Thank you for your feedback. It seems that some points from our previous response require clarification to clear up a few misunderstandings. In the attached document, you will find our answers to your latest comments and requests.

First, we would like to stress that the primary objective of this paper is to demonstrate the significant influence of near-field aerodynamics on contrail development in the far field—that is, a few minutes after the initial formation of ice crystals. Our results clearly show that horizontal tailplane (HTP) vortices play a key role in shaping the microphysical properties of contrails. We believe this is an important finding, as much of the existing literature tends to idealize the aerodynamic wake and neglect HTP vortices when initializing contrail far-field simulations. We acknowledge, however, that it remains an open question whether the influence of near-field aerodynamics persists throughout the diffusion regime and whether it significantly affects the radiative forcing of contrails.

The modifications implemented in the new version of the manuscript are in color **blue**.

The style of this document is as follows: In red, you can see the answers to your comments. The text in color black contains the reviewer first comment, the authors first answer and the reviewer second comment.

GENERAL COMMENTS:

Reviewer comment round 1: The analysis of the extinction and optical depth of young contrail may be misleading and misinterpreted by readers. The contribution of the first few minutes of the full contrail lifecycle to the time-integrated radiative forcing (or extinction) is usually not substantial. A relatively larger extinction in the beginning does not imply a larger radiative impact at later times and in total.

Authors answer round 1: Thank you for this valuable comment. We agree that the early contrail radiative forcing is not relevant for the climatic impact of a contrail. And that an initial larger extinction does not imply a larger radiative impact several hours later. In order to avoid misinterpretation, the following warning has been added line 487: Indeed, the differences in extinction observed for the first few minutes may potentially decrease, or even vanish, over longer timescales owing to the effect of atmospheric turbulence and wind shear on the ice crystals spatial distribution. A larger extinction in the beginning does not necessarily imply a larger radiative impact at later times and over the full lifetime of the contrail.

Reviewer comment round 2*: Thanks for adding these sentences. Nevertheless, I do not think that adding only these few lines is sufficient as you still show all the plots with quantities (for which you write they may not matter).*

Previous studies (Unterstrasser & Gierens, 2010b; Lewellen, 2014) clearly demonstrated that the number of early ice crystals has a significant long-lasting impact. Early differences in mass (and as a consequence, also changes of the total extinction that are due to ice mass changes) are not really relevant. Your selection of plots does not reflect this at all. This is why GCM models with a contrail parametrization aim at a refined initialization with advanced estimates of the initial ice crystal number (Bier & Burkhardt, 2019, 2022). Moreover, measurement campaigns aim at deriving apparent ice emission indices to evaluate contrail ice number formation (Märkl et al, 2024; Bräuer et al, 2021).

References doi: 10.1029/2018JD029155, 10.1029/2022JD036677, 10.5194/acp-24-3813-2024, 10.1029/2020GL092166

Authors answer round 2: The plots of the optical depth have been removed from the paper. We agree with you concerning the very strong influence of ice crystal number on contrails total extinction. As will be discussed below, we have added in our paper the evolution of total ice crystal number per meter of flightpath. However, we would like to add that for some initial ice crystal number values ice mass is a relevant parameter too. For example, in Unterstrasser and Gierens (https://acp.copernicus.org/articles/10/2037/2010/acp-10-2037-2010.pdf), looking at Fig. 1 for total extinction shows that increasing total ice mass by a factor 2 leads to a 14% increase in total extinction at t=20000 s (compare green solid line with green dot line). On the other hand, increasing total ice mass by a factor 10 leads this time to a 6% decrease in total extinction (compare brown solid line with brown dot line).

Reviewer comment round 1: Simulations in Unterstrasser & Gierens (2010b) and Lewellen (2014) show that the total number of ice crystals is the most crucial quantity of young contrails that determines the further fate. The early ice crystal mass (and also optical depth and integrated extinction) does not significantly affect the long-term behaviour of the contrail-cirrus transition. Hence, an evaluation of total ice crystal number would be more insightful.

Authors answer round 1: We give the evolution of averaged ice crystal number (Np) in the contrail as a function of time (Fig.7, Fig. 15, Fig.20 and Fig.24). We believe this gives an insight on the number of ice crystals in the domain.

***Reviewer comment round 2**: The ice crystal number have not been put into context. As mentioned, this quantity is crucial and should compare the survival fraction with previous studies. There are many contrail formation and contrail vortex phase studies to compare with. This would also help to motivate the added value of your approach compared to existing ones.*

Authors answer round 2: As mentioned in our previous response, we have now highlighted the importance of the number of early ice crystals for contrail evolution relatively to other parameters (see line 522). Moreover, a direct comparison has been made with Lottermoser and Unterstrasser (https://doi.org/10.5194/acp-25-7903-2025, 2025) where we computed z_Δ parameter and compared our values with the ones obtained in Lottermoser and Unterstrasser work (see Fig.16). Comparisons with other works from the literature have been made through the paper concerning the values we get for total ice crystal number.

Reviewer comment round 1: Moreover, I strongly suggest to not use the term "radiative forcing of young contrails". First of all, radiative forcing is defined as a radiative imbalance typically evaluated at the top of the atmosphere and this is not what is evaluated in your study. You should make clearer, how to interpret the extinction quantity that you analysed.

Authors answer round 1: "Young contrail" has been replaced by "Recently formed contrail" and "Radiative forcing" (RF) by "extinction" except for the reference to Ferreira et al. work where an RF parametrization has been used to estimate RF for a recently formed contrail. However, it is true that we cannot extrapolate the results of our simulations to estimate RF a few hours after the end of the dissipation phase, that is in the diffusion phase.

Reviewer comment round 2: *Indeed, Ferreira et al computed the RF of a 10s-old contrails. But this RF estimate is irrelevant for several reasons and I recommend to not cite it in the way you do it:*

• The RF parametrization was never meant to be used for a 10s old contrail as it was done by the cited study.

• The contribution of 10s old contrails to the lifetime-integrated radiative effect is negligible.

• Moreover, changes in the RF at t=10s gives no indication about RF changes at later times nor about the lifetime-integrated radiative effect.

Authors answer round 2: Ferreira citation has been removed from the paper.

Reviewer comment round 1: How robust are the evaluations of Δz and Δy? In Eulerian models, contrails typically do not feature very strong gradients at the boundaries and fade out. Hence, the values you determined may depend on thresholds with which you define a contrail. I believe it would help readers to also show vertical and transvers profiles of ice crystal number and mass. This would allow for a more quantitative comparison compared to Figs. 7, 8 and 12 and also makes clearer how robust the evaluations of Δz and Δy are.

Authors answer round 1: Yes, it depends on the treshhold but the goal here was to compare the different initialization strategies results using the same treshhold. Spatially averaged ice crystal numbers field 2D contours have been added to the paper (Fig.12, Fig.13, Fig.21, Fig.23) to have a better understanding of contrail size

Reviewer comment round 2*: I understand your goal, but your findings may depend on which threshold value you choose. You write in your reply that the quantities of interest depend on the chosen threshold values. Hence, it is important to demonstrate the robustness of your definitions. You should convince the reader that your conclusions do not depend on the choice of threshold. Many thanks for adding the additional figures. That helps a lot. However, the figures contain a lot of white areas and you should zoom into the relevant areas. Moreover, clarity could be enhanced by adjusting the colorbar to span from 1e6 to 1e9 and using nicer values on the tick labels! Using one colorbar for all subpanels would be sufficient. Moreover, the figures should be combined into a single one that can then be referenced throughout the text. The identical plot (with caption '2LO initialization' for Nb = 0.012 s-1) appears three times in the manuscript. It is very unusual to show the same plot multiple times in the manuscript.*

- There has been a misunderstanding from us concerning your comment, misunderstanding which impacted our subsequent answer. By "treshhold", we understood the treshhold concerning the definition of the contrail length and width, not the definition of the contrail itself, that is the cells in the domain where rp>rs.

  We strongly believe that defining the contrail by rp>rs using our specific Eulerian microphysics model is a robust definition of the contrail in the context of our Eulerian microphysics model. We agree that this is a point that needs clarification. The mathematical definition of rp is:

$$r_p^3 = \frac{3}{4\pi}\frac{\rho Y_{\mathrm{H_2O}_s}}{\rho_{\mathrm{sol}} N_s} + r_s^3$$

$\rho, N_s, \rho_{sol}, Y_{H_2O_s}, r_s$ are respectively fluid density, soot density number, ice density, ice mass fraction and dry soot particle radius. When rp>rs in a mesh cell, it necessarily means that $Y_{H_2O_s} > 0$ and that the cell contains ice crystals. Thus, that cell is part of the contrail. When rp=rs, $Y_{H_2O_s} = 0$ and the particles contained in the cell are all dry soot particles.

- We have zoomed on the mentioned figures to have less white. Colorbar on the figures has been fixed to span fro 1e6 to 1e9 with nicer values. Only one colorbar is used on each panel now. The titles of the plots/figures have been modified to differentiate them in the paper.

Reviewer comment round 1: How is the boundary of the contrail defined? Why do you choose to apply a weighting by number? Why at all and why not by ice mass e.g.? No spatial distributions of ice crystal number concentration are displayed. Nor the time evolution of total ice crystal number is shown. Hence, it is not transparent what effects the weighting in the averaging procedure does introduce.

Authors answer round 1: The contrail is defined by the mesh cells where the ice crystal radius rp is strictly greater than the radius of a dry soot particle rs (27 nm). This is now stated in the paper. Thus, only particles with an ice cap are considered. Those particles are by definition ice crystals. We applied a weighting by number of ice crystals because it adequately represents the influence of each ice crystals on the contrail mean quantities. For X a microphysical quantity, each value X contributes to the average proportionally to how many particles have that value. This is exactly the same as weighting by the number of ice crystals. Such weighting is commonly used in statistical physics. If we weighted by ice mass, we could have situations where a cell have a high ice mass but not that many ice crystals, which would bias the computed mean.

Reviewer comment round 2*: I understand how the weighting is done, however it is still not reasonable for me. You state that the weighting is applied for all quantities depicted in Fig. 7. For me such a weighting does only make sense for quantities that describe the properties of a single particle! Hence, only the activated surface fraction and the mean crystal radius are reasonably defined (panel c-d). Why should you apply a weighting by number when you want to obtain the mean number concentration (panel a). The same is true for the IWC (panel b). It is not a property of a single particle. IWC is defined as a mass per volume. Hence, only a weighting with the volume of your grid cells makes sense. What is a number-weighted relative humidity (panel e)? What's the physical interpretation of weighting RH with the ice crystal number?*

Authors answer round 2: We agree with your comment. IWC, RH and soot number density are now weighted by volume instead of ice crystal number. Corrections have been implemented in Fig.7 for the near-field RANS simulation. Concerning the temporal LES simulation of the far-field, we replaced the averaged quantities with total quantities as will be discussed below.

Reviewer standalone comment round 2: *A more general comment: Your goal is to analyse the radiative effect of a contrail. However, in most figures, you show mean quantities, which are intensive quantities. If you are interested in the total effect, then total quantities should be analysed (i.e. extensive quantities), and not total quantities divided by the contrail volume. Moreover, mean quantities are hard to interpret as they show the combined effect of a change in the total quantity and in contrail dimensions. And to make matters worse, the latter depends on the choice of a threshold. As a side comment: For contrails, however, it makes no sense to integrate over all three space dimensions. Clearly, the total quantities scales with the length of the contrail in flight direction. Hence, intensive quantities are typically integrated over the contrail cross-sectional area. In the RANS domain, you divide the contrail into thin slices of width dx, sum up the quantity of interest in each slice, and then divide by dx. This yields the integrated quantity as a function of downstream distance x with units of m^-1. In the temporal LES, you can integrate over all three dimensions and divide by the length of your domain/contrail.*

Authors answer round 2: We agree with your comment. Average quantities have been replaced by total quantities. For the RANS domain, we plot total ice mass and total ice crystal number per meter of flightpath (See Fig.9). For the temporal LES, we followed your recommendations and we now plot total ice mass per flightpath m_i (kg/m), total ice crystal number per flightpath N_i (1/m) and the mean particle radius defined by: r_p,mean = $\left(\frac{3\, m_i}{4\, \pi\, N_i \rho_{sol}}\right)^{\frac{1}{3}}$.

Reviewer comment round 1: Your analysis focuses on intensive mean quantities, which depend on the contrail-cross-section of the contrail. It would be interesting to also see integrated quantities like the total ice crystal number and mass (which do not depend on the definition of the contrail boundary).

Authors answer round 1: We believe that the definition of the contrail boundary by rp>rs is valid enough to define the contrail boundary with no ambiguity. As mentioned in the previous answer, in our model if a particle radius is less or equal to its core soot radius, it is not an ice crystal. Therefore averaging on every cell where rp>rs consider all of the ice crystals in the computational domain.

Reviewer comment round 2: I think, you misunderstood the comment. The comment aimed at raising awareness, that the total effect of a young contrail is better described by extensive quantities that means quantities that are integrals over the contrail-cross section. The time series plots in Figs. 7, 14, 16,19, 21, 22,24 all show only intensive quantities. The newly included Fig. 9 is the only figure that shows total quantities. Unfortunately, they are apparently illdefined as stated further below. Hence, none of the current plots shows total values. Hence, it is nearly impossible to interpret your simulation data in terms on implications on the contrail effect.

Authors answer round 2: See previous answer. Intensive quantities have been replaced by extensive quantities.

SPECIFIC COMMENTS:

The reviewer comments are in bold.

COMMENT 1:

- Line 44: The ice crystals do not heat up adiabatically. The surrounding air does so:
-> *adiabatic heating replaced by heating (line 49)*
You misunderstood what I meant. The air heats up and leads to an increase of the saturation pressure. Ice crystals may relax to the air parcel's temperature. But the important aspect is the adiabatic heating of the air. (Your original formulation stated that ice crystal heat up adiabatically, but this works only for gases!)

This has been corrected (see line 54).

COMMENT 2:

- I would prefer to formulate to something like "Contrail evolution is driven or governed by physical processes (not by conditions) which are affected by (conditions like) wind shear, stratification etc.
-> *Modifications implemented accordingly.*
Could you list the physical processes instead of just saying 'certain number of physical processes'?
The physical processes have been listed (line 64)

COMMENT 3:

- **Eqs. 2, 3 and 6 do not convey a lot of information. With which rates do those conversions occur? Would it be more informative to write the equations for the mass fractions of all or selected quantities? :**
--> *The mass production rates are now given in Eq.2 and Eq.4*

The transition from free state to adsorbed states occur through the reactions $SO_3 \rightarrow SO_{3,ads}$ and $H_2SO_4 \rightarrow H_2SO_{4,ads}$. Sentences like this do not convey much information. It remains open with which rate the conversions occur.
What I miss, is a list of all prognostic equations; only the one for soot is given in Eq.1. It would help to see the analogous equations for all other species. They are more complicated, as they contain source and sink terms. Hence, it would be good to write them down.

The full RANS and LES equations for a multi-species gas mixture and solved in our simulations are now given in Section 2.1.1 and 2.1.2. We give the general transport equation for the mass fraction of each species in the gas mixture, including ice water. The transport equation includes source term. The source term for adsorption is given in Eq.9.

COMMENT 4:

Section 3.2 does not explicitly mention how water vapour is initialized. The sentence in line 281 may imply that absolute water vapour mixing ratio is held constant with altitude. Most other studies of the contrail vortex phase kept the relative humidity constant. Could you plot RHi(z) to see how much this quantity changes with altitude?

-> Ambient water vapour is initially constant in the computational domain. We decided to keep ambient water vapour constant instead of RH after informal discussions with climate scientist colleagues. ISSR measurements are needed to accurately define RH and water vapour profiles in the tropopause. RH profiles are now given in in Fig. 5 for the two stratification scenarios. With this hypthesis very high values of RH are reached at the bottom of domain in the strong stratification case. However, no ice crystals descend at such altitude. This point has been developed clarified in line 301.

Fig. 5 needs a lot of space. The ratio of information content over space is quite low. Two lines with vertical profiles would be sufficient. Alternatively, you can show the RHi fields at the end of the simulation. Then, the contrail vertical extent would be directly visible.

How is the flight altitude chosen? It appears to be at some value >0? In Fig. 6 cruise altitude seems to be at z=0. Please clarify.

I do not think that ISSR measurements are needed for your application to accurately define RH and water vapour profiles. The spatial variability in nature is very high. Hence, there is no unique "precise profile". Your profiles should be plausible and not the most extreme examples of what could occur in reality. In this sense, your profile for the strong stratification case is not really appropriately chosen. Supersaturation values below z=-250m are just too high to occur in nature. It is not very comforting to see that one of two meteorological scenarios does not really make sense. I cannot rate how much your results are affected by using such high peak RH values.

Fig. 5 has now been changed to two lines with vertical profiles of RH. We have also added the RH contour inside the plume at the start of the LES simulation to help the reader visualize the initial position of the plume and RH values in the plume at the start of the simulation.

We recognize that we have not been clear enough for the relative humidity profile. First, it is important to differentiate plume initial altitude and flight altitude. For the LES simulation, the plume initial altitude is chosen in the plume area in the LES domain. For the RANS simulation, the flight altitude corresponds the z-coordinate of the wing trailing edge. For both cases, this corresponds to an altitude z ~ 0 in the corresponding reference frame even though it is not exactly the same value of z between the RANS reference frame and the LES reference frame. This is not contradictory because the LES reference frame and the RANS reference frame are different. In the RANS domain, computations are carried out in the aircraft reference frame: the aircraft is horizontal, and the incoming airflow enters the domain with an angle equal to the angle of attack (=3°). In contrast, the LES domain is defined in the ground reference frame, where the aircraft is tilted by 3°. This is explained more in detail in Bouhafid et al. 2024 https://doi.org/10.1016/j.ast.2024.109512.

Regarding the plausibility of the strong stratification scenario, we first note that the elevated humidity levels below z = -300 m are not accessed by the contrail when using RANS initialization (see Fig. 13). This occurs due to the pronounced stratification and its stabilizing effect on the wake system. Under the more stable vortex pair conditions achieved with 2LO initialization, the contrail reaches levels below z = -250 m but remains above z = -300 m, where RHi approaches approximately 165%—a value that appears within the range of atmospheric observations in the North Atlantic flight corridor (see Ovarlez et al. https://doi.org/10.1029/1999JD900954). This work shows that a RHi value of 165%, while uncommon, is not exceptional in cloudy environment (see Plate 2 and Fig 1 of https://doi.org/10.1029/1999JD900954). Thus, we believe that the zones in the LES domain where RHi is extraordinarily high (RHi>200%) do not influence our results as they are not reached by the contrail.

Finally, investigating scenarios with higher than usual RHi holds significant scientific value due to the complex physics involved and the quantitative insights into how stratification influences contrail evolution when its influence becomes dominant in controlling contrail dynamic. This could be seen as an interesting academic exercise.

<u>COMMENT 5:</u>

- Fig. 9: the contrail height and width evolution of the RANS case with Nb =0.03 s^-1 looks a bit strange in the sense, that at t=4.5 the height suddenly stops to increase (which might be linked to vortex break up) and width increases. What process leads to such a large change in the width increase?
->The width increase is most likely due to the strongly turbulent nature of the secondary wake that will mix the ice crystals with the ambient air way more efficiently. This has been clarified in line 440.
Line 440: "contrail height stops increasing". Contrail height is typically used to describe at which altitude a contrail is located. Better say, that "the contrail vertical extent does not increase anymore".

"Contrail height" has been replaced by "contrail vertical extent." for every occurrence in the manuscript.

<u>COMMENT 6:</u>

- **Section 4.1. It would be interesting to also see the time evolution of total ice crystal number Nice and mass Mice and possibly also of the ratio Nice/Ns. Mice and Nice are more straightforward to analyse and interpret than the derived mean radius ~(M ice /N ice) 1/3 . Computing the mean radius via Mice and ice is probably better than evaluating rp in each grid cell and do a number-weighted average. How much do the computed values differ between the two formulas? The formula in line 325 might be interpreted in a way that rp depends only on IWC. I would prefer to include Nice in the formula**

-> We have added the evolution of total ice mass and total ice crystal number in Fig. 9. Concerning the mean ice crystal radius, we are not sure to understand your comment. We believe that knowing the ice crystal radius in each cell of the contrail and doing a weighted average gives a good overview of the ice crystals size in the contrail. rp formula as a function of IWC and N_ice is given in Eq.7.

Point 1
Fig.9: Why does the ice crystal number continuously increase and the ice mass not? I think you misunderstood what I meant with Nice. Nice should be obtained by integrating over the cross-sectional area of the contrail at each downstream distance. Nice then gives the ice crystal number per meter of flightpath in units m^-1. Nice(x) gives then the ice crystal number for different downstream distances/contrail ages! I expect that Nice first increases during ice crystal formation, then it may reach a plateau and further downstream it might likely decrease due to sublimation processes. It seems that your Nice(x) are integrals not only over the cross-section but also from zero up to x along flight direction. I do not see what this quantity should tell us.

Point 2:

As mentioned, the weighting does not make sense for several quantities you show. As written, the number weighting makes in theory sense for computing a mean radius. However, this is more complicated than it should. Once you evaluate the total ice mass and number Mice and Nice, the mean radius can be derived via (Mice/Nice)1/3, which is more straightforward than your approach.

Nice(x) is now defined for the RANS calculation as the integral over the cross-section at different downstream position. Nice(x) quickly reaches a plateau (around x/b=6). Moreover, we now use your definition of the mean radius for the LES simulations.

COMMENT 7:

-Line 330: If all soot particles are activated and more ice crystals are present, then they should be smaller not larger if ice mass is similar.

-> This is true but this effect is not considered in our Eulerian model. More precisely, our model considers that soot surface activation is done only with sulfur compounds. Ice production term in Eq.4 is directly proportional to activation fraction. Consequently, the higher the activation fraction, the higher the ice production and the higher the ice crystal radius. This represents a limitation of our model, as it should also consider soot activation caused by the ice cap formed on soot particles, and not only activation due to sulfur particles. This point has now been clarified in line 355.

Unfortunately, I do not understand your argumentation. I think I understood how the activation works. But at a later stage, when ice crystal formation is completed, I do not see why this should lead to larger ice crystals. Assuming the same total amount of water vapor is depleted onto the ice crystals, having more ice crystals should, on average, result in smaller crystals compared to when the same water mass is distributed over fewer crystals. Could you try to explain your reasoning in a different way?

Let us assume that the same total amount of water is depleted onto the ice crystals. In our model, ice will form on the portion of the soot surface that is activated. This portion of the soot surface is proportional to the activation fraction θ. Mathematically, this implies that ice production rate is proportional to θ regardless of the ice crystal size. The lower θ is, the less ice is formed on particles on a given time interval and the slower ice crystals will grow. This results in smaller ice crystals. For the same total amount of water and same ambient relative humidity, ice crystals will be smaller in the case $\theta < 1$ than in the case $\theta = 1$. The relevant parameter here is the rate at which water vapor converts to ice in addition of total amount of water in the plume.

To put it shortly, the answer to your question lies in the fact that even when ice crystals are formed, the activated surface fraction value does not depend on ice crystal size. This is a strong hypothesis that leads to underestimating ice crystals size. This behavior is erroneous and constitutes one of the limits of our microphysical model. In reality, when ice crystals are formed and large enough, θ should be set to 1 to translate that activation is not longer important for ice crystals growth.

TECHNICAL CORRECTIONS:

- Line 21: estimation of effective radiative ERF of contrails and other forcing agents.

-> Correction added.

Your corrected sentence is not well-formulated. "This work enabled the estimation of contrails' Effective Radiative Forcing (ERF) and other forcing agents such as CO2, NOx, aerosols and water vapor".

What does other "forcing agents such as CO2, NOx, aerosols and water vapor" refer to?

“Estimation of other forcing agents?” (no!)
“Estimation of contrail’s ERF of other forcing agents?” (no!)
Moreover, Lee did not enable the estimation, they only reviewed, summarized and re-scaled existing studies.
My proposition: “This work provided an estimate of the Effective Radiative Forcing (ERF) of contrails and other forcing agents such as CO2, NOx, aerosols, and water vapor.”
Your proposition has been implemented. Thank you.

Further comments on newly added text parts:
Line 68: ‘…then act as condensation nuclei’ makes no sense. When ice crystals are already formed, they cannot act again as nuclei!
You are right, this has been removed from the article.

Fig.6 needs a lot of space without containing much information. You could cut the blue domain.
The figure has been cropped.
Line 350: changes instead of evolves
Implemented to the paper.
422: ice crystal number concentration
Implemented to the paper.

Line 580: full RANS initialization
Implemented to the paper.