Responses to Anonymous Referee #3

“The writing style is in my opinion too informal and formulations are too often too vague and not precise enough. Moreover, it is not explained sufficiently how you compute specific values. Some examples are listed in the specific and technical comments, but my list will not be exhaustive. Hence, I strongly recommend that the authors go over the whole manuscript and work on the text. Sloppy formulations make the life of the reviewers and future readers harder than it should be.”

Author’s response: We appreciate the comment and have gone through the text as suggested, correcting not only the specific issues highlighted by the reviewer but also seeking to be more specific with our technical explanations. We apologize for any difficulties caused by our oversights, and hope that the modifications made to the manuscript have resolved this issue.

Author’s changes in manuscript: Several changes have been made throughout the text, improving formulations, choice and clarity of language, and adding units where they were lacking.

“I am not sure if “fallstreak” and “post-fallstreak” are good expressions for what you want to describe. The first phase (that you refer to as “fallstreak”) is dominated by the creation of the fallstreak that fills the moist layer underneath the flight altitude over time. Once the fallstreak covers the whole layer, you speak of “post-fallstreak”. In my understanding, the contrail at that stage still consists of a contrail core and a fallstreak. The fallstreak continues to exist and is fed by ice crystals falling out of the contrail core. Hence, I would not call it “post-fallstreak”.

[In the following review, I will stick to your terminology and will not make any further comments whether I think the terminology is appropriate.]

Moreover, you speak of a Cocip fallstreak. As a single Gaussian plume is used in Cocip, this model cannot represent the bimodality of the contrail (i.e. contrail core and fall streak as e.g. described in the high-resolution modelling study by Lewellen 2014). Hence, referring to the Cocip plume as Cocip fallstreak is misleading as the Cocip plume falls only slowly in the beginning and accelerates only very late in its lifecycle. Why not use the more neutral term ‘Cocip (Gaussian) plume or contrail’ throughout the text?”

Author’s response:

We appreciate that our choice for the sub-regimes identified in this study are not the most appropriate given the pre-existing connotations of the word “fallstreak”. After much consideration, we have come up with these alternatives:

- Fallstreak sub-regime -> unrestrained sub-regime
    - We believe that the word “unrestrained” accurately reflects the fast vertical separation of the precipitation plume from the contrail core.

- Settling sub-regime -> restrained sub-regime
    - The contrail now spreads in a slower manner and primarily in a horizontal direction, so we believe that "restrained" is more appropriate than "settling".
- Fading sub-regime -> unchanged

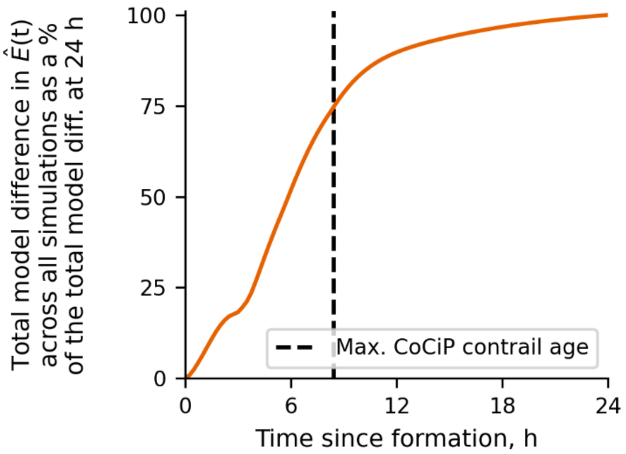
Furthermore, we have replaced the reference to "fallstreak" in the title with "Zero-Dimensional model" to avoid confusion.

Author's changes in manuscript: Changed the sub-regime names in the manuscript text and figures as required. Also changed the title from "Contrail models lacking post-fallstreak behavior could underpredict lifetime optical depth" to "Zero-dimensional contrail models could underpredict lifetime optical depth"

"The prescribed meteorological scenarios are highly idealized and it is likely that subsidence causes the contrail to sublimate before it reaches an age of 15 hours. Hence, the comparison should emphasize the early differences more than the discrepancies beyond 10 hours. I doubt the "fading sub-regime" will be encountered as such very often.

The change in the slope of the ice crystal number reduction might be a particular result of APCEMM and the idealized scenario used. In reality, vertical motions in the atmosphere will perturb the contrail evolution."

Author's response: This comment raises two points. First, it suggests that we should focus on the contrail simulations up to 10 hours due to how uncommon contrails lasting beyond this are. To consider this suggestion in a fair manner, we have produced an additional figure showing the sum across all unique simulations of the difference in time-integrated total extinction between the models. For ease of interpretation, this has been normalized with the total difference (summed across all simulations) at 24 hours. Each time on the x axis serves as the upper limit of integration, hence the variation with time.



After much consideration, we have decided to not shift the focus of the analysis to the first 10 hours. This is because, according to the figure above, 90 % of the model difference in the time-integrated total extinction is produced within 12 hours from formation. Hence, our conclusions still hold even if contrails that persist for longer 12 hours are rare. Nevertheless, we are grateful for this suggestion for the opportunity it gives us to improve the narrative. We have included this figure and the above explanation in Section 3.2.1.

The second point raised by this comment is later reiterated in RC3-14. In summary, we agree that our mathematical definitions of the sub-regimes may not hold in all cases. However, we provide these definitions as a way to deepen our understanding of the behavior of contrails. Please refer to the response to RC3-14 for a more detailed response.

Author's changes in manuscript: Included the above figure and explanation in a new section: Section 4.1.3:

"To understand the sensitivity of our findings to the contrail lifetime, we define the global model difference ($\underline{\delta}$) as the sum across all simulations of the APCEMM integrated total extinction minus the CoCiP integrated total extinction at each timestep:

$$\delta(t) = \sum_{\text{all cases}} \left(\hat{E}_{\text{APCEMM}}(t) - \hat{E}_{\text{CoCiP}}(t) \right), \tag{5}$$

$$\hat{\delta}(t) = \frac{\delta(t)}{\delta(t = 24\text{ h})}. \tag{6}$$

where $\hat{\delta}(t)$ is the normalized global model difference. The variable t in Eqs. 5 and 6, is the upper limit of integration in Eq. 2.

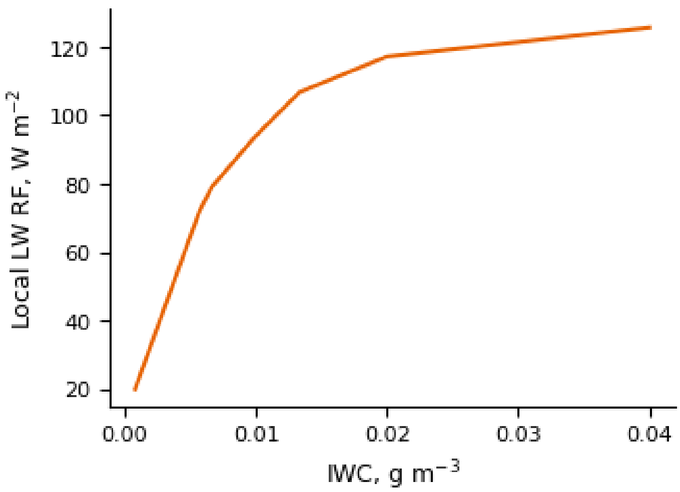
Figure 10 shows how $\hat{\delta}(t)$ varies as a function of time. We hence find that 90 % of the global model difference is produced within 12 hours from formation. For more evidence-based contrail lifetime estimates, we take 4 h and 8 h from a recent preprint by Hofer and Gierens (2025). The proportion of the total model difference reached by 4 h and 8 h are 27 % and 72 % respectively. These results indicate a large sensitivity in our findings to the lifetime of typical contrails. However, they also indicate that our findings are particularly relevant to those 6–7 % of contrails that persist beyond 8 h (Gierens and Vazquez-Navarro, 2018). Such contrails are also likely to be the greatest contributors to aviation warming on an individual basis, and are hence important for contrail avoidance."

For the resolution to the second point raised in this comment, please refer to the changes listed in a later comment.

"Line 39: Can you substantiate the statement about thin contrails having the largest cloud radiative effect (what you call local RF)? Unterstrasser & Gierens (2010) and Lewellen (2014) show at least the dependence on wind shear."

Author's response: We appreciate that we have been vague in this explanation, and we welcome the suggestion to substantiate the point. Please find here an explanation of what we mean:

For the following analysis we reproduce part of the curve corresponding to 5 µm from Fig. 4a) of Wolf et al. (2023):



Consider two contrail segments under the same ambient conditions with the same ice mass per unit contrail length of 20 kg m-1, with both contrails having the same depth of 500 m. Assuming that contrail A is 1 km wide, and that contrail B is 2 km wide, the ice water content (IWC) of contrail A (0.04 g m^{-3}) is two times the IWC of contrail B (0.02 g m^{-3}). Fig. 4b) of Wolf et al. (2023) shows that contrail A will have an instantaneous longwave radiative forcing (LW RF) of ~125 W m-2, while contrail B will have an instantaneous LW of ~115 W m^{-2}. Accounting for the contrail width, contrail A will have a LW RF of ~125 W m^{-1}, whereas contrail B will have a LW RF of ~230 W m^{-1}. Contrail A, the one with higher optical depth (due to its higher IWC), will have a lower energy forcing than the more dilute but wider contrail B. This implies that, for the same total ice mass, contrails that have a large horizontal span but are optically thin may have a greater climate impact than thicker, narrower contrails.

Author's changes in manuscript: Created a new Appendix "G" with the above explanation. In line 41 of the updated manuscript, we replaced:

"Since the local RF of an ice cloud increases fastest with ice water content for thin contrails"

with "Using data from Fig. 4(a) from Wolf et al. (2023) it can be shown that, out of two contrails of different width but the same length, depth and total ice mass, the wider contrail has a higher energy forcing than the narrower contrail (see Appendix G)." and removed "such large, thin contrails are expected to have greater total climate impact than narrower, more visible contrails".

"Line 40: Typically, the introduction of scientific publications does not summarize the results of the present study."

Author's response: We agree that the quantitative discussion of a simulation is not appropriate for the introduction, and have removed explicit discussion of the results in the introduction. Although Figure 1 does show a result of sorts, we have decided to keep it as we believe that it works well as an illustrative example.

Author's changes in manuscript: Replaced "For a typical contrail simulated in APCEMM (Fig. 1), assuming an optical depth observability threshold of 0.1 (Kärcher et al., 2009), 25 % of the lifetime optical depth is produced in the unobservable period"

with "It is hence possible that a significant proportion of the time-integrated total extinction (a proxy for climate impact) remains unaccounted for when relying on observations, as shown by the illustrative example in Fig. 1" in line 45 of the updated manuscript

"Line 44: In my opinion, contrails with cross-sections of 100km2 represent extreme cases. Or did you want to say, the grid boxes of the gridded models are 100km2? In this case, please reformulate."

Author's response: We agree that the chosen value of 100km^2 is representative of extreme cases only and have amended the text to indicate a way this estimate is calculated

Author's changes in manuscript: In line 47 of the revised manuscript, replaced: "since they have cross sectional areas of ~100 km^2"

with "A study by Dickson et al. (2009) found that 53 % of the ISSRs they observed were between 100 and 1500 m deep, and the large eddy simulations conducted in Lewellen (2014) had widths of ~50 km in the transversal direction (defined to be along the horizontal plane perpendicular to the flight direction). Furthermore, a single flight was identified as responsible for creating a cirrus cloud with a bounding box width of 130 km (measured from Fig. 12 (c) in the study by Haywood et al. (2009)). Therefore, the largest persistent contrails can reach cross-sections of up to ~100 km^2 in the transversal direction, making gridded simulations of sufficient resolution computationally expensive."

Haywood, J. M., Allan, R. P., Bornemann, J., Forster, P. M., Francis, P. N., Milton, S., Rädel, G., Rap, A., Shine, K. P., and Thorpe, R.: A case study of the radiative forcing of persistent contrails evolving into contrail-induced cirrus, J. Geophys. Res.-Atmos., 114, https://doi.org/10.1029/2009JD012650, 2009.

"Line 47: This sentence is very general and contains little information. Which models were compared to each other? In which paper was the comparison done? What are the main findings?"

Author's response: We agree with this comment and have improved the description of the ways in which the previous study agree and disagree with each other.

Author's changes in manuscript: Replaced "The limited comparisons that have already been performed for large eddy simulations indicate disagreement in this regard (Unterstrasser and Gierens, 2010a; Unterstrasser and Gierens, 2010b; Lewellen et al., 2014; Lewellen, 2014)" with “A study employing full-lifetime large eddy simulations (Lewellen, 2014) compared its findings with a prior similar study (Unterstrasser and Gierens, 2010a – UG10a; Unterstrasser and Gierens, 2010b – UG10b), and found that “some of the inferences given in UG10a and UG10b are not supported by the present study” and “several of the parameter dependencies discussed here were found previously in UG10a and UG10b” (Lewellen, 2014). Specifically, both studies determined that the total extinction (a proxy climate impact metric) increases with the relative humidity, temperature, and initial contrail ice number. However, they found different parameters dominating the changes in total extinction: relative humidity in Unterstrasser and Gierens (2010a and 2010b), and shear in Lewellen et al. (2014) and Lewellen (2014). Since two models of similar complexity found different dominant factors in predicting a proxy for contrail climate impact, this suggests the need for a more comprehensive assessment of the robustness of contrail modelling techniques being used to inform contrail impact mitigation” in line 56 of the revised manuscript.

“Line 77: I thought Cocip only tracks a Gaussian plume for the ice crystals and the humidity is taken from NWP data. Why is it necessary to have a plume of water vapour concentrations?”

Author’s response: This is true; CoCiP does not assume a Gaussian distribution for the water vapor, only the ice mass. However, CoCiP does keep track of the mass of air in the plume while assuming 100% RHi internally, hence keeping track of the water vapor within the contrail implicitly. Appendix F gives more information about why this is relevant for the study. To address the inaccuracy in the CoCiP description, we have removed “water vapor” from line 98 of the revised manuscript, which talks about the Gaussian assumption in CoCiP.

Author’s changes in manuscript: Removed “water vapor and” from line 98 of the revised manuscript.

“Line 96: are the bins fixed in radius space or dynamic as in Lewellen 2014?”

Author’s response: APCEMM uses ice radius bins with fixed widths in radius space, but with a dynamic modal radius within each bin (Fritz et al., 2020). Specifically, it implements the scheme described by Jacobson (1997). We have added this description to the text.

Author’s changes in manuscript: Added the following sentence to Section 1.1.2 in line 122 of the revised manuscript: “These bins are fixed in radius space, but the modal radius of each bin is allowed to increase within the bin bounds to accommodate the increase in ice crystal sizes with time.”

“Line 82: ‘evaporate’ or is it ‘vanish/disappear’?”

Author’s response: We extend our appreciation for highlighting the misuse of terminology referring to specific physical processes. We meant to say “disappears”.

Author’s changes in manuscript: Change “evaporates” to “disappears” in line 103 of the revised manuscript.

“Line 114: I am not sure whether the title is appropriate. Could you reformulate it? “Meteorological background scenarios/data”??”

Author’s response: We agree with the suggested change and have implemented it.

Author’s changes in manuscript: Changed the title of section 2.1 from “Weather parametrization” to “Description of the background meteorology”

“The quantities you define in Eqs. 1 and 2 have been used in previous studies, yet with other names. It would be good to make the connection to those studies. Unterstrasser & Gierens 2010 introduced the total extinction, which is equal to your definition of γ. Since then, total extinction has also been evaluated in the context of GCM contrail simulations (Bier et al, 2017). Moreover, total surface area S in Lewellen 2014 is basically the same as total extinction (except for a constant scaling factor of 2). Your definition of “lifetime optical depth” was introduced as ‘(life)time-integrated total extinction’ in Unterstrasser (2020). I would recommend to stick to one of the names that have been previously introduced to make clearer that all these studies analyse basically the same quantity.”

Author’s response: We agree with this comment, which concurs with a similar concern by Anonymous Reviewer #1 (RC1-05). We have chosen to use the “total extinction” term from the 2010 papers by Unterstrasser and Gierens.

Author’s changes in manuscript: We have renamed our “lifetime optical depth” variable with the “time-integrated total extinction” throughout the manuscript.

“The crystal loss rate is not well-introduced and I stumble across the units. Is the logarithmic derivative of N(t) used?”

Author's response: We are grateful for having received feedback on the clarity of the formulation for the crystal loss rate, which is indeed the logarithmic derivative. We agree that the absence of its formulation in the original version of the manuscript makes it confusing for the reader. We have addressed this in the manuscript.

Author's changes in manuscript: Added the following sentence to the end of section 2.2 in line 178 of the revised manuscript "Due to the order of magnitude changes in total ice number throughout the contrail lifetime, we define the ice crystal loss rate, $-\frac{d \log_{10} N}{dt}$ In decades per hour."

"Around line 190: You analyse dI/dt and d2I/dt2 which serve as conditions in a contrail phase classification. It makes the impression that those conditions can used as classification criterion across different scenarios. I doubt that the signs of these two quantities are universally interpretable as they may depend on many parameters (such as the thickness of the moist layer, vertical air motions and so forth)."

Author's response: This comment raises a valid point regarding the implied applicability of the criteria determining the diffusion sub-regimes. We agree that, under realistic meteorology, our definitions may not hold. We would like to clarify, however, that we are not attempting to make a universal classification. Instead, we are trying to understand the behavior of the system. We have added a comment to clarify this in the revised manuscript.

Author's changes in manuscript: Replaced: "Using the simplified description of the contrail cross-section, mathematical definitions of the sub-regimes observed in Fig. 5 can be formulated by considering the total ice mass per unit length (I)"

with "Mathematical definitions of the sub-regimes observed in Fig. 5 can be formulated by considering the total ice mass per unit length (I), with the caveat that they are only likely to be valid for contrails simulated in idealized meteorology" in line 220 of the revised manuscript.

"Section 3.2.1: The study by Bier et al (2017) also analysed what factors limit the contrail lifetime. Matching maximum ISSR lifetimes alone are not a sufficient criterion ensuring that your meteorological background state is representative. The characterization of the sub-regimes is more complicated in scenarios where the background humidity changes over time due vertical air motions and ice mass evolution changes by those 'external' drivers. Hence, your claim of widespread applicability is probably a bit overselling."

Author's response: We agree that the "widespread applicability" claim is not accurate, especially since contrails can disappear through synoptic processes and not through sedimentation. We now limit our claim to focus on long-lived persistent contrails only.

Author's changes in manuscript: Replaced "may have widespread applicability" to "may be applicable to some long-lived persistent contrails, likely including some of the contrails that are responsible for 80 % of the climate impact (Teoh et al., 2024)." in line 273 of the revised manuscript.

"If I understand Fig. 6a correctly, the x-coordinate for the blue and the according orange data point are the same. Correct?

Author's response: Yes, that is correct. We have added a clarifying statement to the caption of Fig. 6 to address this.

Author's changes in manuscript: Added "Each entry in the parity plot corresponds to one simulation." To the caption of Figure 6.

I understand the information given in the text about which fraction of the contrail lifecycle is unobservable (based on τ<0.1). Basically, Cocip contrails are nearly always observable and 100% of their lifecycles belong to the fallstreak regime. Due to these rather peculiar values, the panel b is difficult to understand. First of all, the legends in the two panels say 'fallstreak only' and 'fall streak'. Is this the same criterion?

Author's response: We appreciate that the original version of panel b in Fig. 6 was confusing. As pointed out in comment RC3-02, CoCiP does not really have a fallstreak. We have removed the purple dots in Fig. 6(b) since they were distracting from the intended message.

Author's changes in manuscript: Removed the purple dots in Fig. 6(b).

In the text you mention that on an aggregate level, 92% of ΓAPCEMM comes from post-fall streak regime. It is not explained how you derive this number. Is this the ratio of the orange and blue slope in Fig. 6a? Are all data points equally weighted in the averaging? Do you take the average over the ratios ΓAPCEMM, fallstreak / ΓAPCEMM all ?Or do you sum up over ΓAPCEMM, fallstreak and ΓAPCEMM all separately and then compute the ratio of the two sums? Similarly, I miss information about how the values 35% and 15% (in lines 248 and 249) are computed. Are these the mean values of the orange data points in x and y direction in Fig. 6.2?"

Author's response: We agree with the critique about the clarity of our statistical analysis. The 92% is a ratio of sums, calculated by adding all integrated total extinction after the unrestrained sub-regime across all simulations. This number is divided by the sum of the integrated total extinction across all simulations. No weighing is performed. The 35 % is a similar ratio of sums, with the threshold being the observability point. We have clarified how these and similar quantities are calculated in the manuscript.

Author's changes in manuscript: Amended the text in Section 4.1.1 for clarity: "Figure 6(a) compares the time-integrated total extinction from CoCiP and APCEMM when considering all contrail lifetime (orange) and when only considering the unrestrained sub-regime (purple).

The CoCiP and APCEMM simulations disagree regardless of whether the entire lifetime or the unrestrained sub-regime are considered in isolation. When only the APCEMM unrestrained sub-regime is considered, CoCiP simulations have time-integrated total extinction values 3.3 times larger than those from the corresponding sub-regime in APCEMM (given by the reciprocal of the slope of the purple dashed line in Fig. 6(a)). The case in which all sub-regimes are considered lies above the parity line, with APCEMM simulations having a time-integrated total extinction 3.8 times that of CoCiP (given by the slope of the orange dotted line in Fig. 6(a)).

The relationship between the proportion of the time-integrated total extinction in the unrestrained sub-regime and unobservable regions is displayed in Fig. 6(b). Considering the following sums across all 14 unique simulations:

$$\sigma = \frac{\sum_{\text{all cases}}\left(\hat{\mathrm{E}}_{\text{model}}(\mathrm{t}=\mathrm{t}*)\right)}{\sum_{\text{all cases}}\left(\hat{\mathrm{E}}_{\text{model}}(\mathrm{t}=24\ \mathrm{h})\right)}, \quad (5)$$

where t* is a chosen integration threshold, we find that 92 % of APCEMM time-integrated total extinction is produced after the unrestrained sub-regime, and 38 % is produced when the contrail is unobservable. In contrast, across all simulations CoCiP produces none of its time-integrated total extinction beyond the unrestrained, and 17 % beyond the observability threshold." This starts at line 281 of the revised manuscript.

"section 4.2.2: You compare APCEMM and Cocip sensitivities with those found in Lewellen 2014. Unterstrasser & Gierens 210a,b also studied contrails in scenarios with constant RHi and analysed the sensitivities to most of the parameters listed in your table 2. Hence, it would help including the findings from these studies in your discussion."

Author's response: We recognize that including the Unterstrasser and Gierens paper will improve the strength of the conclusions and the scientific quality of this manuscript. We accept the suggestions with appreciation.

Author's changes in manuscript: Added the following sentence to line 369 of the revised manuscript "Similarly, Fig. 4 and Fig. 6 in Unterstrasser and Gierens (2010a) also confirm that increasing the layer RHi increases the ice mass and the total ice crystal count respectively.".

Replaced the following sentence in line 395 of the revised manuscript:

"Lewellen (2014) find qualitatively similar results to APCEMM: increasing the shear leads to higher ice masses earlier and lower lifetimes" with "Other studies find qualitatively similar results to APCEMM: increasing the shear leads to higher ice masses earlier (Lewellen, 2014 and Unterstrasser and Gierens, 2010a) and lower lifetimes (Lewellen, 2014)."

“Abstract, first line: what does ‘optimized’ imply?”

Author’s response: We agree that the implications of the word ‘optimized’ are not clear in the abstract and throughout the rest of the manuscript. By “optimized” strategy, we mean strategies which aim to avoid only the most warming contrails. Such strategies are “optimized” because they reduce the fuel burn penalty relative to more aggressive strategies. Nevertheless, the implication is unclear, so we have replaced the words “optimized” where appropriate in the manuscript.

Author’s changes in manuscript: Removed the words “optimized” and “optimized strategies” when talking about avoidance strategies. Substituted “optimizes strategies” with “strategies using contrail models to select the specific contrails to avoid**”** in line 21 of the revised manuscript. Replaced “strategies involving optimized avoidance” with “strategies involving the prioritization of specific contrails by warming” in line 32 of the revised manuscript.

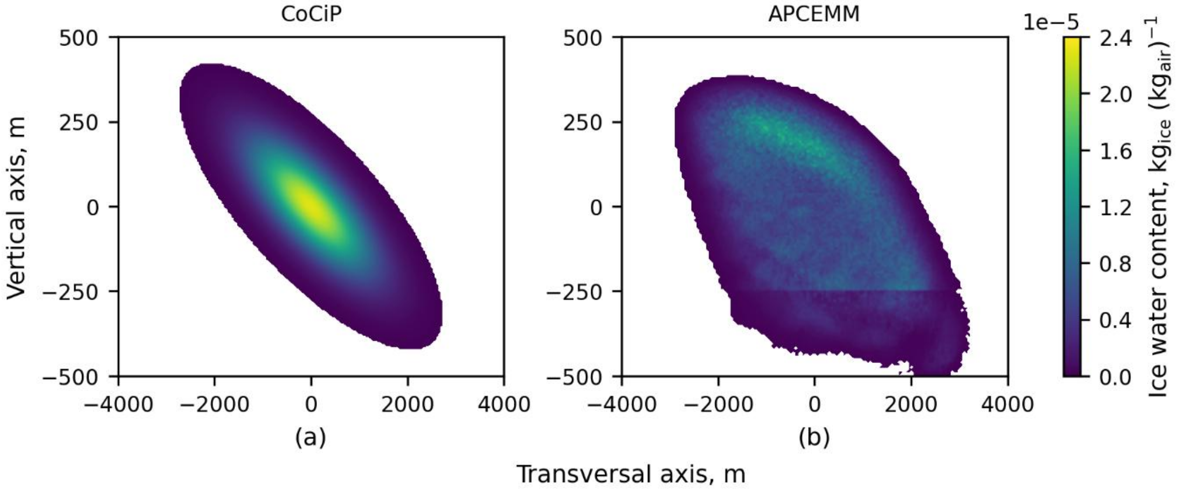
“Line 56: sublimation is a specific physical process, whereas formation and persistence are more general terms. Moreover, contrails can disappear through other physical processes. Replace ‘sublimation’ by ‘demise’?”

Author’s response: We agree that the words “sublimation” and “evaporation” (as well as their conjugated forms) were often misused in the manuscript. We have corrected the oversight.

Author’s changes in manuscript: changed “sublimation”, “evaporation” to “demise” throughout the manuscript. The conjugated forms “sublimates” or “evaporates”, have been changed to “disappears”.

“Fig.2: Would it be possible to use white as colour for the zero IWC bin, which would help to better identify the borders of the contrail?”

Author’s response: We appreciate that distinguishing the low values of IWC from the background is difficult. We have replotted Fig. 2 with a mask that cuts off IWC beneath 10^{-7} kg kg^{-1}:



Author's changes in manuscript: Changed Fig.2 to distinguish between the contrail and the background.

"In Eq.3, should the index be 3 (and not 'n')?"

Author's response: We are grateful for this correction and have implemented it.**Author's changes in manuscript:** Changed the subscript of the first Gamma in Eq.3 from "n" to "3".

"Line 199: "During the fall streak"?? and "fastest center of mass fall rate" (a rate is not fast, it is large)"

Author's response: An omission of the word "sub-regime" and a correction in the description of the center of mass fall rate are correctly highlighted in this comment. We agree with the point raised, and we have clarified the start of line 199.

Author's changes in manuscript: Replaced "During the fallstreak" with "During the unrestrained sub-regime" and "fastest" with "largest" in line 233 of the revised manuscript.

"Line 214: I am not sure what local optical depth means."

Author's response: We agree that "local" optical depth is not properly defined in the text. There are three instances of its use in the manuscript. In each of these instances, we either meant to say "vertical" or "average vertical" optical depth. We have changed this as required.

To be more specific, the contrail optical depth is calculated as follows:

- APCEMM: Each grid cell has an optical depth, which is related to the amount of ice water in the grid cell. The vertical optical depth (a function of width) in APCEMM is the sum of all grid cell optical depths for a given column.
    - The average optical depth is the vertical optical depth integrated along the width coordinate and divided by the width of the contrail.
- CoCiP: The optical depth is calculated for the whole contrail at once (Eqs. 58 – 61 in Schumann (2012)). In a sense, it is analogous to the average vertical optical depth calculated in APCEMM.

Author's changes in manuscript: Replaced "local optical depth" with "vertical optical depth" or "average vertical optical depth" as appropriate throughout the manuscript.

"Line 215: 'is produced at times where …'. Better use time instead of point to make clear it is about time and not space. Moreover, I would prefer to use plural to make clear you consider a time span over which the contrail is not detectable."

Author's response: We agree fully and have adopted this suggestion.

Author's changes in manuscript: Changed "is produced at a point where" to "is produced at times when" in line 250 of the revised manuscript.

"Caption Fig.6: 'unobservable'"

Author's response: We recognize this oversight and have fixed it in the revised manuscript.

Author's changes in manuscript: Changed the misspelled word in the caption of Fig. 6 to "unobservable".

"Line 262: 'at the end of the APCEMM fallstreak regime(?)'; 'shear does not increase the contrail width'. It is true that shear increases the contrail width. But here you want to say that a larger shear value leads to a larger contrail width."

Author's response: We agree and have rephrased the analysis to be more accurate. Furthermore, we omit the word "sub-regime" after the "APCEMM fallstreak" in the original manuscript.

Author's changes in manuscript: We change line 309 of the revised manuscript from "shear increases the contrail width by 143 % and the ice mass by 58 %, leading to a 36 % increase in lifetime optical depth" to "higher shear leads to an increase of the contrail width by 143 % and an increase of the ice mass by 58 %, leading to a 36 % increase of the time-integrated total extinction".

"Line 337: 'once the fallstreak ends': in time or space?"

Author's response: This refers to the point in time. We have modified the sentence in an attempt to improve the clarity.

Author's changes in manuscript: Replaced the sentence "It is also helpful to consider the effect that wind shear has on a contrail once the fallstreak ends" with "It is also helpful to consider the effect that wind shear has on a contrail after the time when the unrestrained sub-regime ends" in line 408 of the revised manuscript.

"Line 345 I believe it should be 'Contrail avoidance strategies that' because the following clause is restrictive. Same in line 385: 'which' -> 'that'."

Author's response: We agree and have changed the language accordingly.

Author's changes in manuscript: Replaced the sentence "Contrail avoidance which does not attempt" in line 416 of the revised manuscript with "Contrail avoidance strategies that do not attempt". Other replacements of "which" with "that" throughout the text are made as appropriate

"Line 348: and also lifetime-integrated radiative effects?"

Author's response: Although we recognize the added value that mentioning radiative effects would bring to the manuscript, we have decided not to include a specific reference to them after very careful consideration. This is because we have not performed any radiative transfer calculations. We see the primary purpose of this study to investigate the plume models without any supplements. We do admit, however, that this scope is limited given the global scale of the contrails problem. Therefore, we have updated the limitations section in the manuscript to recommend further studies running comparisons which include both radiative transfer calculations and a global analysis.

Author’s changes in manuscript: Added this sentence to line 473 of the revised manuscript: “Finally, to determine the full extent of the climate implications of the comparison, we encourage future studies to include radiative transfer calculations on a set of contrail simulations around the globe.”