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Interactive comment on "Effect of contrail overlap on radiative impact attributable to aviation contrails" *by* Inés Sanz-Morère et al.

Anonymous Referee #1

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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 verlapping 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.

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.

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.

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.

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?

Is Eq. (9) correct? 360? is that 360° (2 pi)?

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).

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.

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.

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.

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?

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.

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.

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.

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.

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.

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