We appreciate the time and effort that Reviewer 1 has taken to review our manuscript and we thank them for their useful comments and suggestions on improving the paper. We have now addressed all the comments and made the necessary revisions to the manuscript. Please see our point-by-point responses below.

General comments

In this study by Zhang et al., the authors analyse the impact of the choice of host climate model on the simulation of contrails, their feedbacks to natural clouds and their ERF. They achieve this by running the same contrail parameterisation in two different climate models, the UK Met Office Unified Model (UM) and the NCAR Community Atmosphere Model (CAM). A main difference between the two models which is highly relevant for contrail modelling consists in the complexity of their microphysics schemes, where the UM runs a single-ice-category one-moment cloud scheme and CAM features a two-moment scheme. Overall, the authors find large dependencies of the simulated contrails and their feedbacks on the chosen host climate model with sometimes even opposite signs. In order to evaluate the response of the ERF to the choice of climate model, the simulated optical depth of young contrails in the UM is scaled up linearly to be comparable to the one simulated by CAM. This analysis also reveals a large dependency on both, the chosen host climate model and the contrail optical depth. It is concluded that the uncertainties in modelling the contrail ERF are still large.

To my knowledge, this study is the first to investigate the impact of the host climate model on contrail ERF estimates, which is an important step towards understanding and narrowing down the large uncertainty estimated by Lee et al. (2021). This fits into the scope of ACP and is of urgent scientific interest, in particular in view of plans and ongoing attempts to avoid persistent contrails. Thus, the manuscript is suitable for publication in ACP.

However, some questions, which should be addressed in the manuscript, came to my mind, while reading. Also, I have a number of comments and suggestions for improving the manuscript further and recommend that the manuscript should be revised accordingly, before it can be published.

We thank the reviewer for their positive general comments on our manuscript.

Specific comments

- Line 28f: Unclear what is meant with "When accounting for the difference in cloud microphysics complexity". Suggestion: "When compensating the resulting unrealistically low contrail optical thickness in the UM"

Thank you for pointing this out, we agree this sentence needs clarifying. We have now changed the text at lines 28-29 to:

"When compensating the unrealistically low contrail optical depth simulated in the UM, we estimate…"

- Line 31: The "factor of ~2" better matches the values for 2006. I suggest to either give the 2006 values in the sentence before or omit this parenthesis.

As suggested, we have now removed this parenthesis in the revised manuscript.

- Introduction: The authors write a lot about uncertainties in contrail cirrus RF and ERF. However, I missed a sentence on recent research suggesting a low efficacy of contrail cirrus ERF, potentially resulting in a low temperature response despite the high ERF, due to compensating slow feedbacks (e.g. Bickel 2023).

We agree that the contrail cirrus climate efficacy is extremely important. We have now added it at lines 61-64:

"Contrail cirrus can also have an impact on natural clouds as their presence changes the water budget of the surrounding atmosphere. This may partially offset the direct climate impact of contrail cirrus (Burkhardt and Kärcher, 2011) and therefore reduce the contrail cirrus climate efficacy (Bickel, 2023)."

- Line 51f: This sentence seems a little out of context to me. If the intention is to state that also engines that do not emit soot can produce contrails, I suggest: "At present, the water vapour primarily condenses on particles emitted by todays kerosene combusting engines. However, these particle emissions are not necessary […]"

We agree this seemed a bit out of context and we thank the reviewer for their suggestion. We have now modified this in the manuscript at lines 51-53 as follows:

"At present, water vapour primarily condenses on particles emitted by today's kerosene combusting engines. However, these emitted particles are not necessary at the contrail formation stage as particles from the ambient air could be entrained into the exhaust plume and act as condensation nuclei."

- Introduction: I missed the usual paragraph on the structure of the paper.

We have now included a paper structure paragraph at lines 112-115 as follows:

"This paper is organized as follows: Section 2 provides descriptions of the UM and CAM models, the contrail parameterization, and the model setups used for the contrail simulations. Section 3 presents and analyses the simulated differences in ice supersaturation frequency, young contrail properties, cloud and radiation responses, and ERF estimates between the two climate models. The summary and conclusions are provided in Sect. 4."

- Equations 4 & 5: If I understand this correctly, specific quantities and mass mixing ratios are mixed here. The error is certainly negligible. If this is already mentioned in Chen et al. (2012), it is probably not necessary to repeat it here. However, please be aware of this imprecision.

Thank you for pointing this out. We agree that the distinction between specific humidity and mass mixing ratio is important, though the error is negligible in this context. We have now added the definitions of both specific humidity and mass mixing ratio in the revised manuscript for clarity at lines 183-185:

"where q_t is the aviation water vapour emission mixing ratio (ratio of the mass of aircraft water vapour emission to the mass of dry air) tendency in $\mathrm{kg\ kg^{-1}\ s^{-1}}$, ..., x is the ambient specific humidity (ratio of the mass of aviation water vapour emission to the total mass of air) in $\mathrm{kg\ kg^{-1}}$, ..."

- Line 195: Why "100 m x 100 m"? I would assume "contrails aged for 20–30 min" to be larger in cross section. Are the choices of particle radius and contrail cross section consistent with the model time step?

We acknowledge that the simulated contrails are sensitive to the choice of initial contrail parameters, as previously explored by Chen et al. (2012). In this study, the selection of the 100 m x 100 m initial young contrail cross section and 3.75 μm initial contrail ice particle radius was made for consistency with the recent CAM6 contrail study by Gettelman et al. (2021), as noted in Sect. 2.2. Although contrails may spread to a 300 m × 300 m cross section, the 100 m × 100 m assumption refers to the area of water vapour uptake. Gettelman et al. (2021) found that assuming a larger 300 m × 300 m area was too extensive for water mass uptake, as it would imply contrails absorb all the water in that volume (A. Gettelman, personal communication). Hence, the smaller 100 m × 100 m area was used in this study. The choice of the initial particle radius is aligned with observations of contrails aged 20–30 minutes (Schröder et al., 2000; Schumann et al., 2017) and previous CAM contrail studies (Gettelman et al., 2021; Lee et al., 2021).

This has been clarified in the revised manuscript at lines 212-216:

"The initialised ice particles within contrails in CAM are assumed to be spherical and have a radius of 3.75 μm based on contrails aged for 20–30 min (Schröder et al., 2000; Schumann et al., 2017). In the UM, given its one-moment cloud scheme, the same PSD has to be specified for both contrail ice and natural cloud ice. The cross-sectional area C of the initial volume of contrails is assumed to be 100 m × 100 m for both CAM and UM simulations, similarly to Gettelman et al (2021), the most recent CAM contrail study."

- Line 204ff: What were the reasons for the different treatment of perturbations, run time and nudging between the two models?

These choices were driven by the differences between the standard configurations of the two models, which are therefore the likely configurations to be used in future contrail simulations with the two models. Below is a summary of the reasons for these differences. We have added this discussion in Section 2.3):

- Run time: While CAM was run for 1 year, starting from 1 January 2006, since the UM standard AMIP runs start from 1 September, the UM simulation was run for 1 year and 4 months (1 September 2005 – 31 December 2006), with the first 4 months discarded as the spin-up period.
- Nudging: The nudging schemes in the UM and CAM were developed along different pathways. The use of background meteorology reanalysis differs, as the models were originally configured with different nudging reanalysis dataset. Also, the relaxation times are different , reflecting the recommended practices for each model. For the UM this consists in using the same time as the reanalysis data to maintain a stable background meteorology, while for CAM this consists in using 24 hours to allow for a similar cloud climatology as the free running CAM.

Also, I suppose nudging the temperature, but not the humidity might have strong impacts on relative humidity. Please discuss.

We thank the reviewer for highlighting this point as this was a mistake in the description of the methodology in the original manuscript. Neither temperature or humidity is actually nudged in the runs, therefore allowing the hydrologic cycle, including contrail and ice cloud formation processes, to operate freely. This has now been corrected in the revised version at lines 242-243 as follows:

"To allow both models to capture the relatively small contrail perturbations (compared to the model internal variability in clouds and radiation) and to enhance the signal-to-noise ratio, the u and v wind fields were nudged to a prescribed climatology, …"

As for the effect of nudging temperature on humidity, this is expected to be minimal as demonstrated in Sect. 3.3 of Gettelman et al. (2021).

- Line 246: I do not see a clear signal for East Asia in JJA, especially since it is claimed in section 3.2 that East Asia may compensate the lower ice supersaturation frequency in Europe and the USA in the UM in JJA.

Thank you for pointing this out. We agree the explanation provided was probably misleading. We have now changed this in the revised manuscript at lines 357-359 to clarify as follows:

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

- Figure 2: I wonder why the difference plots show dense air traffic over the North Atlantic, while the single-model plots do not. Is this an interpolation artefact?

Thank you for pointing this out, it was due to an error in the code used to draw the contour lines of air traffic density in the plot. We have now corrected this in Figure 2 in the revised version.

- Line 267: I cannot reproduce the 0.00018% for CAM from figure 3e. Is this a non-normalised value?

Thank you. We are sorry for the confusion – this mistake has now been corrected in the revised manuscript. The correct value of young contrail fraction global mean is 0.00012% after normalisation. This has been updated in Table 1 in the revised manuscript.

In addition, to further clarify and avoid confusions, we also now mention the unnormalized value of 0.00018% at Lines 369-373:

"To account for this dependence, we compare the annual global mean young contrail cover fraction of 0.00018% (unnormalized, since the time steps for both CAM5 and CAM6 are identical) from our CAM6 simulations with 56 levels overall and ~1000 m vertical level thickness in the UTLS with the corresponding value of 0.000061% estimated in CAM5 with 30 vertical levels in total but similar vertical interval in the UTLS (Chen et al., 2012)."

- Line 269ff: Was this normalisation performed before or after adding up the model levels under the random overlap assumption? In the latter case, I would (in theory) expect the reduction to be too strong.

The normalisation was applied after the random overlap calculation. We compared the results of applying the normalization factor both before and after the random overlap calculation, and only found a negligible difference (i.e. 0.0001210% compared to 0.0001208%). This has now been clarified at lines 337-339 as follows:

"The normalisation was applied after the random overlap in our study, and we found that the sequence of normalisation and random overlap only had a negligible effect on the young contrail cover fraction."

- Line 271f: This sounds quite certain that this is the only reason. Have you checked, whether there are also differences in the frequency of contrail generation in general (SchmidtAppleman criterion) that could also contribute to more persistent contrails?

Thank you for this suggestion. We have now performed additional analysis to investigate this further by comparing the frequency of satisfying the Schmidt-Appleman criterion in the two models. This has now been included in the manuscript as Fig. 3 and at lines 305-314.

“The satisfaction frequencies of the Schmidt-Appleman criterion in the two models, averaged between the 200 hPa and 300 hPa pressure levels, are illustrated in Fig. 3. The overall distribution patterns between the two models are similar (Fig. 3a and b), with both showing relatively high frequencies in mid- and high-latitudes. However, the UM has a higher frequency in some regions with intense air traffic (e.g. Europe, East Asia, North Atlantic), indicating a greater likelihood of contrail formation (Fig. 3c). Combined with its generally higher ice supersaturation frequency, this increases the probability of young contrail formation and persistence in the UM. There is also seasonal variation in the differences in Schmidt-Appleman criterion satisfaction between the two models (Fig. 3d, e, f, and g). Over East Asia and most of Europe, the UM generally shows higher frequency during all seasons, except in Western Europe during June-July-August and September-October-November. Over the continental USA, the UM generally exhibits higher frequencies in December-January-February, but lower during the rest of the year.”

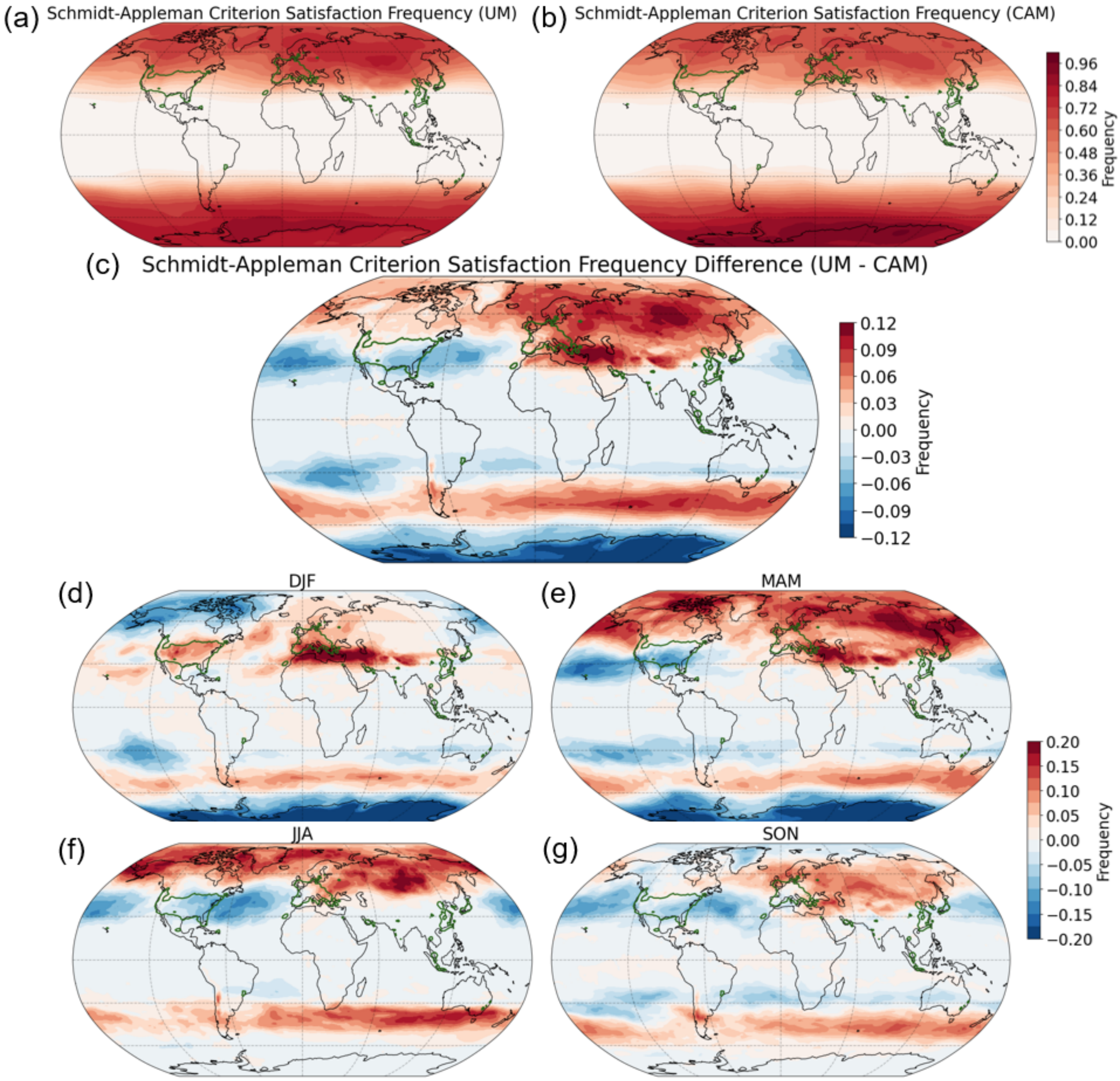

Figure 3. Maps of the annual mean Schmidt-Appleman criterion satisfaction frequency averaged between the 200 hPa and 300 hPa pressure levels for 2006, simulated in (a) UM and (b) CAM. Panel (c) shows the difference between the UM and CAM (UM minus CAM). Panels (d)-(g) show the seasonal mean of difference between the UM and CAM for 2006 in December–February (DJF), March–May (MAM), June-August (JJA), and September–November (SON), respectively. The green contour lines show where the mean flight distance in the AEDT air traffic inventory is larger than 50 meters of aggregated flight distance per second.

- Lines 301ff: I do not understand the second half of this paragraph. Before, it is claimed that the young contrail fraction depends on the total number of vertical levels rather than the resolution in the UTLS. But here, young contrail fractions from two simulations with similar vertical resolution in the UTLS, but clearly different numbers of vertical levels are compared and it is proposed that the differences result from differences in the model physics rather than the different number of vertical levels.

Sorry for the confusion, we have now changed the text to clarify what we mean here. The vertical resolution in the UTLS is the critical factor, as this is where contrails mostly form. In the paragraph in question, we compare results from CAM5 and CAM6 simulations, with different total numbers of vertical levels but similar vertical resolution in the UTLS. So the differences shown are more likely due to variations in the model physics, rather than the number of vertical levels. This has been revised in the new version at lines 367-374 as follows:

"Chen et al. (2012) reported a large dependence of the simulated young contrail cover fraction on the number of vertical levels in the UTLS used in CAM, with differences of up to a factor of 10 between simulations using 1000 m and 80 m vertical thickness in the UTLS. To account for this dependence, we compare the annual global mean young contrail cover fraction of 0.00018% (unnormalized, since the time steps for both CAM5 and CAM6 are identical) from our CAM6 simulations with 56 levels overall and ~1000 m vertical level thickness in the UTLS with the corresponding value of 0.000061% estimated in CAM5 with 30 vertical levels in total but similar vertical interval in the UTLS (Chen et al., 2012). Our CAM6 simulated value is therefore ~3 times larger than the CAM5 value reported in Chen et al. (2012), indicating the effect of the different model physics and simulated ice supersaturation frequencies in the two CAM versions."

- Line 362f: Where do the "simulated contrail ice mass" and the "contrail cirrus optical depth" come from? As far as I understood, the model is unaware of what is contrail and what is natural cirrus. Is this scaling factor only calculated and applied for "young contrails"? If this is the case, I could imagine that this contributes to the lower ERF in the UM, since the contrails would only have the enhanced optical thickness during the first model timestep of their lifecycle.

Yes, that is correct. The 'simulated contrail ice mass' refers to the young contrail ice mass initialized by the contrail parameterization, and the 'contrail cirrus optical depth' refers to the optical depth calculated by the changes in the total cloud optical depth (including both contrails and natural clouds) caused by contrails. So the scaling of the young contrail ice mass does indeed contribute to the smaller contrail cirrus ERF, as it only affects the early stages (i.e. first timestep) of the contrail lifecycle.

We have now clarified 'simulated contrail ice mass' and 'contrail cirrus optical depth' in the revised manuscript at lines 432-436:

"…, we adopt a method to enhance the contrail radiative response by implementing a scaling factor in the model radiation scheme for the young contrail ice mass initialized by the contrail parameterisation. The choice of this scaling factor is based on comparing the simulated UM contrail cirrus optical depth (the optical depth calculated by the changes in the total cloud optical depth including both contrails and natural clouds caused by contrails) with other existing contrail cirrus optical depth estimates."

The effect on UM ERF is now clarified at lines 473-475:

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

- Line 389f: Here, the ERF from the scaled UM simulations is compared with the change in cloud fraction from the unscaled simulations. I wonder, whether the cloud fraction and also the actual ice water path (not the one inside the radiation scheme) are impacted by the change in the radiation scheme.

Our intent here is to highlight the consistency between the cloud fraction patterns in the unscaled run and the contrail cirrus ERF in the scaled run. But the reviewer is correct - there is a larger increase in total cloud fraction in the cloud microphysics scheme in the scaled run (Fig. 1a below) compared to the unscaled run (Fig. 5a in the manuscript). However, the change in cloud ice water path in the scaled run (Fig. 1b below) remains similar to the unscaled run (Fig. 6a in the manuscript).

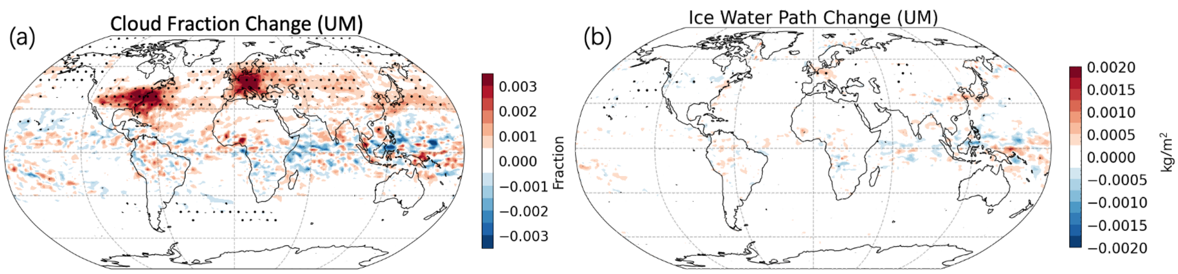

Figure 1. Annual mean (a) total cloud fraction changes at 220 hPa and (b) cloud ice water path changes in cloud microphysics from the scaled-UM

This has now been clarified at lines 470-472 as follows:

"There is a larger increase in total cloud fraction in the cloud microphysics scheme in the scaled-UM run (not shown) compared to the unscaled-UM run (Fig. 5a). But the change in cloud ice water path in the scaled-UM run (not shown) remains similar to the unscaled-UM run (Fig. 6a)."

- Line 391f: Could you comment on this apparent inconsistency? Could it be that CAM does not consider the contrail ice crystals as a cloud anymore, when they have sedimented, such that they still inhibit natural cloud formation but do not appear in the cloud fraction anymore?

The reason for this is explained in Sect 3.3 of Gettelman et al. (2021). We also mention this in our manuscript at lines 397-398:

"This relative reduction in cloud fraction in CAM has also been reported in Gettelman et al. (2021), where it was linked to the reduction in relative humidity caused by the local temperature increase from added contrail ice mass."

- Future work: I would also conclude that more studies of this kind with other climate models and other contrail parameterisations are needed, in order to give a reliable estimate of the model uncertainty and to narrow down the uncertainty together with better observational constraints.

Thank you for this point. We have now added this in the revised manuscript at lines 530-531:

"Another key areas for future work consist in additional climate models to better assess the uncertainties in model physics and observation studies to better constrain the contrail cirrus radiative effects."

Technical corrections

Thank you for the detailed comments on the technical corrections. They have now all been addressed in the revised manuscript.

Reference

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