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Editorial Office
Atmospheric Chemistry and Physics

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Dear Editor,

Re: Submission of "Effect of contrail overlap on radiative impact attributable to aviation contrails" to *Atmospheric Chemistry and Physics*

Thank you for arranging the review of our work. We thank the reviewers for their time and attention, and in particular for their comments regarding additional pertinent literature and for their suggestions regarding the paper's structure. We have now incorporated a deeper literature review, and have significantly restructured the paper to more clearly reflect the goals and novelty of the work. This includes the separation of the "Introduction" section into two components: Section 1, which motivates the work and explains our approach; and Section 2, which is now dedicated to providing a thorough overview of previous contributions in this field, and distinguishing our approach (and objectives) from theirs.

Please find below our responses to the comments from anonymous reviewers #1 and #2, as well as to the comment from Michael Ponater. In the responses below, we have listed the reviewer's comments in *italics* and our responses in **bold**. All line numbers refer to the revised manuscript, which will be submitted after this comment is posted as per *Atmospheric Chemistry and Physics* guidelines.

Anonymous Referee #1

The paper discusses the impact of overlap of contrails with other clouds and of contrails with other contrails for horizontally homogeneous clouds and contrails.

The paper offers a simplified radiative transport model to account for RF changed for overlapping cloud layers.

The main conclusions are:

1) *Contrail-cloud overlap is important. Contrails over clear sky and contrails over other clouds have far different radiative forcings (RF).*

True. However, that finding is not surprising and not new. It is not surprising because the clouds below and above contrails change the reflected solar radiation (local Earth albedo) and the outgoing longwave radiation. It is not new since you find that in several previous papers. See, for example Fig. 6, column 6, in Meerkötter et al. (1999; cited in the paper). That paper discussed the sensitivity of RF to various contrail and ambient parameters, including a cloud layer below the contrail cirrus and the optical depth of "background clouds". It showed that the net RF can increase from close to zero to a large positive value when background clouds get included. The value (94%) stated in the present paper here has no significance because it depends on the reference value and may vary from minus infinity to plus infinity when the net RF happens to be close to zero for the reference case considered. That makes no sense.
We agree that the phrasing of a 94% change was misleading in the context of an RF which is the result of changes in two components of different sign. Upon reflection we have changed the way that this is presented, providing information on the effect for both longwave and shortwave components in the abstract (lines 26-27) and conclusions (lines 1056) instead.

The present paper mentions Minnis et al (1999, cited) and Myhre et al. (2009, cited) and other studies, who discussed contrail-cloud overlap. The abstract and conclusions stress the importance and uncertainties of contrail-cloud overlap, which is correct, but report the findings as if that would be new, which is not correct.

Apparently, this discussion still reflects the history of the present paper, which apparently started with the Corti-Peters model with just one cloud layer (contrails or cirrus) over Earth surface and where the inclusion of other clouds changed the results considerably. This needs to be fully revised.
Based on the insights provided by all reviewers and commenters, we realized that the objectives of the paper were not written sufficiently clearly, and that the literature review did not do justice to the existing work in this field. We have therefore heavily reworked the introduction, splitting it into two sections as described in the opening text of this letter.

With regards to this specific comment, we have made a particular effort to clarify the novelty of this paper. We now separate out our motivation, including the specific aims of this work, in a dedicated Section 1. This is followed by a dedicated discussion of previous work on cloud-contrail overlaps (see Section 2.1) in addition to the approaches taken to account for contrail-contrail overlap in previous work, where relevant.

2) *Contrail-contrail overlap depends on the number and proximity of contrails. For present traffic, contrail-contrail overlap occurs on average over the globe but only rarely. This overlap may occur more frequently for increased traffic and under special flight track conditions.*

The treatment of contrail-contrail overlap is interesting. In areas with dense traffic, many contrails overlap with each other and with other clouds to different degrees. It would be good to have an efficient and still accurate method to account for the climate impact of contrails in such situation.
Contrail-contrail overlap may not be the most important uncertainty in contrail RF modelling. More important parameters may include the amount of ice supersaturation available in the atmosphere, the growth of the contrail cross-section by mixing with ambient air and the life time of contrails depending on many parameters (Schumann and Heymsfield, 2017, cited, and the references cited therein). Still, an investigation of contrail-contrail overlap effects and their modelling is of interest.

We agree with the reviewer that while this is a useful new possibility, it is certainly not the only (or even dominant) uncertainty in contrail modeling. We have made a renewed attempt to distinguish between physical uncertainties such as in available ice supersaturation or contrail ice microphysics (lines 69-71); modeling challenges such as the dependence of contrail-attributable radiative forcing on the representation of cloud-contrail overlap; and the unquantified effects of contrail-contrail overlap (lines 79-89).

Comments on the approach and results:

The radiative transfer model used to account for multiple layers (Section 2.1.3) looks interesting. It seems to have similarities with older theories; see, e.g., Hansen and Travis (1974) and Minnis et al. (1993); Minnis et al. (1998). A paper much cited in this respect is that of Ritter and Geleyn (1992). This part needs review by experts in this specfic field.

Section 3.1.4 compares results for this model with the Fu&Liou model. That is certainly an acceptable approach, as long as one can justify the plane-parallel cloud representation. Only few details are given on how the code was applied, for example with respect to the background atmosphere and aerosols and the specific model parameters. The comparison shows qualitative agreements with the multilayer model derived, but significant quantitative differences. So, how can we be sure that the results are correct? So uncertainties remain.
Additional data have been added with regards to the experiment implementation and design (lines 703-711). We also now have a discussion emphasizing the variation of the error with solar zenith angle (lines 729-731). We have also expanded our discussion regarding how differences between our results and those using FL96 might propagate to our results (lines 948-954).

Eq. (3) needs a bit more discussion: As it is written, this equation does not guarantee that RF_LW is positive. How often are negative values occurring?
We now clarify this on lines 346-348. As in Schumann et al. (2012), we treat negative values of RF_{LW} as being equal to zero.

Is Eq. (9) correct? 360? is that 360◦ (2 pi)?
This equation has been changed to instead use radians as suggested.

The discussion of the range of optical depth values (tau below 0.3) might be reasonable for global mean value, but locally the variability can be far larger (Atlas and Wang 2010).
This is correct. Our core results evaluate the impact of contrails with optical depths up to 0.5, at the upper end of contrails modeled by CERM. However we recognize that this does not cover the full range of possibilities. We have therefore generated alternative versions of Figures 2 and 3, showing results for contrail optical depths of up to 1.5 – potentially representative of more contrail-dense areas like Northern America or Europe. These figures can be found in the SI (Figures S3 and S4).

The paper presents contrail results from the model CERM. As stated, CERM does not account for contrail orientation and does not account for contrail position in a grid cell. How can one compute the contrail-contrail overlap effects without knowing the degree of overlap? The mentioned model CoCiP includes such geometry in more detail.
We apologize for the confusion on this point, and have modified the paper's introduction to clarify. As noted by the reviewer, the lack of orientation information from CERM is a limitation of this work, which we now discuss in Section 5. Accordingly, we assume maximum overlap between contrails, providing an upper bound on the effect of contrail-contrail impact with regards to radiative forcing. However, we agree that it would be an important improvement to either modify CERM to report orientation or to use results from a model such as CoCiP which already includes this data. We now state as much on lines 972-979.

How good do the meteorological data used represent humidity? Which time period is covered by the data, what is the spatial and temporal resolution of the data, and what is the vertical resolution in meters near the mean flight level height (around 10 to 12 km asl)? What is the fraction of ice supersaturated air masses in these data and how does this compare to published findings.
CERM uses a resolution of ~350 m at flight altitudes, but uses meteorological data from GEOS-FP which is provided at a slightly coarser resolution of ~500m at flight altitudes. We have an additional paper in preparation which assesses the magnitude of errors in relative humidity as calculated by GEOS, but this is out of the scope of this paper. We do however now mention known issues with relative humidity estimates in reanalysis (lines 1001-1005) including the possibility of a consistent humidity bias in reanalysis data (Jiang et al., 2015; Davis et al., 2017).

I assume, the model uses gridded emission source rates as provided by the FAA's Aviation Environmental Design Tool, but the paper gives no details on this. Are these data accessible to the community? Otherwise the results cannot be checked by other scientists.
We apologize for the oversight, and have added a sentence detailing the source of aircraft emissions data for this work on lines 399-400. Although CERM has previously been used with AEDT, we chose to instead use flight track data estimated by the Aviation Environment Inventory Code (AEIC) for this work (Simone et al., 2013). AEIC is an open-source tool, enabling independent validation of our results.

Very little is said about the satellite data CERES. The paper cites NASA Langley Research Center Atmospheric Science Data Center 2015 as reference and says that the data are provided at three-hour intervals. How can one derive hourly average values from 3-hourly data? How well do they represent the diurnal cycle and how sensitive are they to cirrus clouds and to geometrically thin contrails? How uniform is this sensitivity spatially and temporally, e.g. over land and oceans?
We now provide additional detail regarding the CERES satellite data on lines 447-473. As stated, CERES provides data once very three hours, and we assume constant cloud coverage over this period. Data are provided on a 1° by 1° global grid. We also discuss the likelihood of double-counting (i.e. contrails being observed by CERES while also being simulated by CERM) on lines 479-486. Sensitivity fo thin contrails depends on visibility thresholds. General visibility thresholds have been discussed lately and a common value is an optical depth of 0.05 (Kärcher et al., 2009), while a specific reference on MODIS instrument (used for CERES) mentions thresholds around 0.02 (Dessler and Yang, 2003). This is now mentioned in line 460-467.

The paper says that the "CERES instruments observe both contrails and natural cirrus clouds". I assume this means that CERES provides information only on the sum of contrails and other clouds. That should be clarified.
Correct. This is now clarified on lines 470-471.

The conclusion claims that the results "help to inform policymakers and researchers to identify technical, operational, and regulatory means to reduce these impacts." I think, based on the information given, the paper is still quite far away from this goal.
Based on this comment and those from other reviewers, we have decided to remove this conclusion.

The conclusion "The radiative forcing attributable to a contrail layer increases by a factor of three due to the presence of natural clouds on a global mean basis, but this varies by region" could be formulated inversely, e.g. "if a model would ignore other clouds the results could be wrong by a factor of three", but the conclusion should also make clear that this is not state of the art. Other models do account for ambient clouds.
We agree that the previous framing was unclear regarding the goals of the paper, while also not providing enough context regarding the depth of existing literature. With that in mind, we now provide a dedicated discussion regarding our contribution on the aspect of cloud-contrail overlap – specifically, to provide a quantitative analysis of the factors which contribute to and determine the magnitude of the effect that clouds have on contrail radiative forcing (lines 73-84). We also reiterate this point in the conclusions (lines 1026-1029).

In the abstract, the growth rate of air traffic is cited. I agree, growth rates of 4.5 % each year over the next 20 years have been estimated in the past by industry, as cited. However, such trend values have large uncertainty and I would recommend omitting such uncertain values from the abstract.
We have removed this statement from the abstract as suggested.

Unfortunately, the paper is not really clear and understandable and the conclusions are overselling the findings. The subject of the paper and some of the results are interesting but the approach and its presentation require major improvements.

I suggest splitting the paper into two parts: One on radiation transfer and one on the application. The first one should describe the model for the impact of cloud overlap on radiative forcing as a purely technical paper, with full validation. That paper should be reviewed by radiation transfer modelling experts. The other paper might then deal with the consequences of contrail-contrail overlap for climate forcing of aviation, addressing the corresponding community.
We hope that the reviewer finds that the revised structure of the paper is clearer, and more accurately conveys our intended message. We also hope that findings are no longer oversold. We believe that these revisions have helped to make a more coherent paper which is best packaged as a single unit, as the technical work (extending the Corti and Peter (2009) model to include multiple layers) was performed with the specific goal of enabling this comparison.

<u>*Anonymous Referee #2*</u>

The paper investigates the impact of cloud-contrail and contrail-contrail overlap on the radiative forcing due to contrail cirrus. The authors use a radiative transfer model of Corti and Peter that they modify in order to study the impact of cloud overlap on contrail radiative forcing. They consider two options no overlap and maximum overlap and study the difference when using those two assumptions. Cloud and contrail properties are varied systematically and the impact on LW and SW RF is analyzed. Cloud properties are prescribed using observed natural clouds (from Satellite) and CERM simulated contrails. Whereas, in principal the impact of cloud overlap is an interesting topic, the authors' extreme assumptions (no or maximum overlap) limit the relevance of the paper. They wrongly claim that other studies assume no or maximum overlap between contrails and clouds and contrails and contrails and claim that the sensitivity that they see is a measure for the bias of contrail cirrus RF published in the literature. Relevant literature that discusses the impact of overlap on RF in detail has not been discussed and comparisons with the results in the literature only partly made.

We agree that the treatment of literature in the previous version of this paper was insufficient. We have now dramatically expanded the literature review into a separate section (Section 2). This includes a dedicated review of previous attempts to quantify cloud-contrail overlap (Section 2.1) as well as the approaches used in other models to account for contrail-contrail overlap (Section 2.2).

I suggest presenting the work as a sensitivity study, including a detailed comparison with the many results in the literature and removing text stating that the present work improves contrail RF estimates and estimates the uncertainty in contrail RF in the literature or that the results help to inform policymakers and similar claims.

We have now softened our statements regarding policy applicability, including the removal of a sentence identified by Reviewer #1 as problematic in the conclusions. We have also worked to reframe the paper so that its objectives are clearer: to provide a first quantification of the effects of contrail-contrail overlap on net contrail radiative forcing, and to quantitatively investigate the relationship between physical

parameters and the effect of contrail overlap (with natural or artificial clouds) on estimated contrail radiative forcing.

Major comments:

1. *The sensitivity of contrail RF on the overlap analyzed in this paper is not a measure for the uncertainty in contrail RF in the literature. Both assumptions used in this paper, no or maximum overlap, are very extreme whereas in the literature mostly maximum random overlap for contrail-cloud and contrail-contrail overlap has been used. The statements that in the literature mainly maximum overlap has been used (e.g. line 346- 348) or that random overlap has been used for contrail-contrail overlap (line 350) the authors partly contradict their own table 1.*

This comment was very useful in directing our thoughts regarding uncertainty. We agree fully that what is being addressed here is not uncertainty so much as a previously unquantified component of contrail radiative forcing, and one where prior estimates of contrail radiative forcing had differed in their treatement. We have rephrased our study (Section 1) and have also separated out the previous treatments of contrail-contrail overlap (Table 2).

2. *The estimate for contrail RF is not an improved estimate relative to the estimates in the literature*

We agree that this is not the purpose of this paper and have removed such claims from the text.

a. *Neither maximum nor no overlap are good assumptions. Maximum overlap is certainly an upper bound for the overlap but far away from the truth.*

We agree with this assessment. The purpose of the paper is to provide a quantitative estimate of 1) the effect that variations in key parameters (e.g. location, season) have on the effect of multi-layer overlap with regards to contrails; and 2) a quantitative assessment of the potential impact that contrail-contrail overlap might have on contrail RF. Since the CERM simulation does not provide contrail orientation (line 407-412), we instead quantify an upper bound using the maximum overlap assumption. This is now explicitly stated in the limitations section (lines 971-979).

b. *Using only cloud data with 3 hourly temporal resolution does not allow for a realistic representation of contrail-cloud overlap or a realistic estimation of overlap frequencies. The low temporal resolution cannot resolve the correlation between cloud and contrail frequency.*

We have added this to our section on limitations (line 989). Although CERES is a state-of-the-science observational product, we agree that its low temporal resolution will bias estimates of the effects of cloud-contrail overlap. Since we also find other limitations associated with the use of CERES data, we have listed the identification of alternative approaches to incorporate natural cloud data as a high priority for future work (lines 1015-1019).

c. *Overlap assumptions have been shown to be dependent on vertical resolution. Even if vertically extended clouds are assumed the vertical overlap decreases strongly with layer depth. When levels are separated by more than 4km the overlap is essentially random (Hogan and Illingworth, 2000). At a low vertical resolution random overlap is a good assumption while maximum overlap is realistic (for vertically extended clouds) at high vertical resolution. At lower horizontal resolution the arguments must include a discussion of synoptic situations and the resulting vertical and horizontal statistics of the moisture field. Using observed cloud statistics aggregated in only 4 atmospheric layers and calculating the overlap with contrails the assumption of maximum overlap is far from realistic.*

We agree that vertical resolution is an important factor, for multiple reasons. Although CERM is capable of simulating the formation and trajectory of contrails at essentially arbitrary vertical resolution, it is limited by the vertical resolution of the moisture field as simulated by the meteorological data (in our case GEOS-FP data have a resolution of approximately 500 m at flight altitudes). The coverage data used in this work, generated by CERM, have a vertical resolution of approximately 350 m at flight altitudes, by interpolating RH and temperature from GEOS-FP. As for the issue of the vertical resolution of clouds, we are grateful to the reviewer for pointing out the assessment by Hogan and Illingworth, which we now cite as part of a discussion of this issue on line 991. We state there that, although a flawed assumption, we believe that maximum overlap is still the most appropriate approach to estimate an upper bound on contrail overlap impacts. However we fully agree that an assessment which prioritizes a more nuanced description of layer overlap, including higher vertical and temporal resolution of natural cloud data, should be prioritized for future work (line 1015).

d. *Assuming maximum overlap between contrails and contrails is not realistic. Even if planes would follow each other on the same flight track advection would mean that contrails don't maximally overlap. But this topic does not seem to be very promising as contrails have a low optical depth and the overlap between contrails does not impact the radiative forcing strongly.*

It is true that planes following each other on the same flight path would not be expected to produce overlapping contrails, but patches of overlapping contrail are frequently observed in satellite imagery (e.g. Minnis et al., 2011) and model estimates (e.g. Figure 9). However, we realize that our goal of finding an upper bound on this effect is not clearly enough stated, and have made efforts to clarify this throughout the manuscript.

e. *As the authors say whether a contrail cools or warms depends on the height of the clouds that may be vertically overlapping this contrail. The cloud height cannot be properly represented using cloud observations aggregated on only 4 levels.*

As discussed in the prior responses, we have extended our discussion of the limitations associated with use of the CERES natural cloud observations (lines 989-995 and 1015-1019).

3. *Comparison of the results to the previous publications should be improved.*

a. *The publications of Markowicz and Witek (2011 a,b) have not be cited. They discuss the impact of contrail cloud overlap in great detail e.g. the dependence on particle habit. They also show that contrail RF turns negative for all considered ice crystal shapes at much higher optical depth (at zenith angle 30◦) then shown in fig. 3a. As a zenith angle of 30◦ is often used in the literature it would be good to supply results for that angle in order to allow for comparison.*

These references are now cited in Table 2 and Section 2.1. We have also regenerated Figures 2 to 5 using a solar zenith angle of 30° and added them to the supplementary information (Figures S3, S6, S7 and S8).

b. *The results in Fig. 7 should be compared with results of e.g. Schumann et al. 2012 and Myhre et al 2009 in detail. Why are absolute values of contrail RF so different from previous results?*

This comparison is included on lines 760-765. The resulting global sensitivity in all-sky conditions is of the same order as previous estimates (~100 mW/m^2). The differences in clear-sky results might be due to the assumed asymmetry parameter, with a significant impact on global longwave radiative forcing in clear-sky conditions (Sanz-Morère et al. 2020).

4. *The result that cloud-contrail overlap is responsible for 93% of the net radiative forcing attributable to contrails in 2015 relies on the assumption that the authors have simulated the 'true' overlap between contrails and natural clouds which is not the case. Instead they should say that overlapping contrails maximally with clouds instead of prescribing no overlap leads to an increase in radiative forcing by xx%. The same is true for the statements about the importance of contrail-contrail overlap. Note also that those values will be resolution dependent so that their significance is very limited and should not appear in the abstract!*

We agree that this is misleading. We've removed the statement in question, instead providing the contribution to longwave and shortwave individually which should be more robust. We have also made this modification throughout the paper (e.g. lines 854-855).

5. *Line 891-892: How do you suggest avoiding cloud contrail overlap? Contrails mostly form close to natural clouds which means that they often overlap with other clouds. Cloud-free areas are mostly dry and therefore persistent contrails cannot exist.*

There have been several studies of possible contrail avoidance strategy which may be pertinent here, including recently by Teoh et al (2020). This suggests that, if high-accuracy meteorological data are available, it may be possible to avoid contrails through altitude adjustments. However, we do agree that contrail avoidance is a non-trivial task, and have added this caveat on line 1050.

Minor comments:

1. Table 1 is incomplete, contains mistakes and is misleading: The table serves to show the great scatter in the contrail RF estimates but it omits to say

- that the Marquart et al and the Frömming et al estimates are for line-shaped contrails only whereas the other estimates are for contrail cirrus.
- the main difference between the contrail cirrus modeling studies are the different ways of treating contrails, keeping them separate from natural clouds or treating them with the cloud scheme, and the contrail initialization. The Chen and Gettelman study follows a very different approach from the others.
- That there is another estimate for contrail cirrus RF (Bock and Burkhardt, 2016 – which is already in the literature list) that lies in between the Schumann et al. and the Burkhardt and Kärcher estimate. This means that 3 of 4 estimates lie close together.
- Chen and Gettelman include contrail-contrail overlap since overlap is dealt with in the cloud scheme. The model assumes maximum random overlap for clouds. Only at the initialization stage contrails (age of up to ~30 min.) do not overlap but that is not the same as no overlap between contrails in general. That means that the scatter between contrail cirrus RF estimates is much smaller than suggested by the current table 1 and that the scatter is not due to the overlap scheme. Even though the uncertainty in the overlap between clouds and contrails leads to an uncertainty in contrail cirrus RF, the results from the literature do not demonstrate this fact as they tend to use the same overlap scheme. The range of net RF due to contrail cirrus encompasses the estimate of this study only because estimates for line-shaped contrails are included here.
A deep analysis of previous literature has been done for this revision. Table 1 has been completely rebuilt, adding two new parts (now Tables 2 and 3), and each part reviewed for accuracy. Additionally, a whole section (Section 2) has been added to better address existing literature. This new section in particular benefited greatly from the comments of both reviewers and Michael Ponater, for which we are grateful. We no longer list the Chen and Gettelman (2013) estimate in Table 2, and instead discuss their approach on contrail-contrail overlaps on lines 185-188 of Section 2.2.

2. Biofuels have little impact on contrail formation likelihood but instead on soot number emissions, ice nucleation and contrail life times and optical depth (Moore et al. 2017, Kärcher et al. 2018, Burkhardt et al. 2018)
We have adjusted this comment to make clearer that biofuels will affect the properties of contrails, and not necessarily their likelihood of formation (line 67).

3. Marquart et al (2003) and Frömming et al. (2011) both use the contrail parameterization of Ponater et al. (2002) and simulate line-shaped contrails of varying optical depth including those with an optical depth smaller than 0.02. They both calculate the fractional increase in cloudiness due to contrails. Only the overlap was calculated differently.
This has been corrected in Table 1.

4. Chen and Gettelman do not assume zero overlap between linear contrails when calculating radiation. Instead they use the overlap scheme of CAM (maximum random) in order to take care of overlap between different clouds.
This has been corrected in Table 3. Since Chen and Gettelman (2013) use a very different approach, we no longer list their estimate in Table 2, although it remains in the discussion (lines 185-188).

5. Page 6: You cite papers that determine if cloud-contrail overlap reduces or enhances contrail RF and conclude that this is a major uncertainty instead of mentioning the known fact that the effect depends on the contrail and cloud properties and temperature/height.
We agree that the prior phrasing was incorrect, and that this is a specific issue given that quantifying this dependence is a goal of our paper. We have rephrased the corresponding text in Section 2.1 (lines 169-174). We have also emphasized this in the Introduction in an attempt to better clarify the goals of the paper with regards to investigating this dependence (lines 73-84).

6. On page 7 you talk about contrail-contrail interaction. Please note that interaction can happen due to a number of processes and does not necessarily have to do with radiation.
We have removed this comment as part of the restructuring of Section 1 into two sections.

7. On page 11 the error of the model compared to FL96 is determined. In that context the systematic bias, the underestimation of the dependency on the zenith angle, should be discussed and its impact on the results of this paper.
We agree that this is important. We have extended the discussion in Section 4.1 to draw attention to the possibility of biases combining (lines 721-733). We have also extended the limitations section (Section 5.1) to discuss how these two sources of bias may affect our results (lines 951-954).

8. Line 341-344: All 3 sentences are unclear. It is not clear what it means to aggregate single contrails into one layer! Are coverages due to single contrails added up or not? 'Overlap between . . . is therefore not explicitly resolved': What does that mean – minimum overlap? 'the same approach . . ... as if clouds were centered in the grid cell'. What does that have to do with overlap. Equations would make this text easier to understand.
We have completely rephrased this paragraph, and split it into two for clarity. The comment regarding some overlap not being "explicitly resolved" regarded cases where contrails might form in very close spatial and temporal proximity, an inherent limitation of the version of CERM used here.

9. Line 349: what does '(linear in most cases)' refer to?
This clause has been deleted as it does not add meaning.

10. Line 349: I assume you mean to say that contrail area 'overlaps' and not 'interacts'
Thank you, this has been corrected.

11. Line 353: what is 'potential maximum overlap'
This sentence has been changed to make the intended meaning clearer - that there may be more colinear contrail overlap in flight corriders, while overlaps over areas such as the mainland US may more often inolve unaligned contrails (lines 419-430).

12. Line 355-357: even more useful would be a higher temporal and vertical resolution. The lower the resolution the larger the overlap.
We agree. We have extended our discussion of the advantages of greater temporal and vertical resolution (lines 989-995).

13. In order to correct for errors in the model the authors use for the clear-sky estimates CERES data. Without any discussion where the errors are coming from it is difficult to understand if this approach is acceptable. The radiation emitted from the cloud could still be affected by this error which would mean that the estimated contrail RF would include this model error.
Lolli et al. (2017) found that the regression used by Corti and Peter (2009) to estimate longwave emissivity resulted in significant errors for lack-body temperatures greater than 288 K. We assume that those temperatures are most relevant to the surface, for which we now use the clear-sky CERES estimate. We have added clarifying sentences to this effect on lines 289-306.

14. The term 'independent overlap' needs to be defined.
In this work we distinguish between 'independent contrails' and 'overlapping contrails' with regards to how RF is calculated. This has been clarified in the caption for Table 4 (line 922). We compare 'independent' contrails to 'overlapping' contrails, with the former treating contrails as if there was no radiative interaction between them. A comparison between both cases can also be found in Section 4.1.1.

15. Line 434: The asymmetry parameter in Schumann et al is 0.787 (and not 0.77) for older contrails and 0.827 for younger contrails.
The choice of 0.77 is based on an analysis in a recent paper (Sanz-Morère et al 2020) which drew on the results from Schumann et al (2017) among others. We have extended the reference list appropriately and have added a brief explanation of how the number 0.77 was determined (lines 518-521).

16. Line 503: Figure 3b is the upper right panel
This has been corrected.

17. Line 854: You probably meant to cite Kärcher et al., 2009 and not 2002.
This has been corrected.

18. Line 886 – 888: Those conclusions depend very much on the type of clouds you prescribed. In the tropics many very thick clouds can be found and differences between cloud and contrail top temperature can be very large as well.
This is true. A comment to this effect has been added in the conclusions (line 1044).

Specific comments from Michael Ponater

Introductory remarks

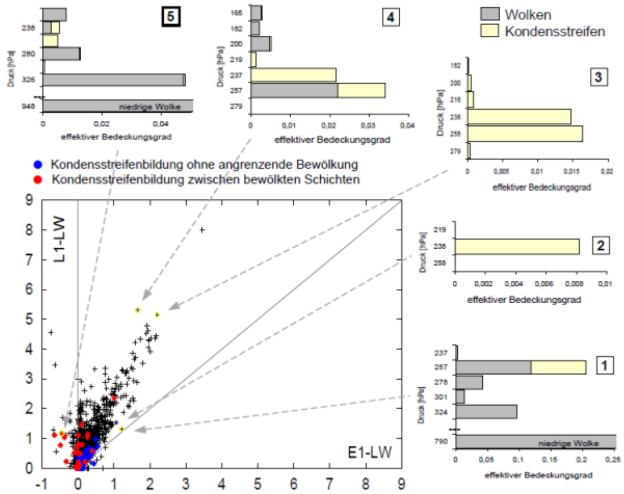
I find this work of Sanz-Morere et al. quite interesting as it emphasizes an important aspect in contrail radiative impact studies that usually has not been investigated as systematically as is done here. In this respect, I feel the two official referees have taken a somewhat stern attitude towards the paper. I think that using a parameter scanning approach in assessing cloud-contrail and contrail-contrail overlap situations goes beyond what currently available studies have done. Figure 3 of the present paper is certainly worthwhile providing. Yet, the referees are certainly right when reminding the authors not to overreach their conclusions, given the limitations of the model framework used in the paper. It is also true, I agree, that some aspects (and even whole papers) of previous research work has been overlooked by the authors.
However, this specific comment is mainly written to provide additional information (including insider knowledge of previous papers) that can help to rectify some statements where the authors – in my view - have interpreted previous work inaccurately.

We would like to express our gratitude to Dr. Ponater for taking the time to read and comment on our manuscript. Based on this comment, and those of the reviewers, we have dramatically reworked our introductory section (now two sections), including an extended discussion of previous work which was previously not well represented.

General comments

- *Contrail-cloud overlap has been generally accounted for in previous contrail radiative impact studies in the way the contrail radiative forcing is usually given under clear-sky as well as all-sky conditions (Myhre and Stordal, 2001; Stuber and Forster, 2007; Yi et al., 2012, and others cited by the authors). It has been a common finding that both the shortwave and the longwave radiative forcing decrease in magnitude under cloudy-sky conditions. Often, but not always, the daily mean net radiative forcing gets more positive with natural clouds included.*
We now dedicate a subsection (2.1) to better exploring how previous studies have quantified cloud-contrail overlap. We hope that this section provides a fairer evaluation of the literature, and better highlights the contribution of our work.

- *As rightly pointed out by referee 2, contrail-contrail overlap has usually been accounted for in contrail studies with global climate models (Marquart et al., 2003; Rap et al., 2010, Burkhardt and Kärcher, 2011; Bock and Burkhardt, 2016), except for when used in idealized setups like the GCMs (ECHAM and CNRM) contributing to Myhre et al. (2009). For illustration what situations can occur in climate models, I reproduce a figure from the PhD thesis of Marquart (2003, unfortunately only available in German language). This picture makes it clear that layers with contrails not only may overlap with layers containing natural cloud, but that situations with contrails and natural clouds existing side by side in the same grid box are also possible. That renders the overlap situations in models like that rather complicated, even if the overlap principle is straightforward. Anyway, it is clear that the climate model parameterizations can include the effect. Possible grid box column situations containing contrails (yellow) and natural clouds (grey) in the ECHAM4 model of Ponater et al. (2002), Marquart (2003), and Marquart et al. (2003). Adapted from Marquart, 2003, her Figure 2.7.*
Thank you very much for this insightful comment and for the useful illustration. We agree and acknowledge that our approach is an approximation only, and is most useful for quantifying an upper bound of multiple layers overlaps impacts on contrail attributable net RF. We have modified our literature review to more accurately reflect the approach taken in GCMs (Section 2), with references added in Tables 2 and 3.



Possible grid box column situations containing contrails (yellow) and natural clouds (grey) in the ECHAM4 model of Ponater et al. (2002), Marquart (2003), and Marquart et al. (2003). Adapted from Marquart, 2003, her Figure 2.7.

Specific comments

• *It is important to realize that the GCM studies (at least those based on the ECHAM climate model) use the maximum-random overlap scheme for calculating radiative fluxes in all sky columns. This refers to contrail-cloud overlap as well as to contrail-contrail overlap situations. See, for example, Figure 4 in Marquart and Mayer (2002).*
This has been added in Table 2 as well as in section 2.1.

• *It may be of interest for the present study that the use of the maximum-random overlap principle has been shown to create severe problems when used in an unfavorable parameterization combination, as discussed by Marquart and Mayer (2002). This has caused the radiative forcing values given by Ponater et al. (2002) to be basically incorrect (amended in Marquart et al., 2003).*
We agree that this caveat is important. We have reviewed Marquart et al (2003), Marquart and Mayer (2002), and Ponater et al (2002), and have attempted to synthesize this in Section 2.1 (lines 135-140).

• *It is not correct (as given in your Table 1) that the overlap assumption in the studies of Marquart et al. (2003) and Frömming et al. (2011) is different. Both use the maximum-random overlap principle in the radiative transfer calculations, as do Burkhardt and Kärcher (2011).*
We have re-reviewed the appropriate literature and corrected our summary accordingly. This is now reflected in Table 2.

• *Note that the ECHAM4 studies of Ponater et al. (2002), Marquart et al. (2003), and Frömming et al. (2011) mainly give mean optical depth values of visible contrails (i.e., averaged over those contrails that exceed a "visibility threshold" of 0.02), to enable comparison with observations. However, the "invisible"contrails are not excluded from the radiative forcing calculations. This may confuse an unaware viewer of your Table 1. I also note that the visibility threshold has been a subject of debate. According to Kärcher et al. (2009) a threshold value of 0.05 is more appropriate and has been preferred in later studies (e.g., Bock and Burkhardt, 2016).*
We have updated Table 1 accordingly, and attempted to added a brief discussion in Section 3.2.4 regarding assumptions on satellite contrail visibility, and how "invisible" contrails are treated in radiative forcing calculations (lines 447-474).

• *The global mean optical depth value of 0.05 given in Table 1 for the Frömming et al. (2011) results seems to be incorrect. Table 2 in that paper provides the consistent value of 0.08. The confusion may originate from the first paragraph of Frömming et al.'s section 3.1, where the optical depth of all contrails (including "invisible" ones below 0.02) is additionally given, and this one is indeed 0.05.*
Table 1 has been corrected accordingly.

• *Finally I recommend to split your Table 1 into two parts, one referring to lineshaped contrails (first two rows), and one to contrail cirrus (last four rows). Otherwise any reader observing totally different radiative forcings for*

nearly the same air traffic volume will be misleaded and will mistakenly be tempted to attribute the difference to the cloud overlap assumptions!
We agree that this improves the paper significantly. We have split Table 1 as suggested.

- *Line 89: As stated above, I disagree with the claim that Frömming et al. (2011) assume random overlap in the radiative transfer calculations.*
This has been corrected.

- *Line 239: Do you mean sufficiently thick or sufficiently extended? Yet, either assumption appears to be somewhat bold for contrails, I think.*
We have clarified this on line 308. This is a significant assumption, and one which we would like to investigate in more detail in the future. The overall error for contrails has previously been estimated at around 10% (Gounou and Hogan 2007) (line 312), but may be greater for the purposes of investigating contrail overlap specifically. We state this in our Limitations section (lines 937-940) but now also include a statement that the inclusion of 3-D effects would be a productive topic for future work (line 1021).

- *Line 256: "Due to the known strong dependence …" I think at this point the fundamental work of Markowicz and Witek (2010) on the subject ought to be acknowledged.*
This reference has been added on line 326.

- *Line 408: Here, or somewhat earlier, the notion of "quantifying the effect of cloud overlap by the difference of all-sky minus clear-sky" should be scrutinized a little bit. The point is that Rap et al. (2010) extensively discuss the potential effect of a correlation between contrails and natural clouds, increasing the frequency of all-sky situations with respect to clear-sky situations in comparison with a setup assuming climatological background (natural) clouds. My impression is that you do not account for this correlation in your calculation setup. If this is true, it might be fair to mention this as a caveat.*
We now include this in our opening literature review, but given the concerns regarding this work specifically we have also added a discussion on this topic in the Limitations section (lines 962-969).

- *Line 431: I think that there are also many contrails below 0.05 (see Kärcher et al., 2009). This only supports your approach to extend your parameter space down to tau = 0!*
This is a good point – we have corrected line 516 to emphasize this strength.

- *Line 439: In situ measurements like the one cited here may not fully represent the parameter range of contrails, hence you might consider to add some citation of a satellite study such as Bedka et al. (2012) or Minnis et al. (2013).*
We have reviewed the studies suggested to ensure that our parameter range is reasonable, and have added them as references (section 3.1.1).

We would again like to thank the reviewers for their time and insight, and believe that their input during this review process has improved the paper substantially. Thank you again for considering our manuscript for publication in *Atmospheric Chemistry and Physics*, and we look forward to your response.

Best wishes,

Sebastian Eastham

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