Review of revised version of Bouhafid & Bonne

The replies of the authors to my first-round comments are not satisfactory, in particular most major issues have not been addressed properly. Resolved issues are not listed here, only open issues are repeated. The style of this document is as follows: In red, you can see the new comments of this second-round review. The text in black is extracted from the author's reply (it lists the reviewer comment of the first round, the author's answer, and manuscript changes use italics).

The authors should put more effort in addressing the reviewer's comment, otherwise it becomes tedious for the reviewers.

Most figures show quantities that are not really relevant for the later climate impact of contrail-cirrus. Some of the quantities can be shown to explain early contrail processes, but your descriptions tend to say that every effect you see can have a long-lasting impact on the mature contrail.

I understand and acknowledge that it is out of scope to present contrail-cirrus simulations in this study. Yet, the selection of plots and displayed quantities and the interpretation of the results should reflect what is known about the connection between early contrail properties and the radiative impact of the contrail-cirrus over its full life cycle.

GENERAL COMMENTS :

Reviewer comment: 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: 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.

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

Reviewer comment: 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: 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.
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.

Reviewer comment: 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: "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.

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.

Reviewer comment: 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: 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.

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.

Reviewer comment: 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: 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.
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?

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.

Reviewer comment: 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: 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.

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 ill-defined 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.

SPECIFIC COMMENTS :

The original reviewer comments are in bold.

- 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!)

- Line 52 : I would prefer to reformulate 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?

- **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" to "adsorbed" states occurs through the reactions SO3 → SO3,*ads* and H2SO4 → H2SO4,*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.

- 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. 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?

→ Height stopping to increase is due to vortex break up which happens sooner for Nb=0,03 s^-1. 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".

- **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 N_{ice}. N_{ice} should be obtained by integrating over the cross-sectional area of the contrail at each downstream distance. N_{ice} then gives the ice crystal number per meter of flightpath in units m^-1. $N_{ice}(x)$ gives then the ice crystal number for different downstream distances/contrail ages! I expect that N_{ice} 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 $N_{ice}(x)$ 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 M_{ice} and N_{ice}, the mean radius can be derived via $(M_{ice}/N_{ice})^{1/3}$, which is more straightforward than your approach.

-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 taken into account 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?

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, NO*x*, aerosols, and water vapor.".

What does "other forcing agents such as CO2, NO*x*, aerosols, and water vapor." refer to?

"Estimation of other forcing agents?" (no!)

“Estimation of contrail’s ERF of other forcing agents?” (which makes no sense!)

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, NO*x*, aerosols, and water vapor.”

Further comments on newly added text parts:

Line 168: ‘…then act as condensation nuclei’ makes no sense. When ice crystals are already formed, they cannot act again as nuclei!

Fig.6 needs a lot of space without containing much information. You could cut the blue domain.

Line 350: “changes” instead of “evolves”

422: ice crystal number concentration

Line 580: full RANS initialization

Summary:

The revised version and also the author reply revealed many shortcomings of the current study design and data analysis. In my opinion, much more changes are needed than what the authors offered in the first revision round. In particular, the selection of analysed and displayed quantities must be strongly adapted to focus on aspects that really matter for the long-term contrail evolution. And more connections to existing contrail formation and contrail vortex phase studies should be made.