Responses to Anonymous Referee #1

“I do not agree with the final conclusion that it would be currently better to avoid all contrails instead only the ones with strongest warming. With the current ATC system this would not be possible, at least not in dense airspaces.”

Author’s response: We agree - our intention is to highlight that there is insufficient agreement regarding which contrails are "big hits" between models, and thus more robust strategies should be considered. An example of a robust strategy is unbiased avoidance. This is because avoiding a certain proportion – say, 5% - of persistent contrails (with equal probability of targeting each individual persistent contrails), will result in a proportional reduction in contrail warming. In theory one could use a contrail model to instead select the most impactful 5%, but our results indicate that the ones chosen are likely to differ depending on the model – and that in fact one model may prioritize contrails which another model specifically predicts to be the least impactful. We have modified the manuscript throughout to more clearly reflect this argument.

Author’s changes in manuscript: Clarified in the conclusions, as well as throughout the manuscript, that the strategy that we propose is unbiased avoidance.

“My question here is whether it is actually monodisperse which means that every ice crystal has the same size (variance zero), or whether it rather may have a variance which is implicit. Note that many bulk cloud physics models (1-moment or 2-moment) implicitly assume a size distribution, where the variance is a function of the mean size. Please check, how CoCiP does this, i.e. whether it is monodisperse or perhaps monomodal.

Later, you say that CoCiP uses the ice mass and number and that the single size of all crystals is computed from these two values. There may be reasons for this, but in cirrus models, if they have 2-moment schemes (e.g. ice mass and number), one of the reasons for this is that this allows more freedom in treating size distributions implicitly.”

Author’s response: We agree that the description of the way CoCiP handles the different crystal sizes was not sufficiently clear in the manuscript. In short, CoCiP, tracks the total ice mass and total crystal number only, with various different implicit assumptions made regarding the size distribution. The pycontrails implementation of CoCiP makes the following implicit assumptions about the ice radius distribution:

- The fall speed is calculated using a parametric model from Section 3.1.3 in Spichtinger and Gierens (2009). This calculates a single fall speed for all crystals.
- For the optical depth, extinction is calculated based on a single radius (line 1020 in contrail_properties.py from the pycontrails CoCiP implementation).
- CoCiP attempts to compensate for the lack of gravitational sorting by enhancing its vertical diffusivity, but still does not explicitly represent the particle size distribution (Schumann, 2012). See Equation 25 of Schumann (2012) for further information.

- For the ice particle size, CoCiP tracks the ice mass and the number of ice crystals separately. The ice mass is calculated based on the following statement "The water mass MH2O in the segment is composed of contrail water in the ice phase and water in the vapor phase at ice-saturation" (Schumann, 2012). Therefore, the ice mass is the mass of the water vapor minus the mass of water vapor at saturation at each timestep (see equation 44 in Schumann, 2012).
- The only equation that makes an assumption about the particle size distribution (PSD) is the equation estimating ice number loss due to aggregation. Schumann (2012) says "Since we have no explicit information on the particle size spectrum, we assume that the size spectrum in the contrails has a width of order r ".

With this evidence, we can conclude that indeed the CoCiP model is monodisperse in most relevant cases. The only place in which a non-monodisperse PSD is assumed is in the ice number loss due to aggregation. We have added this exception to the manuscript to make it explicitly clear.

Spichtinger, P. and Gierens, K. M.: Modelling of cirrus clouds – Part 1a: Model description and validation, Atmos. Chem. Phys., 9, 685–706, https://doi.org/10.5194/acp-9-685-2009, 2009.

Author's changes in manuscript: Added the following in line 108 of the revised manuscript:

"The monodisperse assumption is used when calculating the ice particle size, fall speed, and optical depth of the Gaussian plume. However, ice crystal loss due to aggregation is also modelled in CoCiP (see Eq. 52 in Schumann, 2012), for which the width of the size spectrum is assumed to be of order r (Schumann, 2012). Although the crystal loss parameterizations implicitly assume a size distribution, all crystals are treated identically with a single radius value and no size distribution is diagnosed. As such, we still refer to CoCiP as a monodisperse model."

"Figure 5 left shows that dN/dt = -N/τ, that is the crystal loss rate is not constant. Wouldn't a monodisperse distribution of crystals, falling with identical speed, lead to a constant loss rate? Furthermore, if all crystals do what the crystal in the contrail centre does, why then is there ongoing crystal loss instead of instantaneous vanishing of all crystals?"

Author's response: This is true. Following the initial loss of ice crystals during vortex sinking, CoCiP assumes continuous loss of ice crystals through three mechanisms – parameterizing some of the effects of a non-monodisperse size distribution to estimate the effect on the two stored values (total ice mass and total crystal number):

- Losses due to internal plume turbulence (denoted as "turb" in Equation 49 from Schumann (2012)):

  $$\text{“}(\mathrm{dN/dt})_{\mathrm{turb}} = -\mathrm{E_T}\left(\frac{\mathrm{D_H}}{\max(\mathrm{B,D})^2} + \frac{\mathrm{D_V}}{\mathrm{D_{eff}^2}}\right)\mathrm{N}\text{”}$$

- Losses due to sedimentation-induced aggregation (denoted as "agg" in Equation 52 from Schumann (2012)):

“$(dN/dt)_{agg} = -E_A 8\pi r^2 V_T N^2 / A$”

- Losses by turbulent humidity fluctuations, mesoscale turbulence, and gravity waves (denoted as “meso” in Equation 55 from Schumann (2012))
  “$(dN/dt)_{meso} = -E_{meso} N_{BV} w'_{meso} (dT/dz)/\Delta T_c$”

The only other mechanism for loss of crystals is total evaporation of the contrail. Once subsaturated air begins to mix with the contrail, all crystals will give up ice to maintain 100% saturation – meaning that the ice mass (and therefore the effective radius) decreases uniformly. Once one of the end-of-life conditions is reached, all crystals are eliminated instantaneously. This can take a few time steps but is typically very rapid as CoCiP does not model the horizontal distribution of water vapor or crystals within the contrail air mass.

The equations for these losses depend on several time-varying contrail and ambient properties. Hence, even though CoCiP is a monodisperse model, the ice loss rate is not constant when the plume is in supersaturated air.

We have updated the manuscript to explain why the ice loss rate is not constant in an ISSR, and why contrail demise is not instantaneous when leaving the moist layer.

Author’s changes in manuscript: Added a new appendix - “Appendix H” containing the above explanation.

“In line 200 you write "average ice particle". If the distribution is monodisperse, all ice crystals are average.”

Author’s response: This is correct. We have modified the manuscript accordingly.

Author’s changes in manuscript: Replaced “with the fall speed of the average ice particle” with “due to its monodisperse ice radius distribution” in line 234 of the revised manuscript.

“Section 2.2, Eqs 1 and 2: Why is the spatial integral only over the width of a contrail and not along its length? This quantity as used here seems to have some similarity to the "total extinction" of Unterstrasser and Gierens and a similar quantity introduced by Lewellen. These authors use them as proxies for climate impact, perhaps an instantaneous one. But in the present case, I fear this could not serve the intended comparison. The total radiative effect of a contrail should be the lifetime integral of the vertical optical thickness at every point of the contrail (width X length). For the current purpose, length should somehow increase with lifetime, and it seems that this effect is overlooked. Wouldn't the differences between CoCiP and APCEMM be even larger if the integrals would cover the complete contrail area over the complete lifetime?”

Author's response: First, we agree that the "total extinction" metric is a more appropriate one and thank the reviewer for this suggestion. We have renamed our "lifetime optical depth" variable to instead be the "time-integrated total extinction" to maintain consistency across different works.

Second, we chose to only consider the evolution of the contrail in the 2D sense to ensure we capture the native model behavior for APCEMM and CoCiP, without any addons to represent the entire flight-wise span of the contrails. This is because, although the models can be programmed to axially stretch the contrail in a consistent way, this would not change the conclusions drawn regarding the behavior of the contrails in the absence of axial stretching. We believe that the "time-integrated total extinction" is an appropriate metric for this purpose. We acknowledge that the manuscript was originally unclear in this regard and have added a clarifying sentence in the methodology.

Author's changes in manuscript: Replaced "integrated optical depth" with "total extinction", and "lifetime optical depth" with "time-integrated total extinction" throughout the manuscript. Added clarifying statements in the methodology to further justify the chosen impact metric and scope of the manuscript: "and no effects in the flight direction are considered" (in line 152 of the revised manuscript) and "Given that we do not consider effects along the flight direction, the time-integrated total extinction accounts for persistence, lateral spread, and optical properties" (in line 167 of the revised manuscript).

"The title can be improved. It is not clear what "lifetime optical depth" may be. I think, the problem is not the optical depth, but the lifetime-integrated radiation effects or the change of the radiation energy flow integrated over the contrail lifetime.

When the same expression appears in the abstract, it should be written as "lifetime-integrated optical depth". As an explanation is given (proxy), the expression is acceptable, but in the title it should be changed. At the end of the abstract you explicitly write "contrail climate impact", why not so in the title?"

Author's response: We appreciate the comment and share the opinion that our original choice of language may not be optimal. In response to this, the authors had a meeting to discuss alternative titles, but we found that more specific language took away from the effectiveness and conciseness found in the original. For that reason, we have chosen to abstain from changing the title. We do recognize, however, that the terminology used in the abstract is mistaken, and have made the appropriate changes.

Author's changes in manuscript: Changed "contrail climate impact" in the abstract to "the time-integrated total extinction" (line 10 of the revised manuscript).

“Abstract, line 19: "a strategy avoiding all contrail formation is still expected to yield a reduction in climate impact". I believe such a strategy does not work for practical reasons (ATC problems) and does not exist therefore. I also do not believe that such a strategy has been proposed, as written in Line 25.”

Author’s response: .We agree that our original presentation was inaccurate given the point that we are trying to make. As stated in comment RC1-01, we believe that unbiased avoidance is more robust than strategies using contrail models to select the contrails to avoid, provided that the same proportion of flight length is avoided, irrespective of what that proportion is. We have modified the manuscript to clarify our point. Secondly, we appreciate the comment regarding the use of the word “proposed” in line 25. We agree that this word is misused and unnecessary. We have therefore chosen to remove it.

Author’s changes in manuscript: Removed the word “proposed” in line 25 of the original manuscript.

Replaced: “While a strategy avoiding all contrail formation is still expected to yield a reduction in climate impact, implementing optimized requires more research to establish confidence in model predictions”

with: “While a strategy avoiding a given proportion of persistent contrails in an unbiased way is still expected to yield a proportional reduction in the time-integrated total extinction, implementing strategies using contrail models to select the specific contrails to avoid may lead to fewer reductions in the time-integrated total extinction, primarily due to the current level of disagreement between models. We therefore recommend more research to establish confidence in model predictions at later contrail ages” in line 19 of the revised manuscript (part of the abstract).

“Line 29: why hypothetical? Why not as well in actually occurring situations?”

Author’s response: We agree with the point that prediction use cases for contrail models are not just limited to hypothetical scenarios. We have adjusted the manuscript accordingly and are grateful for this comment.

Author’s changes in manuscript: We replaced “rely on contrail models to accurately predict net contrail warming in hypothetical scenarios” with “rely on the availability of accurate contrail models” in line 32 of the revised manuscript.

“Line 44: cross sectional area of ~ 100 km^2, what kind of cross section do you mean? Say a contrail is 1 km deep, than it must be 100 km broad in your example.”

Author's response: We have added a clarifying statement, with reference to previous studies, to address this.

Author's changes in 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" in line 47 of the revised manuscript

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: "The limited comparisons that have already been performed for large eddy simulations indicate disagreement in this regard", can you be more specific? As far as I remember, the cited papers weren't model comparison papers. What do you mean?"

Author's response: We agree that the original sentence was vague and did not provide much insight. We have now modified the manuscript to include some points of agreement and disagreement between Lewellen (2014) and Unterstrasser and Gierens (2010a), and Unterstrasser and Gierens (2010b). According to Lewellen (2014), "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". In particular, both studies determined that the total extinction 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).

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 50: "inconsistencies"? Probably you simply mean model differences or disagreements or contradictory results. To my view, two different models, independently developed, can neither be consistent nor inconsistent."

Author's response: We agree with the comment and are grateful for it. We think that the best way to describe this is "differences between the behavior of the models"

Author's changes in manuscript: We replaced "inconsistencies between the models" with "differences between the behavior of the models" in line 67 of the revised manuscript.

"Lines 68-70: The two sentences "Teoh shows..." and "For this reason" are not logically connected, to my opinion. To only consider long-lasting contrails is justified without Teoh's results. Whether the latter turn out tenable can be doubted in view of your results, in particular the probably wrong sensitivity to layer supersaturation lets me doubt to which degree Teoh's results are believable."

Author's response: We are grateful for bringing this non-sequitur to our attention. To address this, we added a sentence that bridges the jump in logic. Regarding the validity of the study conducted by Teoh et al. (2024), we would like to emphasize that no one model is "right", or "better" than another since we recognize that there is no ground truth covering cases at such late lifetimes. We are therefore unable to comment on the validity of the results presented by Teoh et al. (2024).

Author's changes in manuscript: Added "This implies that the most warming contrails produce the majority of their climate impact in the diffusion regime" in line 89 of the revised manuscript.

"Line 109: "Equivalent"? Is there a subtle meaning that I do not understand or do you just mean "Equal"?"

Author's response: Although this comment raises a good question over our use of the word "Equivalent", it reveals some underlying ambiguity in the sentence. The meteorological scenarios are identical but the meteorological inputs are not exactly identical due to differences in the required input format for each model. To address this, we have clarified the entire sentence.

Author's changes in manuscript: We replaced the following sentence in line 135 of the revised manuscript: "Equivalent meteorological scenarios are then produced for each model, with each scenario described by six independent meteorological parameters (Section 2.1)" with "Meteorological inputs for each model are then produced for each simulation scenario, each of which described by six independent meteorological parameters (Section 2.1)."

"Line 216: correct "observable in from satellites"."

Author's response: This has been corrected.

Author's changes in manuscript: We replaced "observable in from" with "observable from" in line 251 of the revised manuscript.

"Figure 6: The figures are not entirely understandable. Partly, because they are just scatter plots and it is not clear which CoCiP point is paired to which APCEMM cross. Then, while CoCiP should indeed be represented by a single group of points for "fallstreak only", there should be 2 groups of crosses for APCEMM: "fallstreak only" and "all phases". I suggest, to have the integrated optical thickness on the y-axis, while the x-axis should be numbered 1-14, that is the number of the sensitivity experiment. Then each number would have one blue point for CoCip, and, say a red point and a red cross for APCEMM "fallstreak only" and "all phases". A similar outline would also work for the rhs panel."

Author's response: We extend our appreciation for the thoughtful suggestions made in this comment. Upon review, we have concluded that the caption and legend of this figure are not clear enough for most readers to interpret these properly. Nevertheless, we have decided to keep the original Figure 6 format while modifying the caption and the legend to clarify how the data should be interpreted. We believe that the parity plot format adds an additional layer of insight through the visualization of the agreement of the entire simulation suite. This capability is not present in many other visualization techniques. However, if this Figure is raised again as a source of confusion, we are happy to replace it with an alternative. Furthermore, we would like to inform Referee 1 that we have updated the terminology used to describe the sub-regimes. More details can be found in RC3-02.

Author's changes in manuscript: Added the following sentence to the caption of Figure 6(a): "Each entry in the parity plot corresponds to one simulation. In (a), the crosses indicate the simulations where the whole lifetime has been considered, whereas the dots indicate the simulations where only the unrestrained sub-regime has been considered."

"Figure 6, caption: correct "unobserbavle"."

Author's response: Fixed.

Author's changes in manuscript: Changed "unobserbavle" to "unobservable" in the caption for Figure 6.

"Line 300: "Varying the layer RHi causes the lifetime optical depth to decrease in CoCiP and increase in APCEMM". This sentence is a bit unclear, since the word "varying" includes both decreasing and increasing. Please correct."

Author's response: We are grateful for highlighting this ambiguity. We have now resolved it.

Author's changes in manuscript: Replaced the word "Varying" with "Increasing" in line 367 of the revised manuscript.

"Line 345/6: The Schmidt-Appleman criterion says nothing on contrail persistence, so I suggest to add ice supersaturation as a condition. Avoidance of all, that is, including very short contrails, is not useful and probably worsening the climate (unnecessary fuel consumption and emissions)."

Author's response: This comment highlights an oversight in the manuscript: this sentence should have considered persistent contrails only. We have corrected the sentence.

Author's changes in manuscript: Specified that avoiding all persistent contrails can be done by predicting the Schmidt-Appleman criterion and ice supersaturation in lines 416-418, as well as in line 31 of the revised manuscript.

"Line 385: "our results suggest that contrail avoidance strategies which focus on avoidance of all contrails will have the greatest chance of producing a real climate benefit". I would not subscribe to this conclusion. It renders contrail avoidance practically

impossible, in particular for ATC reasons. The ATC sectors where no contrails form would become overcrowded if they are neighbours to sectors where contrails can form. I think, the appropriate conclusion of your test is that more work is needed to "calibrate" the models to realistic behaviour and to test whether the promised results are satisfying. Your Section 5 points to this direction and I fully agree to the statements of Sect. 5."

Author's response: This was a miscommunication – we are not proposing avoiding all persistent contrails. Instead, we propose avoiding contrails over a certain flight proportion in an unbiased fashion, as opposed to model-informed contrail avoidance. Please refer to our response to comment RC1-01 for more details.

Author's changes in manuscript:

Replaced "our results suggest that contrail avoidance strategies which focus on avoidance of all contrails will have the greatest chance of producing a real climate benefit" with "our results suggest that unbiased contrail avoidance strategies at any scale will have the greatest chance of producing a real climate benefit" in line 461 of the revised manuscript.