Review of the paper by Inés Sanz-Morère, Sebastian Eastham et al.: Effect of contrail overlap on radiative impact attributable to aviation contrails

The paper got improved in many respects.

Still it needs further changes before it can be considered for publication:

It became now a rather long paper. Parts need further explanations. Other parts may be shorted instead.

Treatment of overlaps between contrails and other clouds is an important issue but not new. But the various approaches used so far are now reviewed, and I think this is valuable. I am not sure that this paper extends our understanding of contrail-cloud overlap much, but that is not the main topic.

I agree, treatment of overlaps between contrails and other contrails is likely important, at least in regions with high air traffic density, and more understanding and proper method to account for such overlaps is welcome.

Unfortunately, I am not certain that the approach and assumptions used in the modelling of overlapping clouds, Section 3.2 and Appendix B, are fully justified. In fact, I had suggested publishing the technical part separately and letting it review by radiation transfer experts. The authors decided not to go this way. O.K. But then I now have to bring forward my concerns about the model in more detail:

Reasons for concern:

The authors assume (line 345) that the downward LW fluxes remains unchanged when an upper-level cloud layer is induced. I questioned this assumption in my earlier review. But the response is not fully satisfactory to me. The downward fluxes change not only when the temperatures change but they change also when the upward fluxes change and cloud layers scatter long wave radiation backward. It seems that the model cannot account for this. Please discuss. But I would not simply state that these effects are negligible without further studies.

In Chapter 3.1.1, line 235: please explain how epsilon, the contrail emissivity, is computed (see Eq.4 in Corti and Peters). It is an important and simple relationship to the optical depth, but with an adjustable parameter delta, and it assumes a simplified scattering model, which may be appropriate. But how is this parameter determined? In principle this relationship should be different for SW and LW fluxes and should depend on the ice particle properties and on absorption and scattering material in the atmosphere.

Chapter 3.1.3, Line 356 ff: The authors should make clear that Eqs. (4, 5) in the present SW model are identical to the Corti&Peters model for single layered cirrus clouds. Hence, the present model adopts the same underlying assumptions like isotropic surface scattering (see text before Eq. (12) in Corti & Peter, 2009). A more realistic non-isotropic wavelength-dependent bidirectional reflectance distribution functions (BRDF) for surface scattering could change the present model equations and its results considerably. Also any wavelength–dependent absorption by water vapor in the atmosphere between surface and cirrus layer near-infrared solar radiation absorption by ice particles in the cirrus clouds is not included in this model. So it cannot explain, e.g., the sign change shown by Myhre and Stordal (GRL, 2001, doi: 10.1029/2001GL013193), their Fig 1, for low zenith angles and large albedo values – though these negative values may be of little relevance practically.

Please note that the Corti & Peters model results were compared to a set of libRadtran simulation results as benchmark before. See Table 2, ”Cloud free” cases, in Schumann et al. (2012). Here the Corti & Peters model exhibited considerable deviations, both for LW and SW components. Hence, your model may account for cloud overlaps but may introduce other problems, e.g., because of non-isotropic surface reflection or missing water vapor absorption.

Fig. A3 shows that the ratio of RF_SW values for two different surface albedos (0.3 or 0.5) can be strongly sensitive to changes in solar zenith angles, as shown by the present model and by the FL96 model application, while the COCIP RF parameterization does not show this dependency. The mentioned Myhre and Stordal (2001) results suggest an even stronger sensitivity and sign change, though cases with high albedo may occur rarely at low zenith angles in reality. Since there is very little discussion, the results may be suggesting that the new model is “better”. However, I miss a fair discussion and physical explanation of these results: How was CoCiP applied for this comparison? One of the important CoCiP input parameters is the TOA reflected shortwave radiation (RSR). How was this input determined? How frequent are cases with low zenith angles and high albedo values occurring (e.g. in your study used for Fig. 7)? Therefore, this needs either a suitable discussion (which takes more space) or, if there is no clear physical explanation and fair assessment of the results, I suggest removing lines 1400 ff and Fig A3.

I doubt about the wisdom of the decision to refer the longwave RF to the clear sky outgoing longwave radiation, OLR_{clear}. The clear sky OLR is unknown and cannot, strictly speaking, be measured, because for cloudy atmospheres, the clear sky situation is a fictitious situation not existing in reality. Any attempt to measure or to compute the OLR_{clear} is by necessity approximate. I think one of the big steps forward with COCIP was to relate the contrail RF to the top of the atmosphere irradiances, OLR and RSR. These terms have a meaning in reality and can be measured and computed. The contrails act as disturbances of the atmosphere without contrails and the OLR and RSR for the atmosphere without contrails are well described by NWP models which compute the state of the atmosphere without contrails. Moreover, contrails impact radiation properties directly mostly above other clouds, remote from the surface. Hence, I do not agree to the wording praising the progress in using OLR_{clear} instead of surface temperature, in the lines near 290-295. I ask that the alternative of TOA irradiances is at least mentioned as an alternative.

Line 295: The OLR error estimates for CERES data (1.7%) may apply to its global and annual mean values, but not to local values. Local errors can well exceed 20 W/m2. There is plenty of literature on this. Please change the text accordingly

I appreciate the comparison to the Myhre et al. (2009) test case shown in Fig 6. However, the figure is hard to read and reports the results only qualitatively. Please report and discuss mean values as in the Myhre et al. paper (their Fig. 5) or in the related Table 3 of Schumann et al. (2012) and the extended Table A1 in Schumann and Graf (2013, JGR, doi: 10.1002/jgrd.50184.).

Details:

Tables 2 and 3 refer to an asterisk "*Only linear contrails considered), but I cannot see the **asterisk** in the tables. I would prefer omitting the asterisk.

Line 120: I do not understand the sentence in the bracketed version. Why should a change from liquid to ice cloud change the sign in the net RF? Is this a general finding worth mentioning?

Table 2, line Schumann et al. (2012). Please extend the table text slightly: "Parametric RF model as a function of contrail properties, longwave and shortwave fluxes from below and above the contrail, and optical depth of clouds above the contrail."

Please explain similar to: CoCiP computes the RF as a function of contrail properties (temperature, optical depth, ice particle effective radius, ice particle habit), upward fluxes in the atmosphere from below the contrails (upward longwave radiation and reflected shortwave radiation), solar constant for given time of the year, and solar zenith angle, and optical depth of clouds above the contrail. Any clouds in the atmosphere below the contrails cause changes in the upward fluxes. CoCiP takes these upward fluxes from model output from NWP or climate model results. This way, the parameterized RF takes into account changes in the contrail-RF caused by clouds below the contrails.

Table caption near Line 115: ECMWF, replace "Forecasting" by "Forecasts".

Line 139: I miss Marquart and Mayer (2002) in the list of references.

Line 146 and corresponding reference: Replace "Radel by "Rädel".

Line 208: delete "or re-emission" (or do you think about fluorescent clouds?)

Line 275: "long-loved" ?

Line 452 and other places: Replace "Kärcher, 2009" by "Kärcher et al., 2009".

Line 597: Kärcher and Burkhardt (2013) –> Burkhardt and Kärcher (2013) or missing reference.

Eq. (9) Please omit the second term "Delta RF" – you do not need it and it is misleading because RF has the unit W/m^2 while the term you are discussing is a unit-less ratio.

Lines 1026 -1029: Tempus? Past tempus better than present?

Reference Boeing. https://www.boeing.com/resources/boeingdotcom/commercial/market/commercial-marketoutlook/assets/downloads/cmo-2019-report-final.pdf - Please check the address.

Appendix B1.1, Eq. (B2): there is a sign error in the second part of the equation. I have not checked how subsequent equations are affected.

Line 1315: avoid "but", e.g. by replacing ", but requires an effective radius" by "for given ice particle effective radius" (effective radius is now a standard definition). In fact, the contrail and cirrus RF depend on the particle sizes. It depends actually on the ratio of particle sizes to wavelengths. This was shown clearly long ago, e.g., by Zhang, Y., A. Macke, and F. Albers, 1999: Effect of crystal size spectrum and crystal shape on stratiform cirrus radiative forcing. *Atmos. Res.*, **52**, 59-75, doi: 10.1016/S0169-8095(99)00026-5.

At many places in the text and in the title I suggest replacing "contrail-attributable radiative forcing " by radiative forcing by contrails". The word "attributable" suggests wider implications (Climate changes get attributed to anthropogenic activities, e.g.). In line 1057: "global contrail RF" without "-attributable" is shorter and clear enough.

After all, I agree that it is worth to discuss the effects of contrail-contrail overlap and it is worthwhile to provide approximate methods to account for this. I think the paper could still be shortened and reduced in emphasis in respect to cloud-contrail overlap issue because that is not new. With respect to the treatment of contrail-contrail overlap, the model presented is interesting but it should be clearly stated that it is based on important simplifying assumptions and approximations which the users should be aware off.

Finally a remark: A paper that the team published in parallel (Sanz-Morère et al., in Environ Sci. Technology, 2020) cites the present submission with the sentence: "The model calculates radiative transfer using a two-stream approximation and was validated by comparison against other radiative transfer models." I would say: the present study tests the model by comparison against other radiative transfer models but it does not validate it. The term "validation" is rarely appropriate when the truth is unknown.