We appreciate the time and effort that Reviewer 2 has taken to review the manuscript and we thank them for their useful comments and suggestions on how to improve the paper. We believe we have now addressed their comments and made the revisions described in our point-by-point responses below.

**General Comments:**

**The aim of this manuscript is to address the differences in radiative forcing from contrail cirrus due to the use of different host models. The same contrail cirrus parameterization is applied to both models (CAM and UM).**

**However, the two models differ significantly in some key parameters (as shown in the table), so it is unsurprising that the results also vary greatly. As a result, it's unclear what conclusions can be drawn from this comparison.**

**This raises the question of whether the title should instead be: "Impact of the microphysical scheme on contrail cirrus effective radiative forcing estimates." Alternatively, the differences in radiative forcing could be investigated by focusing on aspects such as horizontal or vertical resolution, model time step, or even different nudging datasets. After reading the manuscript, it's unclear to me why two climate models were used. I believe the study could have been effectively conducted using a single model with different microphysical schemes.**

**This leads me to question what can be learned from these results. Would it not be more useful to focus initially on a single host climate model, examining the impact of factors such as microphysics or spatial resolution? Could the spatial and temporal dimensions of the models not at least be harmonized? Doing so would make it easier to assess the differences between the two host models.**

We believe our study brings two key contributions:

- The first contrail cirrus scheme in the UK Met Office Unified Model (UM): This work marks the first implementation of a contrail cirrus parameterisation in the UM, making the UM the third global climate model (GCM) capable of simulating the climate impacts of contrail cirrus. This addition helps to address a current limitation in contrail cirrus effective rafiative forving (ERF) assessments, which are presently constrained by the small number of GCMs—currently only ECHAM and Community Atmosphere Model (CAM). While the UM is not yet able to provide a fully independent contrail cirrus ERF estimate, it has the potential to achieve this with the future adoption of a two-moment cloud scheme.
- A comparison of contrail cirrus simulations with two widely used climate models: Despite various sources of uncertainty in contrail cirrus ERF estimates, the uncertainty stemming from the use of different host GCMs has been highlighted in the latest IPCC report. Our study reveals significant differences in contrail simulations between different host GCMs, offering new insights into this area of uncertainty.

This has now been clarified in lines 105-111 of the revised manuscript:

"In this study, we perform the first comparison of a contrail cirrus scheme across two global climate models (GCMs), each in its respective standard configuration. The main aim is to investigate the impact of key host climate model characteristics on contrail cirrus simulations by adapting the Chen et al. (2012) contrail cirrus CAM parameterisation for the UK Met Office Unified Model (UM) (Sellar et al., 2019). By using the same contrail parameterisation in two different host climate models, we are able to directly compare contrail cirrus estimates, therefore contributing to improving the understanding of main sources of uncertainty in simulated contrail cirrus microphysical and optical properties, as well as the associated natural cloud responses.”

However, we do not attempt to provide a comprehensive analysis of the sources of uncertainty in GCM contrail cirrus ERF estimates. Previous work has investigated the role of the GCM’s microphysics scheme (Bock and Burkhardt, 2016) or the model resolution (Chen and Gettelman, 2013; Chen et al., 2012) on contrail cirrus ERF. Our study provides the first comparison of the contrail cirrus scheme in two GCMs, each in their respective standard configurations. Similarly to investigations of the impact of other atmospheric agents (e.g. aerosols (Ratcliffe et al., 2024; Henry et al., 2023) and ozone (Brown et al., 2024; Son et al., 2018; Brown-Steiner et al., 2015)), these standard configurations are likely to be used in future assessments of contrail cirrus ERF. For spatial resolution, the UM configuration is consistent with its CMIP6 setup (Sellar et al., 2019), while CAM6 maintains the same horizontal resolution as its CMIP6 version (Danabasoglu et al., 2020) with an adjusted vertical resolution in CAM6 Specified Dynamic compset (nudging) to be aligned with the MERRA2 meteorology vertical layers. The nudging and time step settings are also the models' default configurations.

While we do not aim to provide a comprehensive analysis of the role of different configurations, in the revised version of the manuscript we further clarify the discussion of these effects at lines 221-229 as follows:

“The models used in this study are configured in their respective standard setups, which are expected to be employed in future assessments of contrail cirrus simulations, similar to evaluations conducted for other atmospheric agents. The spatial resolution of the UM follows its CMIP6 setup (Sellar et al., 2019), while CAM6 maintains the same horizontal resolution as its CMIP6 version (Danabasoglu et al., 2020), with an adjusted vertical resolution in the Specified Dynamics (nudging) configuration to align with MERRA-2 meteorology vertical layers. The nudging and time step settings used here reflect the default model configurations. Previous studies have quantified the impact of different configurations within a GCM, such as the impact of the microphysics scheme (Bock and Burkhardt, 2016) or the model resolution (Chen et al., 2012; Chen and Gettelman, 2013) on contrail cirrus ERF estimates. The model configurations of the UM and CAM used in this study are described in detail below.”

**Specific Comments:**

- **Section 2: Since ice supersaturation is a key prerequisite for contrail cirrus formation, it would be helpful to include a more detailed description of how ice supersaturation is treated in the host climate models. For example, is ice supersaturation allowed within clouds? What about saturation adjustment?**

Thank you for this point. We now add a more detailed description of ice supersaturation treatment within the models at lines 131-136 in the revised manuscript:

“In-cloud supersaturation is permitted by the model and is diagnosed by the parametrization described in Furtado and Field (2017). The parametrisation assumes that the ice cloud fraction in each gridbox is partitioned into supersaturated and sub-saturated sub-areas. The areas and RH of these regions are parameterised in terms of grid-box mean quantities from an assumed sub-grid RH distribution. Additional complexities are introduced to handle mixed-phase and super-cool-liquid-only areas. In this scheme, there is no requirement that grid-scale RH over ice must be zero – i.e., depositional growth of ice is handled prognostically, without assuming instantaneous saturation-adjustment.”

At lines 147-151:

"Ice supersaturation is allowed as described by Gettelman et al. (2010) and Gettelman et al. (2015). Saturation adjustment and condensation is performed based on the vapour pressure over liquid. Ice formation occurs only when nucleation conditions are satisfied based on the available ambient aerosols and the ice nucleation scheme of Liu et al (2005). Once ice is formed, a vapour deposition process occurs onto ice as described by Gettelman et al. (2010), and contrails uptake water in the same manner."

- **L179: You point out the important differences in microphysical schemes here, but you also discuss differences in contrail representation. This is confusing, as both models use the same contrail parameterization. Please clarify.**

Sorry for the confusion and thank you for pointing this out. While both models use the same contrail parameterisation, the difference lies in how contrail ice number concentration is handled when added to natural clouds. In the UM, which uses a one-moment cloud microphysics scheme, the contrail ice number concentration is not explicitly specified when added to natural clouds.

We have now rephrased the sentence for clarity at lines 199-200 as follows:

"Contrail ice number concentration is treated differently when added to natural clouds due to the different cloud microphysics schemes in the two models."

- **L194ff: The cross-sectional area of the initial volume is set to 100m x 100m for both models, despite their significantly different horizontal resolutions. How does this influence the results?**

The difference in horizontal resolution does not have a big impact on the young contrail results, as long as the background meteorology conditions stay similar. This can be explained as follows:

Here is the Eq (4) in the manuscript for the parameterisation of contrail ice mass mixing ratio:

$$M = q_t \Delta_t + \frac{d \cdot C}{V}\left(x - x_{sat}^{i}\right), \tag{4}$$

The term $d \cdot C$ in Eq (4) represents the initial contrail volume:

contrail volume (m$^3$) = contrail cross section C (m$^2$) × distance flown d (m)

Contrail cross section is only related to the contrail age and distance flown is aggregated within grid boxes. So when the grid box's horizontal size changes, while the contrail cross-section size remains the same, the distance flown within the grid box would change accordingly.

Assuming the background meteorological conditions remain similar with changes in horizontal resolution, the $\frac{d \cdot C}{V}$ term in Eq (4), the ratio of contrail volume to grid box volume, would not change very much, and therefore the contrail mass mixing ratio and fraction would not be affected much.

This has now been clarified at lines 216-219 as follows:

"We note that using the same cross-sectional area across different spatial resolutions of the two models is expected to have only a negligible effect on young contrail properties. This is because the total contrail volume in a grid box depends not just on the cross-sectional area but also on the grid box aggregated distance flown, which ensures consistency across varying spatial resolutions."

- **L210ff: Could you comment on or estimate the expected differences due to the use of different nudging datasets? Why are two different datasets used in the first place?**

In our simulations we only nudge the wind fields, with UM winds nudged to ERA5 reanalysis and CAM winds nudged to MERRA2 reanalysis. To acknowledge the potential effects of using different datasets for wind nudging, we now add the following in the manuscript at lines 248-250:

"We note that contrail spreading would be affected by the model wind fields, which in our UM and CAM simulations are nudged to ERA5 and MERRA2 reanalysis, respectively. Therefore, differences in the wind fields between these two reanalyses will contribute to variations in the simulated contrail spreading across the two models."

The nudging datasets of the two models are different as they are linked to the standard configurations of the two models, which use ERA5 for the UM and MERRA2 for the CAM simulations as noted in Sect. 2.3. We believe that using these different nudging datasets also allows us to capture the existing uncertainties in contrail cirrus spreading. Providing a comprehensive study of the uncertainty of GCM-simulated contrail cirrus by using the same nudging dataset is beyond the scope of our study.

- **Section 3.1: You mention good agreement between UM and CAM ice supersaturation versus observations, but Figure 1 shows clear differences in the annual zonal mean. Could you comment on this? Which model aligns more closely with observations? Over what time period is the annual zonal mean calculated—one year or more?**

Thank you for pointing this out. Our objective was to highlight the differences in ice supersaturation frequency between the two host climate models, which help explain the significant differences in young contrail fraction and ice water mass. Evaluations of ice supersaturation and humidity in both models have been performed in other previous studies:

"The ice supersaturation generated by the host climate model is key for determining both the microphysical properties and lifetime of the simulated contrail cirrus. Previous evaluation studies show good agreement between simulated UM and CAM ice supersaturation and observations (Chen et al., 2012; Irvine and Shine, 2015). The models' humidity has also been validated against observations and intercompared with other CMIP5 climate models (Jiang et al., 2012)."

We agree it is important to expand a bit the discussion on the differences in ice supersaturation between the two models. We have now updated Fig. 1 in the manuscript to also include a comparison with the ice supersaturation frequency in ERA5 (see updated Fig. 1 below). The ice supersaturation frequency in both models has been calculated on a 1-hour basis to ensure consistency with ERA5 time frequency. We have now also added the following text at lines 265-270:

"Both the UM and CAM capture the general pattern of ice supersaturation found in ERA5 (Figure 1c). However, there are notable overestimations of ice supersaturation across much of the UTLS in both models compared to ERA5 which is known to have a dry bias in the UTLS (Kunz et al., 2014; Wolf et al., 2023). In high-latitude regions below the tropopause, the UM and CAM show ice supersaturation frequencies up to 50% higher than those in ERA5. In the tropical tropopause layer, CAM simulates ice supersaturation frequencies closer to ERA5, while the UM still exhibit higher supersaturation frequencies."

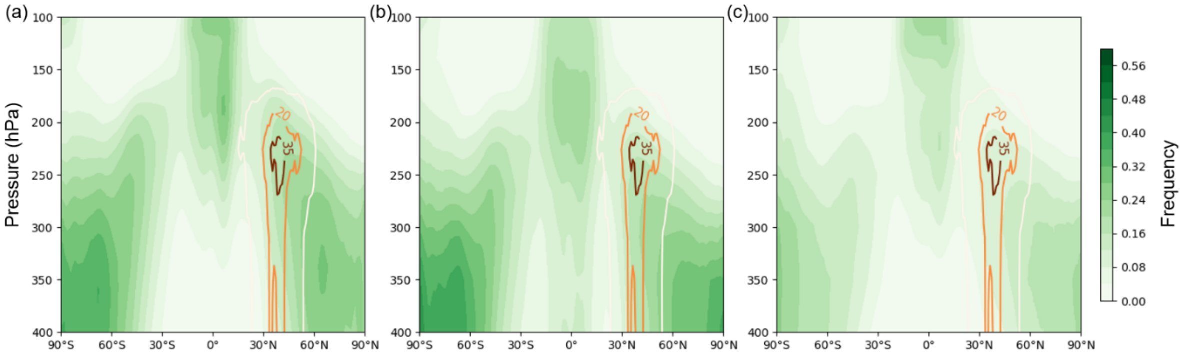

**Figure 1. Annual zonal mean frequency of background ice supersaturation simulated by the (a) UM, (b) CAM, and (c) ERA5 for the full year of 2006. The ice supersaturation frequency is calculated on a 1-hour basis. The coloured contour lines represent the annual zonal means of the flight distance (in m/s) in the AEDT air traffic inventory.**

The annual zonal mean is calculated for the full year of 2006 – this is now specified in the caption of Fig. 1.

**• L244ff: Why was 2006 chosen for the seasonal cycle, and how representative is that year? Which December (2005 or 2006) was used?**

The choice of the year 2006 for the seasonal cycle was made for consistence with the the air traffic inventory year. We agree that the choice of a particular year will always introduce some uncertainty and we try to account for that by performing perturbed ensembles to account for variations in the background meteorology. We then apply a Student's t-test to indicate statistically significant results under different meteorological conditions, as shown in the figures in Sects. 3.3, 3.4, and 3.5.

December of 2006 was used – all months are in 2006. We now specify this in the caption of Fig. 2 as:

"Panels (d)-(g) show the seasonal mean of the ice supersaturation difference between the UM and CAM for 2006 in December–February (DJF), March–May (MAM), June-August (JJA), and September–November (SON), respectively."

**• Section 3.2: The young contrail is defined as the contrail in the first time step of its life cycle (L258). What differences can be expected if one model has a time step of 20 minutes and the other 30 minutes? Later, it's mentioned that CAM values are normalized by multiplying by 2/3. What if CAM shows no contrails after 30 minutes, but they would still be present after 20 minutes?**

Yes – thank you, this is an important point.

The distance flown d in Eq (4) is calculated as:

Distance flown of the time step = distance flown tendency from the air traffic inventory (m/s) × one model time step length (20min or 30 mins)

Thus, the distance flown calculated from a 30 mins time step will be larger than that from the 20 mins model time step by a factor of 1.5. This is now mentioned in the revised manuscript at lines 335-337:

"As shown in Eq. (4), the young contrail fraction and ice water mass mixing ratio are proportional to the time step length. To ensure comparability of the young contrail quantities, the CAM values were normalised to 30 minutes by a factor of 2/3 to account for the different model time step lengths (i.e. 20 minutes in the UM and 30 minutes in CAM)."

We agree that this normalisation will not account for situations where CAM would show no contrail after 30 minutes, despite potentially being present after 20 minutes. This will contribute to the difference in simulated young contrail fraction, however will have a negligible effect on the contrail climate impact - a contrail lifetime of 20, 30, or 40 minutes will have virtually no effect on the climate impact of that contrail. To acknowledge this potential contribution to the simulated young contrail fraction, we now add the following in the manuscript at lines 339-340:

"This normalisation may slightly underestimate CAM values, as it does not account for contrails lasting between 20 and 30 minutes period."

- **L283: What do you mean by "high temperature in the Northern Hemisphere"? Are you referring to temperature in the UTLS region?**

Yes, we now clarify this in the manuscript at lines 352-354:

"The minima of contrail cover fraction in boreal summer primarily result from the larger upper tropospheric temperatures in the main regions of intense air traffic (i.e. Northern Hemisphere), which inhibits the formation of contrails."

- **L289: The phrase "This may be due to…" sounds speculative. Can this be substantiated?**

Thank you for pointing this out - we agree it is important to provide more clarity here. We have now reformulated the text in the revised manuscript at lines 357-359 as follows:

"During the boreal summer, the global means of the young contrail cover fraction simulated in the two models are more similar due to the lower UM ice supersaturation over some of the regions with intense air traffic (e.g., Europe and the USA)."

- **L297ff: Differences in CAM results are discussed, showing a factor of 10 when different vertical resolutions are used. How useful is it to compare CAM and UM, which have significantly different vertical resolutions?**

We agree it is important to clarify this. According to the Table 2 from Chen et al. (2012) (also shown below), the global mean young contrail fraction increases with increased vertical resolution in the UTLS. Therefore, the finer vertical resolution (500 m in the UTLS) in the UM likely contributes to a higher contrail fraction compared to CAM (1000m in the UTLS).

| Model level | Vertical resolution in the UTLS | Young contrail fraction global mean |
|---|---|---|
| L30 | ~30hPa/1000m | 6.10E-07 |
| L40 | ~10hPa/300m | 1.73E-06 |
| L54 | ~5hPa/160m | 3.36E-06 |
| L82 | ~2.5hPa/80m | 6.63E-06 |

We now clarify this in the manuscript at lines 375-376:

"In addition, the finer UM vertical resolution in the UTLS also contributes to a larger young contrail cover fraction (Chen et al., 2012)."

- **L308f: What is the temporal and spatial resolution of ECHAM5 in this case?**

The ECHAM5-CCMod at T42L41 resolution has a grid spacing of 2.8° x 2.8° in latitude and longitude, with 41 vertical layers (500 m vertical resolution in the UTLS) (Kurz, 2007), and a time step of 15 minutes. These are different to those of the UM and CAM and they will have a slight contribution to differences in simulated contrail fractions. However, the main contributor to this difference is the overwhelmingly different definition of young contrail fraction used in ECHAM, compared to UM or CAM. As noted in Sect. 3.2, the contrail fraction in ECHAM corresponds to contrail cirrus with an optical depth threshold of at least 0.05, which also includes contrails older than one model time step.

**• Section 3.4: The description of the scaling factor is brief. Could you explain how you arrived at the value of 4900? Is this value representative for regions with less traffic?**

To obtain the most suitable scaling factor, we used a trial-and-error method based on several simulations spanning scaling factors between 1000 and 20000. The 4900 scaling factor was derived based on the simulated optical depth over the European region, where the contrail signal is relatively stronger (compared to other regions) due to the higher air traffic. For regions with less traffic, where the contrail signal is much weaker, it is very difficult to obtain statistically significant results, even when employing a large number of ensemble simulations. This does indeed mean that this scaling factor is not necessarily representative for regions with less traffic.

To acknowledge this, the revised version of the manuscript states at lines 437-441:

"To obtain the most suitable scaling factor for each optical depth reference value, we used a trial-and-error method based on several simulations spanning scaling factors between 1000 and 20000. We used the European region (35°N – 60°N latitude and 10°W – 25°E longitude) as benchmark due to its large air traffic and therefore larger statistical significance. As a result, the scaling factors may not be representative for areas with lower air traffic density, where obtaining statistically significant results is more challenging."

**• Figure 6 Caption: What are the "annual mean simulated contrail driven changes" compared to?**

Thank you for pointing to this source of confusion. The 'annual mean simulated contrail-driven changes' refer to the average annual changes caused by contrails, calculated as the difference between simulations with contrails and those without contrails. We now clarify this in the caption of Fig. 6 as follows:

"These values are calculated as the difference between simulations with contrails and those without contrails."

**• L393: The phrase "is likely due to…" sounds speculative. Can this be substantiated?**

We agree and we have now modified the text in the revised manuscript at lines 473-475 as follows:

"In addition to the scaling of young contrails mass in the UM, the factor of 2 difference between these values is also due to the different radiative transfer schemes and different cloud microphysical process rates in the UM and CAM."

**• Summary (L416): You mention the use of the same contrail scheme in two different host climate models. However, disentangling the differences due to one- and two-moment microphysics is challenging enough. Including different climate models with varying resolutions seems to skip a necessary step (as mentioned above).**

We believe that by using the same contrail scheme (i.e. a scheme where contrails are diagnosed within a single model timestep and then passed to the model cloud scheme) in the standard configurations of two widely used climate models is an important contribution. The fact that these standard configurations are different (e.g. spatial and temporal resolutions, nudging) and their consequences are acknowledged in our discussion and is in itself an important consideration of how future assessments of contrail cirrus climate impacts need to be interpreted. We believe that our paper highlights several key differences in the contrail simulations between the host models, including young contrail estimates, cloud fraction changes, and contrail cirrus ERF. The very small contrail cirrus ERF in the UM, despite its larger young contrail fraction and ice mass compared to CAM, is primarily due to differences in cloud microphysics, specifically the treatment of contrail ice number concentration in the two host models. In the UM, contrails are added to natural clouds with microphysical properties similar to those of natural clouds. Given that young contrail ice particles are much smaller than natural cloud particles, this approach is not ideal. As a result, contrails in the UM form with a lower number concentration and larger particle size than in CAM, which likely leads to increased sedimentation rates and shorter lifetimes. This accounts for the negligible contrail cirrus ERF in the UM. Additionally, variations in background meteorology and resolution affect young contrail estimates, while opposing cloud fraction changes are attributed to differences in cloud microphysics between the models.

This has now been clarified in the manuscript at lines 524-526 as follows:

"Another source of uncertainty arises from the differences in configurations (e.g. spatial and temporal resolutions, nudging) between the UM and CAM. In this study, both these widely used climate models are employed in their standard configurations, which are also likely to be used in future contrail studies."

**• L429ff and L373ff: You write that the contrail cirrus is misrepresented in UM for understandable reasons, but it should be shown more clearly that CAM provides a more realistic representation, especially since UM's optical depth is matched to CAM's values**

We do not think that we can make this point. Here we cannot provide an entirely independent UM estimate for contrail cirrus ERF due to the current shorcomings of the UM cloud scheme. However, by reporting the UM estimates for contrail cirrus ERF when matching different simulated optical depths, we provide an additional insight into how model characteristics contribute to contrail cirrus ERF uncertainty. We compare the UM simulated contrail cirrus optical depth with both CAM and ECHAM (as the other GCMs that simulate contrails) to better reveal this existing uncertainty range. The differences in optical depth across these models stem from both the physics and contrail parameterisations of host models. Both UM and CAM implement the same contrail parameterization (Chen et al., 2012), whereas ECHAM follows a different approach (Burkhardt and Kärcher, 2009). There is no clear benchmark contrail cirrus ERF to definitively determine which model provides the most accurate contrail ERF estimates, with large uncertainties remaining, as noted in Lee et al. (2021).

**• L443 (Future Work): It appears that microphysics is recognized as the greatest uncertainty, and improving UM with a new two-moment microphysics scheme is suggested. If microphysics is indeed the primary factor driving differences, the title of the manuscript should reflect this focus, perhaps as "Impact of Microphysics on Contrail Cirrus Radiative Forcing."**

As we mentioned in our general comment response, we believe that our paper highlights several important differences in contrail simulations between two host climate models – which in itself is something that has not been done before. While the microphysics scheme is indeed a very important one, there are other important findings that are presented and discussed in this study, such as the differences in young contrail estimates, the model cloud fraction response due to contrails, and contrail cirrus ERF. The small contrail cirrus ERF in the UM, driven by differences in cloud microphysics, represents only one aspect of the study. We therefore believe that the current title is an accurate representation of our paper and its scientific contribution.

**Typos, Format, etc.:**

Thank you for the detailed comments regarding the typos, formatting, and other corrections. These have been addressed in the revised manuscript.

**L150ff: In LaTeX math mode, use \textrm for text and \unit{} for units. Ensure consistent typesetting and spacing between different units.**

This has now been addressed in the revised version. Just to clarify that the "spacings" before the commas in Line 167 are due to the document's justification format, and there are no actual spacings before the commas.

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