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Discussions

Interactive comment on "Contrails and Their Impact on Shortwave Radiation and Photovoltaic Power Production – A Regional Model Study" *by* Simon Gruber et al.

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This is a nice model study of contrail and contrail cirrus formation on a regional scale. The approach is straightforward in extending a two-moment cloud microphysics scheme by including a separate contrail ice class. The scale jump from young contrails at aircraft wake vortex scales to grid scales is approximated by assuming that the contrail ice spreads immediately over a grid scale (vertically and horizontally). Part of the effects of this strong simplification is corrected by using an ice-crystal loss parametrization derived from LES results.

Of course this simple approach is possible only because the model study is restricted to

regional scales, with just one day of simulation (actually only 8 h of contrail formation). The model is indeed useful to study regional effects of contrail cirrus on shortwave surface radiation and possibly on weather prediction. If applied for climate studies at global scales and for long simulation periods (years), the present approach would suffer from the same problems as other global models. For example, a regional model would require contrail cirrus boundary conditions if run for longer time periods, which are nontrivial if contrail effects outside the region impact the meteorology at inflow. Any climate simulation would also require coupling to oceans etc. Nevertheless, the approach is useful for regional studies and interesting.

Of course, finally, the study should be published, though several minor and some major text issues need to be considered, as listed below, before the paper is acceptable.

Page 1, Line 11 insert "and humid" after "hot".

Line 13 Note that the threshold temperature is pressure dependent; hence, -45°C is perhaps even too rough. More relevant is the fact that below a temperature near -38°C to -40°C, contrail particles, which are formed in liquid phase initially, freeze homogeneously and quickly to form ice particles which then persist in air with relative humidity below liquid saturation (but above ice saturation).

Line 17: there are other long-life-time contrail observations, but I agree, the one described by Minnis et al. (1998) is an early example. Others were summarized in Schumann and Heymsfield (2017), who also review the definition of contrail cirrus and other related knowledge.

Line 20: It is not clear whenever important properties are "sufficiently Investigated", and there are many more observation (see Iwabuchi et al., 2012; Vazquez-Navarro et al., 2015; Schumann et al., 2017) and model studies than those covered in the IPCC report (Boucher et al., 2013).

Page 2, line 17-18: "whether this study is the first of its kind" is at least debatable. In particular since the authors do not discuss earlier attempts like those of Duda et al. (2004) at this place. But I agree that the simplicity of the present approach including a balanced microphysics model (similar to recent approaches in global models) is attractive and the authors can claim a fresh approach.

Page 3, line 1-3: The method developed by Schumann (2012) and Schumann et al. (2015) (added reference, see below), though certainly with limitations, is still likely the only one covering the scale transition from thousands of single contrails to multi-year global climate cases. This method is not represented fairly in this citation (and in this Introduction) which correctly mentions a problem but misses to mention the advantages of that approach. In fact, the mixed Lagrangian-Eulerian approach could be listed as a basic alternative to the Eulerian grid scale models. This approach has also been used by Caiazzo et al. (2017).

Page 3, lines 13-15: The paper seems to make a big deal out of using 8 h of traffic movements. There were earlier studies doing far more in that direction (e.g. , Schumann, 2012; Voigt et al., 2017).

The authors did not consider a case for which insitu- and satellite observations and other model studies are available, such as the ML-CIRRUS observations of 10 April 2014 over Germany (Voigt et al., 2017), for which the waypoint-traffic data (partly also from flightradar24.com) are available for about 4 weeks and for nearly the whole of Europe. See, e.g., Fig.4 in Voigt et al. (2017). The existence of such data for future studies should be mentioned.

Perhaps, the Introduction should mention the use of satellite data. But it should mention that there were many studies of satellite data in the past (from polar orbiting and geostationary satellites) and also a large variety of in-situ observations is available. So far, I feel that the discussion in this paper is too much biased to LES results instead.

Page 4, Lines 20 to next page: The text explaining the parameters used in Eq. (2) appears a bit lengthy. I assume it can be reordered and shortened.

Page 5, line 23: reorder “several input” -> “input several”

Page 5, line 28: Is there any physical argument for using this maximum mixing ratio limit value? If not, say that this is arbitrarily taken. How sensitive are the results to this threshold?

Page 5, lines 30 ff. The team around Ping Yang has developed an ice particle parameterization including smaller ice particles in recent years (see Bi and Yang, 2017, e.g.). The existence of such parameterizations should be mentioned and such new parameterizations could be used at least in future studies.

Page 9, line 2: The introduction to this section, with “In contrast to previously mentioned global modeling studies,..” and with “globally averaged fuel consumption. . .” is no longer true if you mention CoCiP properly.

Page 7, line 4: replace “a potential function” by “a power law” or similar.

Page 7, line 13: “Microphysical properties” – this is a very vague term. What do you mean? If you mean optical extinction, I am not sure that your statement is correct (I expect that extinction and optical depth are equally sensitive to number and mass).

Page 8, line 19: 600 m is a large upper bound which is reached very rarely. To be fair, the lower bound should be correspondingly small (100 m; even smaller values occur for small business jets).

Page 9, line 2 (“In contrast to..) : Note that some previous global model studies used similar data for the whole year of 2006 (“ACCRI" waypoint data). Hence, this is not really a big step forward.

The word “exact” does not fit well to this description. When are data exact? Also “real time-based” data is not really the right term. I would simply say you use traffic waypoint data from transponder data (not radar).

Page 11, line 3: Here and at several later places you could also cite observation results.

Here, e.g., Petzold et al. (1997) and Heymsfield et al. (1999) found strongest growth at the edges of contrails from in-situ measurements.

The discussion of observability of contrail cirrus is not needed in this paper. Otherwise, the statements are controversial and would require more in-depth discussion for completeness. For example, observations discriminate contrails and cirrus also based on the concentration of exhaust trace gases and aerosols and use trajectory analysis, partly in correlation with air traffic and meteorological situation history (Voigt et al., 2017). I suggest reducing this discussion in this paper. It is not needed for this paper.

Page 14, line 1 etc.: I agree that Fig. 5 exhibits a remarkably thick layer with apparently strong supersaturation (what is the maximum RHi value in this figure?). I wonder how this layer developed. Is this the results of initial conditions or the result so vertical lifting or radiative cooling? How realistic is the model result in this respect? As far as I understand, the high humidity coincides with some thin cirrus. How can the humidity persist so long in the presence of cirrus? If there is no cirrus yet then I would have expected some homogeneous ice nucleation at such high humidity values. Therefore: How realistic are the high RHi values? Please explain.

Page 14, line 11-12: Why did contrails form only between 11 km and 13 km altitude? Is this because there was no traffic below and above, or because of drier and warmer air above and below?

Page 15, line 2-4. Again you compare to LES only. You could as well compare to observations. Such data are readily available from Schumann et al. (2017).

If you would have plotted the ice particle concentration per volume along a line though the contrail cirrus clouds, you could have compared to the findings in, e.g., Voigt et al.(2017), Fig. 6.

Page 15, line 16: The last sentence of section 4.2 appears trivial. If there is no traffic, there is no chance to affect cirrus that moves in from upstream. That would be different

if you could show that a cirrus parcel which you follow in a Lagrangian manner and that did contain contrails for some time recovered and approached the properties of natural cirrus in a short time after the period with traffic. I think, you cannot show this from this study. So, this sentence should probably be eliminated.

Section 4.3 is on low technical level. It is not even clear from which satellite and sensor the data are taken. What is spatial and temporal resolution of the data? Which spectral channels are used? How sensitive are the observation results to the processing methods used? This needs improvements.

Page 19, Fig. 11: It took me some time to find the red circle. Please add the coordinates (12°E, 53°N) in the figure caption.

Page 22, line 11: What is a “nominal capacity”?

Page 22: Section 4.5: This section depends on the accuracy of the model used (with about 2 km horizontal and 300 m (or 400 m at the tropopause?) vertical resolution) since the early contrail ice crystal loss certainly depends on the time scale of plume mixing. This should be mentioned.

Page 23 line 10-12. The numbers given depend on temperature. See Fig. 5 in Schumann et al. (2017) and Fig. 6 in Voigt et al. (2017), and many other related studies. In fact, this should have been discussed earlier in the text. In the conclusion, it should be said that the numbers for IWC and other contrail-cirrus properties are valid for the specific meteorological situation considered.

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