manuscript.

To complement this, we have included Table A1 in the Appendix, which provides quantitative estimates of contrail width at 30 and 60 minutes after initialization, both at flight altitude and across all vertical levels. These values can be interpreted as a proxy for the contrail spreading rate.

The lateral spreading of contrails is known to exhibit substantial variability, and reported widths depend strongly on the definition of contrail boundaries, particularly the threshold used to include optically thin regions. Observational and modelling studies therefore show a wide range of values. For instance, Kärcher et al. (2009) report contrail widths up to ~18–19 km when optically thin edges are included, whereas satellite-based analyses by Iwabuchi et al. (2012) yield smaller mean values (≈5.1 ± 1.6 km for mature contrails and ≈12.3 ± 5.2 km for older contrails), highlighting the sensitivity to detection criteria. Similarly, earlier observational and modelling work suggests that contrail widths after about one hour typically range between 1 and 10 km, depending on environmental conditions such as wind shear and the depth of the ice-supersaturated layer (e.g., Jensen et al., 1998; Unterstrasser and Gierens, 2010).

In the present simulations, contrail widths at flight altitude range from 2.9–6.1 km at 30 min and 4.7–8.2 km at 60 min, while the overall horizontal extent reaches 5.2–10.4 km at 30 min and 8.4–21.6 km at 60 min. These values fall within the range reported in the literature and are therefore not smaller than typical clear-air contrail widths.

It is also important to note that the width definition in this study is based on differences between perturbation and control simulations, which allows detection of even weak contrail-induced signals. This approach is inherently more sensitive than observational methods, where width estimates depend on retrieval thresholds and instrument sensitivity. As a result, some differences between model-derived and observation-based widths are expected.

3) *Introduction: Recent (2025) Nature papers be Petzold et al. and Seelig et al. should be mentioned. They investigated contrails within cirrus clouds as well.*

Thank you for the useful suggestion, the two papers were included accordingly (lines 431-433, 533-534, and 577 of the revised manuscript).

4) *Minor but important: It is not always clear in the results section whether you mean N/IWC or the anomalies. Please check your text again. I suggest to use Delta signs when you speak about anomalies.*

Thank you for this important remark. We agree that in parts of the results section, especially Section 3.3, the wording could be clearer in distinguishing absolute quantities from perturbation–control differences. We have therefore revised the text to use explicit delta notation (ΔN_i, ΔIWC) wherever anomalies are discussed, while retaining N_i and IWC without delta in sections and figures showing absolute fields or separate perturbation/control values. We also added a clarifying sentence at the beginning of Section 3.3 (lines 325-328 of the revised manuscript) to define this notation consistently.

5) *Abstract, line 13, and conclusion, line 564: What dou you mean here? "relying solely... is insufficient", insufficient for what? And "simplistic ... strategies... may be insufficient...". I don't know what you mean. Please give concrete examples with references of the insufficient methods that you have in mind.*

Thank you for this helpful comment. We agree that the wording in both the abstract and conclusion was too vague. In the revised manuscript, we now state explicitly that the limitation concerns the use of thermodynamic threshold criteria alone to predict subsequent contrail growth, radiative impact, and therefore the effectiveness of contrail-avoidance strategies. In the conclusion, we further clarify the type of methods we had in mind, namely operational mitigation concepts based on simple altitude adjustments or rerouting guided primarily by Schmidt–Appleman and RH_{ice} / ISSR thresholds, and we added relevant references (see lines 573-577). The abstract has also been revised to make the intended meaning explicit without introducing references there (lines 13-14 of the revised manuscript).

6) Line 558: Please note that air has no water holding capacity. Although this opinion is quite popular, unfortunately even under atmospheric scientists and meteorologists, it is complete nonsense.

Thank you for this correction. We agree that the phrase "water holding capacity" is physically imprecise. Our intention was to refer to the fact that at low temperatures the saturation vapor pressure over ice is small, so the absolute amount of excess water vapor available for depositional growth is limited even under high relative humidity. We have revised the wording accordingly (lines 567-568 of the revised manuscript).

7) I suggest to have a look on Holger Vömel's collection of vapour pressure formulations, https://cires1.colorado.edu/~voemel/vp.html. The Magnus-Tetens formula is way off other formulations.

Thank you for this helpful suggestion. We agree that the Magnus–Tetens formulation is not the most accurate choice at very low temperatures. In this study, however, the excess-water-vapor diagnostic was computed consistently with the saturation-vapor-pressure formulation used by the ICON version employed for the simulations (ICON-2.6.6), rather than being selected as a preferred standalone formulation. Our intention was to maintain internal consistency between the post-processed diagnostic and the model thermodynamic fields. We have revised the manuscript to make this explicit (lines 456-458 of the revised manuscript).

Other issues:

1) Section 1 has no title

Thanks for pointing it out, the issue is resolved

2) Line 27: the mentioning of "jet phase" is correct, but a reader without the necessary background may not understand it here.

Thank you for this remark. We agree that the first mention of "jet phase" appeared before the term was defined. We therefore revised the sentence (lines 27-28 of the revised manuscript) in the introduction to clarify that the jet phase refers to the first few seconds after emission, while retaining the fuller description of the three contrail life-cycle phases in the following paragraph.

2) Line 35: "Fuel emissions"?

Thank you. We agree that "fuel emissions" was imprecise in this context and have replaced it with "engine emissions." (line 35 of the revised manuscript)

3) Line 47: "Supersaturation thickness" ?

Thank you. We agree that "supersaturation thickness" was unclear and have replaced it with "the thickness of the supersaturated layer." (line 47 of the revised manuscript)

5) Line 104: insert "is" between "condition" and "often"

Thanks, we have revised the sentence (line 104 of the revised manuscript),

6) Line 143: "turbulent statistics" ?

Thank you. We agree that "turbulent statistics" was not the clearest wording in this context and have replaced it with "turbulent-related quantities." (line 143 of the revised manuscript).

7) Line 149: I suggest upper-TROPOspheric

Thank you. We agree and have replaced "upper-atmospheric" with "upper-tropospheric." (line 149 of the revised manuscript)

8) Line 167: Check whether ECMWF is meant or rather ICON-global

Thank you for this remark. In our setup, the outer ICON-NWP simulation was initialized and forced directly with ECMWF IFS operational analysis data, not with ICON-global output.

9) Section 3 should be ResultS and discussion, if there is more than one result.

Thanks, we revised the section title.

10) Line 393: please add "(1,2,6)" after "these three cases".

Thank, the manuscript is revised accordingly (line 345 of the revised manuscript)

11) Line 438: Delete "are".

Thank, the manuscript is revised accordingly (line 445 of the revised manuscript)

Reviewer 2

Review of the revised version of Investigating the Development of Persistent Contrails in Ice Supersaturated Regions with Cloudy Backgrounds Using ICON-LEM

by Sajedeh Marjani et al.

General comment:
The authors responded to my major issues and added some text to the manuscript. However, the manuscript only slightly improved. Actually, the main issues, ***as the inadequate microphysics model (classes and representation of ice cloud processes****) and the* ***lack of a quantitative evaluation*** *remain. The explanations are still not satisfying, and to my opinion, the main issues* ***cannot be resolved*** *by just providing some more explanations.*
I still think that the manuscript should not be accepted, as long as the ***methods*** *and the evaluation are not improved. However, it is certainly the decision of the editor, how to proceed.*

We thank the reviewer for the continued assessment of the manuscript and for again emphasizing the importance of microphysical representation and quantitative interpretation. We agree that source-tagged or multi-class ice schemes, together with dedicated sensitivity and evaluation studies, would be valuable. However, we respectfully note that such developments are outside the scope of the present manuscript.

The present study is intentionally designed to investigate the post-vortex evolution of an identical contrail perturbation embedded in cloudy, ice-supersaturated environments, and to quantify how differences in the surrounding atmospheric background modulate its subsequent development. The manuscript therefore does not claim to introduce, validate, or replace a contrail-specific microphysical parameterization.

The concerns raised here largely repeat the two main points addressed in our response to the first review round. In response to the first-round comments, we revised the manuscript to clarify both points. In particular, we stated explicitly that contrail ice and natural cloud ice are not treated as separate source-tagged populations, and that once both occupy the same grid cell the model represents their combined bulk ice evolution. We also clarified that the paired perturbation–control simulation design is used to diagnose contrail-induced anomalies.

This modelling perspective is consistent with recent closely related work by Verma and Burkhardt (2026), who argue that, although previous modelling studies often kept contrail cirrus and naturally formed cirrus in separate cloud classes, such separation is not necessarily the most appropriate representation when studying contrail formation within cirrus, because the two ice populations are mixed within the same air volume and interact. In their approach, the jet-phase contrail ice nucleation and vortex-phase ice crystal loss are treated with dedicated parameterizations adapted for cloudy conditions; after these early stages, the contrail evolves during the dissipation and diffusion phases through the model cloud scheme. This distinction is directly relevant to our study, which starts from an initialized post-vortex contrail perturbation and focuses on its subsequent evolution within cloudy, ice-supersaturated environments.

We also respectfully disagree that the manuscript lacks quantitative evaluation, or that the last revision only added explanatory text. The manuscript already contained a quantitative paired perturbation–control analysis, including comparisons of ice crystal number concentration, ice water content, humidity changes, excess water vapour availability, and radiative responses across the investigated cases. In the last revision, we further strengthened the quantitative basis of the study by adding a clearer characterization of the background weather conditions for all eight cases, including time series of excess water vapour, ISS depth, RH_{ice} and vertical velocity, as well as additional case-summary information. In the current revised manuscript, we have further added quantitative diagnostics requested during review, including the local perpendicular wind-shear component at the contrail initialization location and contrail-width estimates at 30 and 60 minutes. These additions support the interpretation of the simulations within the stated scope of the study.

We have therefore kept the interpretation of the results within the limits of the model configuration and explicitly acknowledged these limitations in the revised manuscript. In our view, the reviewer's remaining concern reflects a disagreement with the chosen scope of the study rather than an unresolved methodological inconsistency. We therefore do not make further manuscript changes in response to this general comment but refer the editor and reviewer to the clarifications already added in the revised manuscript and to our detailed response to the corresponding first-round comments.

Major issues:

The manuscript explicitly addresses contrails INSIDE cirrus clouds, implying that the contrail can be placed into a pre-existing cloud, and of course accepting the fact that ice crystals from cirrus clouds above can sediment into the contrail. Without a conceptual separation of the two "clouds" the investigation must remain incomplete. The argument about the high vertical/horizontal resolution does not help, since even in a super high resolution one CAN place a contrail into an ice cloud; without a separation of the microphysical properties (i.e. different mean masses) the resulting cloud is neither contrail nor cirrus (for instance, the sedimentation velocity is changed completely, if ice crystals from a cirrus cloud above fall into the contrail – no ice can fall out to lower layers).

We agree that, for a study whose objective is to distinguish contrail ice and natural cirrus ice at all times, or to diagnose class-specific sedimentation, growth, sublimation, and source-resolved microphysical tendencies, a multi-class or source-tagged ice framework would be required.

However, this is not the objective of the present manuscript. The study does not aim to identify the separate microphysical histories of "contrail ice" and "natural cirrus ice" once they coexist in the same model grid cell. Instead, it is designed to quantify the net atmospheric perturbation caused by introducing an identical post-vortex contrail into different cloudy, ice-supersaturated environments. This is why the analysis is based on paired perturbation and control simulations under otherwise identical meteorological conditions.

We acknowledge the reviewer's point that, in a single-category bulk ice scheme, the combined ice population in a mixed grid cell is not purely "contrail" or purely "cirrus" in a source-resolved sense. This limitation is explicitly stated in the manuscript. In particular, we clarified that contrail ice and natural cirrus ice were not tracked as separate source-tagged populations; once they occupy the same resolved grid cell, only the combined bulk ice evolution is represented. We also clarified that the paired perturbation–control framework diagnoses contrail-induced bulk anomalies, not source-resolved microphysical budgets.

We therefore respectfully disagree that this limitation makes the investigation invalid. It means that the results must be interpreted as bulk atmospheric responses to an imposed post-vortex contrail perturbation, rather than as a decomposition into contrail-origin and natural-origin ice fractions. This interpretation is stated explicitly in the revised manuscript, and the conclusions have been framed accordingly.

This point was already addressed in our response to the first review round, where we explained that multi-class schemes are valuable for source-resolved diagnostics but go beyond the scope of this study. In response to that comment, we revised the manuscript to make the limitation explicit and to avoid implying that the model provides a source-separated representation of contrail and cirrus microphysics.

> *It is quite clear that most of the ice microphysics scheme can be used for both, the contrail and the natural ice cloud (e.g. growth equation, formulation of the terminal velocity).* ***However, the formation of ice is certainly different****. For the contrail, this is just an instantaneous placement of the ice crystals, which is fine with me. The "natural" ice formation pathways as homogeneous freezing of solution droplets and heterogeneous ice nucleation is a different issue.* ***To my opinion, the Kärcher et al. (2006) scheme must be adapted for high time steps (as e.g. carried out in Köhler and Seifert, 2015);*** *probably the nucleation events can be resolved directly. The authors state that the scheme was already used in many studies before. This argument is not convincing, since all studies cited in the response are investigations of warm or mixed-phase clouds (except of the study by Verma and Burkhardt, 2022, which uses a coarser resolution and an adapted cirrus-contrail scheme). The authors have to give arguments according to the physics of the ice clouds, that this scheme can be used.*

We thank the reviewer for clarifying this concern. In the present study, the formation of contrail ice during the jet and vortex phases is not simulated by the natural cirrus nucleation scheme. The contrail is initialized directly as a post-vortex ice population, and the study focuses on its subsequent dispersion-phase evolution in different cloudy, ice-supersaturated environments.

The reviewer's comment therefore concerns the suitability of the default ICON-LEM ice nucleation treatment for natural cirrus formation. This is an important model-development question, but it is not the objective of the present manuscript. We do not introduce, modify, or validate a new ice nucleation parameterization. Rather, we use the standard ICON-LEM configuration, in which homogeneous and heterogeneous ice nucleation are treated following Kärcher et al. (2006) and Phillips et al. (2008), respectively. This configuration is part of the established ICON-LEM setup used in high-resolution LES applications.

We note that this is also consistent with the treatment of later contrail evolution in closely related ICON-LEM work. Verma and Burkhardt (2022) modified the parameterizations for contrail ice nucleation and vortex-phase ice crystal loss to account for contrail formation within cirrus. However, the subsequent dissipation and diffusion phases were handled by the model cloud scheme, as also stated explicitly in their recent follow-up study (Verma and Burkhardt, 2026). Our study starts after the vortex phase and therefore concerns this later evolution stage, for which using the ICON-LEM cloud microphysics is consistent with that modelling approach.

In addition, the natural ice nucleation treatment is applied identically in each control simulation and its corresponding perturbation simulation. Thus, the paired perturbation–control analysis is not used to validate the absolute natural cirrus nucleation pathway, but to diagnose the additional bulk atmospheric response caused by introducing the initialized post-vortex contrail under otherwise identical meteorological and microphysical conditions.

We therefore respectfully consider the request to adapt or replace the Kärcher et al. (2006) treatment to be beyond the scope of the present study. Such an implementation would constitute a separate model-development and microphysics-evaluation study. In response to the first review round, we revised the manuscript to describe the ICON-LEM microphysics configuration more clearly and to state explicitly that we use the standard model setup.

> *My concern about the resolution was mostly driven by the anisotropy of the atmosphere (vertical vs. horizontal scales). The authors have clarified the used configuration (resolution) of the model. However, the explanation is lacking arguments about the different horizontal and vertical scales. If they use a horizontal resolution in order of 150m, then the vertical resolution should be much smaller (in order of few 10m), if we think about the thin layers of the atmosphere. The authors should again argue with physics; again, referring to former studies is not enough if there is no convincing physical argument.*

We thank the reviewer for reiterating the importance of atmospheric anisotropy and the different characteristic horizontal and vertical scales in the upper troposphere. We agree that finer vertical spacing would allow sharper vertical gradients and thinner atmospheric layers to be resolved more explicitly.

This point was already addressed in detail in our response to the first review round. In particular, we clarified the vertical grid structure and included a figure showing that the ICON vertical grid is stretched: the model uses 160 vertical levels, with spacing starting from approximately 20 m near the surface, gradually increasing with altitude, and reaching approximately 150 m in the 8–12 km cruise-altitude region relevant to this study. Thus, the model does not use a uniform 150 m vertical grid throughout the atmosphere.

The chosen configuration therefore reflects a deliberate compromise between resolving upper-tropospheric contrail evolution, maintaining numerical stability, and keeping the full set of paired control and perturbation LES simulations computationally feasible. A vertical resolution of a few tens of metres throughout the upper troposphere would substantially increase the computational cost and further restrict the stable model time step, making the complete set of realistic nested simulations impractical.

In response to the first review round, we revised Section 2.1 to state the horizontal and vertical resolution more explicitly, including the stretched height-based coordinate, the 160 vertical levels, the approximately 137 m average spacing over the full column, and the approximately 150 m spacing in the 8–12 km cruise-altitude region. We therefore do not make further manuscript changes in response to this repeated point.

> *The investigations lack a clear quantitative evaluation. It would be interesting how the contrails modify the environment and thus the relevant cloud processes in order to produce a different time evolution and other physical states as compared to the control run.* ***An analysis of time scales would be very appropriate and can be done offline*** *(i.e. after performing the simulations). I am completely sure that such investigations would result into a clear understanding how the contrail inside the cirrus cloud influences the further evolution. The issue of quantitative evaluation is strongly related to the choice of the scenarios.*

We agree that an additional offline time-scale analysis could provide further process interpretation, and it is indeed a very important question on how the contrails interact with the meteorological regime. We are already in the process of performing this kind of analysis but since it is a rather large endeavour and requires in-depth analysis and description, we will do this in a separate paper.

> *The author state that the scenarios are chosen "to represent a range of realistic, cloudy, and ice-supersaturated environments". How do they know that these scenarios are really representative? They are just case studies from a former investigation, and at least in this study there is no statistics or whatever evaluation that these scenarios are "representative". If they can show with a concise evaluation based on physics (e.g. time scale analysis) that the outcome of the time evolution with contrails is based on the same general effects on the natural cloud processes, then this investigation would lead to a better understanding. At the moment it is just a collection of cases, which were randomly chosen and evaluated. However, I appreciate the attempt to collect the main features as found in the evaluation thematically.*

We appreciate this point and agree that "representative" could be interpreted as implying statistical representativeness. This was not intended. The eight cases are real-world case studies selected from Marjani et al. (2022) to cover a physically diverse range of cloudy, ice-supersaturated environments.

To avoid ambiguity, we have replaced "representative" in the Introduction (see line 130 of the revised manuscript) and Section 2.3 (see line 184 of the revised manuscript) with "selected real-world atmospheric conditions" and "a physically diverse set of cloudy, ice-supersaturated background conditions."

An additional time-scale analysis could provide further process insight, but it would be an extension beyond the scope of the present non-idealized case-study investigation.

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