Dear Mr. Schumann,

We thank a lot for your valuable comments and suggestions. We followed them as explained below.

The reviewers comments are repeated in **bold letters,** our replies are given in *italic,* and text modified or added to the manuscript is given in blue.

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.

The authors present a new contrail cirrus parameterization within the COSMO-ART model and study the impact of contrail cirrus on the SW radiation. The work is different to existing studies since they implemented their parameterization in a weather forecasting model. This would actually enable the authors to compare their results to observations but they do not do that. Instead they discuss the development t of a contrail field in simulations only. Nevertheless, I find the model itself and the impact on the SW radiation interesting. There are a couple of major issues that need clarification and/or sorting out. The paper needs to be expanded regarding a more detailed evaluation with observations and a better comparison with earlier work.

Dear Mr. Schumann,

Thanks a lot for your interest in our work and your detailed review. The issues raised in this review as well as the many questions and explanations clearly help to improve this manuscript. We hope to have addressed them to a satisfactory extent.

1. Page 1, line 11: insert ”and humid” after “hot”.

Done

2. Page 1, 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).

We change in page 1, line 13:

Following the Schmidt-Appleman-Criterion (Schmidt, 1941; Appleman, 1953; Schumann, 1996), contrails form only when the ambient temperature is below a threshold temperature of roughly -45°C. With a favorable state of the atmosphere, i.e. characterized by supersaturation with respect to ice, the originally line-shaped contrails undergo various physical processes at the micro scale…

to:

Contrails form in case, the Schmidt-Appleman-Criterion (Schmidt, 1941; Appleman, 1953; Schumann, 1996) is fulfilled, i.e. the ambient temperature is below a threshold of around -45°C. With plume temperatures near -38°C to -40°C, contrail particles, which are formed in the liquid phase initially, freeze homogeneously and quickly to form ice crystals. Those ice crystals grow in air with relative humidity above ice saturation and contrails persist. With such a favorable state of the atmosphere, the originally line-shaped contrails undergo various physical processes at the micro scale …

3. Page 1, 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.

We add in page 1, line 17:

Other examples of long-lifetime contrail observation are summarized by Schumann and Heymsfield (2017).

4. Page 1, 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).

Even taking into account the newer observations you mention we still believe that our statement is reasonable.

5. 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.

We weaken the statement and mention the study of Duda later in the manuscript as it also uses real flight data. However, we do not consider the contrail advection tool of Duda to be a "full" contrail model, in the sense that ice microphysics is included. Hence, our aforementioned claim is not invalidated by disregarding the Duda work.

We add in page 2, line 18:

Another approach using commercial flight data to study contrails on a regional scale is described in Duda et al. (2004). Here, a combination of commercial flight data and coincident meteorological satellite remote sensing data was used to perform a case study of a widespread contrail cluster.

Reference:

Duda, D. P., Minnis, P., Nyuyen, L., and Palikonda, R.: A case study of the development of contrail clusters over the Great Lakes, J. Atmos. Sci., 61, 1132 – 1146, 2004.

6. 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).

We changed the text accordingly:

There, a mixed Lagrangian-Eulerian approach is used instead of the usual Eulerian treatment. This approach allows covering the scale ranging from thousands of single contrails to multi-year global climate simulations (Schumann (2012); Schumann et al. (2015); Caiazzo et al. (2017)).

7. 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 in situ- 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.
We now cite several more studies of contrail observations.
We do not think that the presentation or plausibility issues are overly biased towards LES, but it is clear that LES results build an integral part of the contrail initialisation used in our model.
At the time the manuscript was prepared, ML-Cirrus results were not published yet and it is not clear whether we would have been granted access at that phase to all the data needed to make conclusive comparisons.
We add the following after page 3, line 15:
In the future, further case studies should be performed for which in-situ observations of natural ice clouds and especially contrails are available.

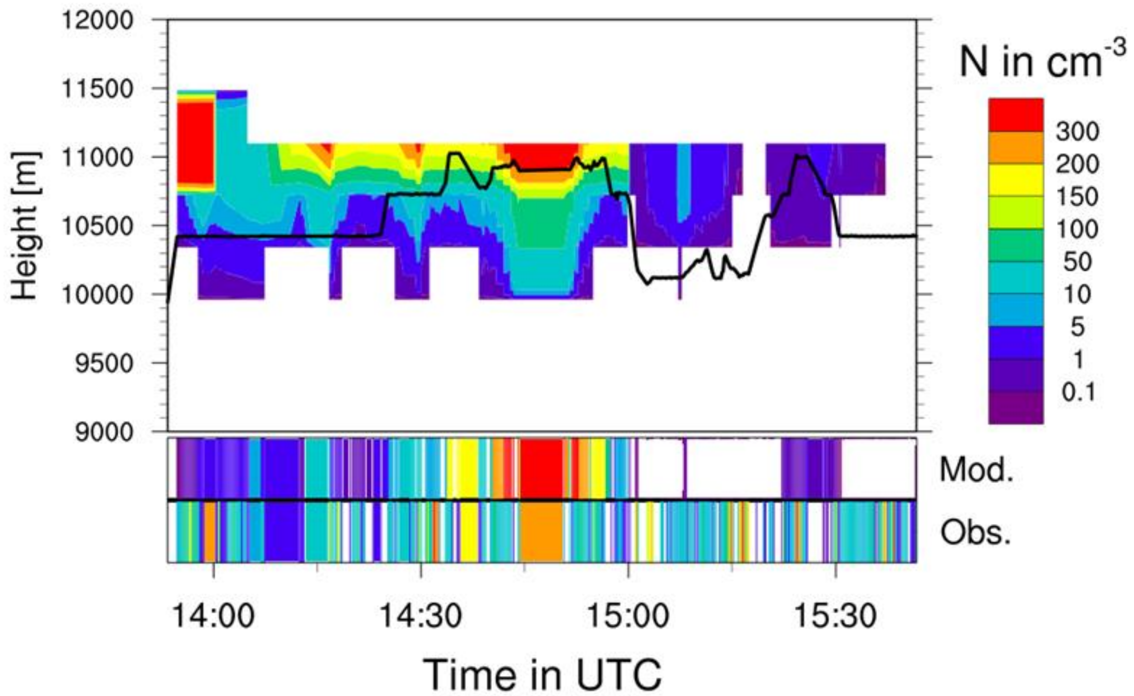

Fig. 1: 2014/04/10: Comparison of COSMO-ART ice crystal number concentration (contour) to SID3 measurements (ML-CIRRUS).

We also performed a case study for 10 April, 2014 to compare with ML-CIRRUS data (Fig.1). In the future, we want to extend this work and present it in a separate study.

8. 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.
We agree that the mentioned section is more technical than others. We condensed this part and moved the technical issues to the appendix.

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

10. 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?
In fact, this threshold is used for grid-scale ice clouds in the radiation scheme. For reasons of consistency (and as we find no other suitable study to justify another number) we leave it as it is. Indeed, the results do not seem to depend strongly on this threshold, unless it is changed to very large values (> 1.0e-6). Then, the contrail ice becomes mostly" invisible" to the radiation scheme. Thus, only modification in the natural ice class is present.
We follow your suggestion and add after page 5, line 28:
The same threshold value is used in Seifert and Beheng (2006) for grid-scale natural ice clouds.

11. 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.
We are aware of this work and other (e. g. Fu et al. (2007)). Implementing a new description of cloud optical properties is a rather large effort, as much of the tuning of the COSMO model depends on the results of this part of the code. Currently, we are working on the cloud optical properties but this work will not be finished and ready to use in the near future.
We add the following after page 7, line 5:
Other parameterizations exist that can compute reliable values for optical properties of small ice crystals with sizes down to 0.2 µm (Bi and Yang, 2017). For future studies, using such an approach clearly could overcome the necessity of the threshold described above.

12. 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.
We change:
“In contrast to previously mentioned global modeling studies…”
to
In contrast to most of the previously mentioned global modeling studies

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

14. 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).
At a given time, certainly both, mass and number control optical properties. What we wanted to say is that optical properties of AGED contrails depend on the initial ice crystal number and not much on the initial ice mass. We expand the description to avoid misunderstandings.
We change:
“Microphysical properties of contrails depend a lot more on the number of ice crystals than on the ice mass after the vortex phase (Unterstrasser and Gierens, 2010b).“
to:

Microphysical properties of aged contrails depend a lot more on the number of ice crystals than on the ice mass after the vortex phase (Unterstrasser and Gierens, 2010b). The initial ice mass is of minor importance, as the later growth of contrail ice crystals and the related ice mass evolution in a persistent contrail is mainly controlled by the ambient water vapor supply .On the other hand, the ice crystal number changes only slowly in a long-living contrail. Hence, its initial value determines the typical ice crystal sizes in the evolving contrail-cirrus (for a given environmentally controlled ice mass), which affects the radiative properties and the sedimentation-related life cycle.

15. 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).
We change the limits to 100 m and 500 m.

16. 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).
We change:
Rather than statistical calculations for globally averaged fuel consumption, the basic data consist of exact flight trajectories over a limited area that are recorded from real time-based data (flightradar24.com, 2015).
To:
Rather than statistical calculations for globally averaged fuel consumption, or radar data, the basic data consist of traffic waypoint information over a limited area recorded from transponders on the plane (flightradar24.com, 2015).

17. 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.
We now mention the studies by Petzold and Heymsfield , additionally we included Poellot et al 1999 and Voigt et al 2017 at a later occasion.
We change page 11, line 10:
"Consistently, large-eddy simulation studies indicate the strongest growth at the edges of a contrail (Unterstrasser and Gierens, 2010a; Lewellen et al., 2014)."
to:
Consistently, large-eddy simulation studies (Unterstrasser and Gierens, 2010a; Lewellen et al., 2014) and in-situ measurements (Petzold et al., 1997; Heymsfield et al., 1998) indicate the strongest growth at the edges of a contrail.

We change page 13, line 1:
"The effective radii in the aviation simulation are typically one order of magnitude smaller than in natural cirrus clouds and do not exceed 10 µm. These results are in reasonable agreement with large-eddy studies (Unterstrasser and Gierens, 2010a; Lewellen et al., 2014)."
to:

The effective radii in the aviation simulation are typically one order of magnitude smaller than in natural cirrus clouds and do not exceed 10 µm. These results are in agreement with large-eddy studies (Unterstrasser and Gierens, 2010a; Lewellen et al., 2014) and in situ observations (Poellot et al., 1999).

For comparison with Voigt et al. (2017), see answer to Comment 20.

We think that the paragraph on observability gives useful indications on how to interpret the COSMO results and hence we leave this section as is. The given reference (Unterstrasser et al, 2017) discusses this issue in more detail and the interested reader can read it.
A few thoughts on observability and your mentioned examples:
Lewellen et al (2014) contrast the spatial distribution of a passive chemical tracer and contrail-cirrus. Clearly, contrail area and plume area evolve differently over time, as ice crystals sediment as opposed to a passive tracer. Hence, exhaust trace gas measurements cannot be used to identify the complete contrail-cirrus. This method only allows tracking the contrail regions around the formation altitude where small ice crystals reside which have not yet fallen out of the aircraft plume.

References:
Heymsfield, A. J., Lawson, R. P., and Sachse, G. W.: Growth of ice crystals in a precipitating contrail, Geophys. Res. Lett., 25, 114 013, 1998.
doi:10.1029/98GL00189

Lewellen, D., Meza, O., and Huebsch, W.; Persistent Contrails and Contrail Cirrus. Part I: Large-Eddy Simulations from Inception to Demise, J. Atmos. Sci., 71, 4399 – 4419, 2014.
doi: 10.1175/JAS-D-13-0316.1

Petzold, A., Busen, R., Schröder, F. P., Baumann, R., Kuhn, M., Ström, J., Hagen, D. E., Whitefield, P. D., Baumgardner, D., Arnold, F., Borrmann, S., and Schumann, U.: Near-field measurements on contrail properties from fuels with different sulfur content, J. Geophys. Res., 102, 114 013 1997.
doi:10.1029/97JD02209

Poellot, M. R., Arnott, W. P., and Hallett, J.: In situ observations of contrail microphysics and implications for their radiative impact, J. Geophys. Res., 104, 12 077–12 084, 1999.
doi:10.1029/1999JD900109, http://dx.doi.org/10.1029/1999JD900109

18. 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.
We changed the color coding as well as the scale in Fig. 5 . The upper bound is now 1.4 instead of 1.8. The maximum value for RHi is 1.34.

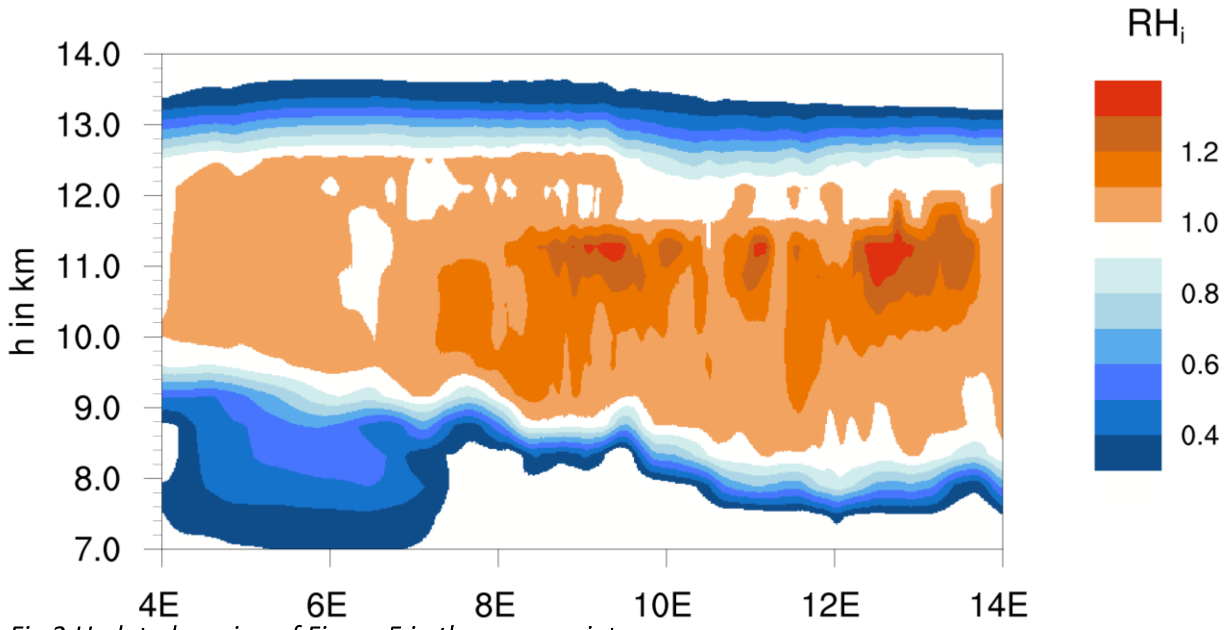

Fig 2.Updated version of Figure 5 in the manuscript.

Furthermore, we extend the discussion about RHi, adding the following figure:

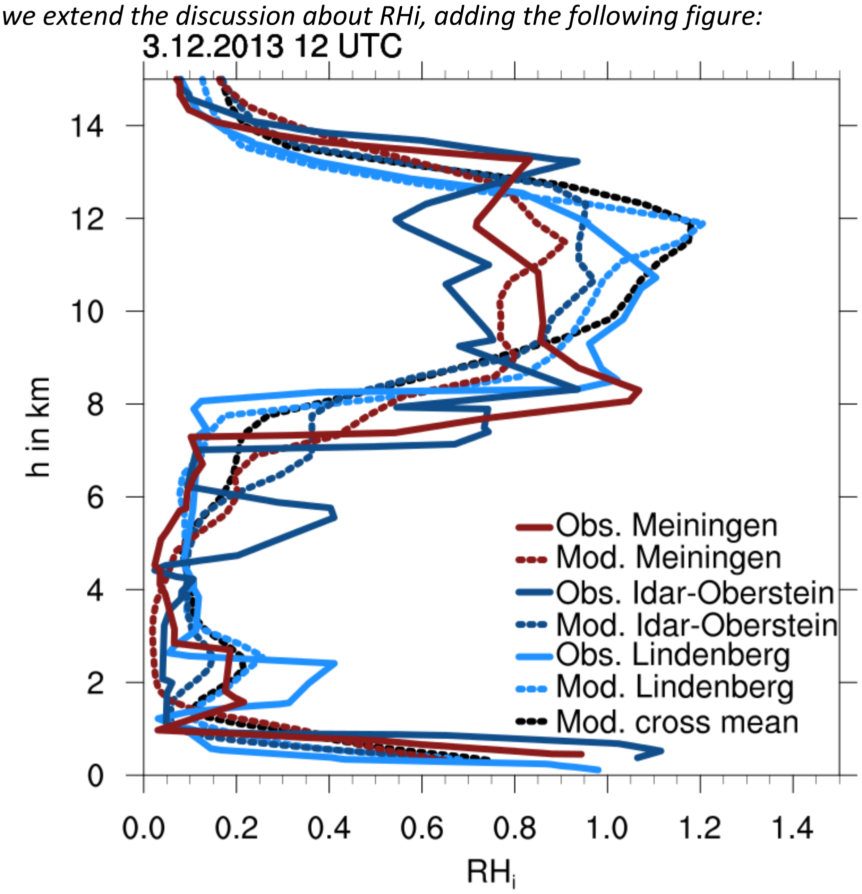

Figure 6. Radio soundings for several locations (solid lines) (UWYO, 2018) and corresponding profiles from aviation simulation (dotted lines) of relative humidity RHi. The black dotted line are mean values along the black line in Fig. 4.

The fidelity of such high values of RHi is corroborated by a comparison with observations. Here, vertical profiles obtained from radio soundings (UWYO, 2018) and simulated data evaluated at radiosonde stations are depicted in Fig. 6. In general, high values of RHi are observed by radio soundings, even supersaturation occurs over Lindenberg and Meiningen. The model clearly overestimates RHi for Idar-Oberstein, however Meiningen and Lindenberg agree quite well. Additionally, the black curve shows the mean vertical profile of the cross section displayed in Fig. 5. From this it becomes apparent that the relative humidity in the displayed cross section is remarkably high compared to the radiosonde locations. The large layer of supersaturation is caused by lifting and radiative cooling. It can persist, as natural cirrus clouds are located mostly below this layer (Fig. 7b). Also, the cirrus clouds present in the layer are too thin, i. e. occurring number concentrations are too low (Fig. 7e) to effectively reduce supersaturation.

Reference:
UWYO: http://weather.uwyo.edu/upperair/sounding.html, 01.02.2018.

Acknowledgements:
We acknowledge the use of the radio sounding data provided by the University of Wyoming, Department of Atmospheric Science.

19. 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?

We add in page 17, line 6:

This is caused by the absence of air traffic below this layer.

20. 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.

We follow your suggestion and add the following figure and description:

In Fig. 8, the relative occurrence of ice crystal number concentrations and temperature for the cross section shown in Fig. 7 is depicted. The relative occurrence is normalized with the sum over all values. Both, reference simulation (Fig. 8a) and aviation simulation (Fig. 8b) are similar for higher temperatures (i. e. lower heights) up to 220 K. For lower temperatures, high number concentrations up to 7 cm^{-3} occur in the aviation simulation, whereas number concentrations clearly decrease strongly with temperature in the reference simulation. Here, a rough comparison to measurement data can be made. In Voigt et al. (2017), mid-latitude cirrus clouds and contrails where probed in-situ during an aircraft measurement campaign. Comparing their Fig. 6(b) to Fig. 8, a similar increase in n below temperatures of about 220 K is found. Therefore, most likely, the high values occurring in the aviation simulation and not in the reference simulation, are due to aviation induced clouds.

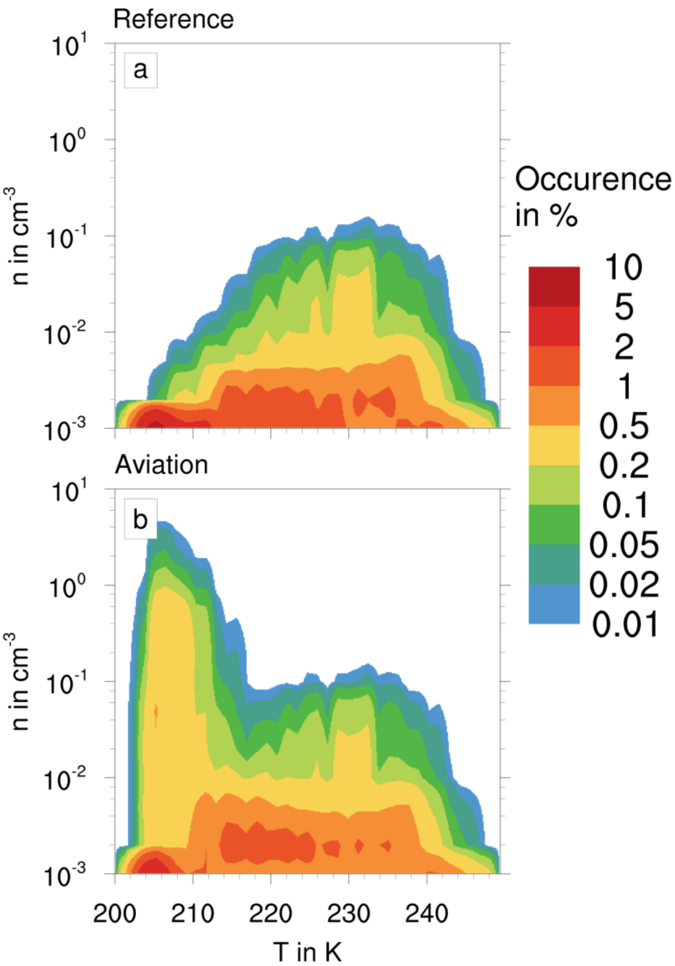

Figure 8. Relative occurrence of ice crystal number concentration versus temperature for natural cirrus, contrail and contrail cirrus at 12 UTC for the cross section along the black line in Fig. 4; a) reference simulation; b) aviation simulation.

Reference:

Voigt, C., Schumann, U., Minikin, A., Abdelmonem, A., Afchine, A., Borrmann, S., Boettcher, M., Buchholz, B., Bugliaro, L., Costa, A., Curtius, J., Dollner, M., Dörnbrack, A., Dreiling, V., Ebert, V., Ehrlich, A., Fix, A., Forster, L., Frank, F., Fütterer, D., Giez, A., Graf, K., Grooß, J., Groß, S., Heimerl, K., Heinold, B., Hüneke, T., Järvinen, E., Jurkat, T., Kaufmann, S., Kenntner, M., Klingebiel, M., Klimach, T., Kohl, R., Krämer, M., Krisna, T., Luebke, A., Mayer, B., Mertes, S., Molleker, S., Petzold, A., Pfeilsticker, K., Port, M., Rapp, M., Reutter, P., Rolf, C., Rose, D., Sauer, D., Schäfler, A., Schlage, R., Schnaiter, M., Schneider, J., Spelten, N., Spichtinger, P., Stock, P., Walser, A., Weigel, R., Weinzierl, B., Wendisch, M., Werner, F., Wernli, H., Wirth, M., Zahn, A., Ziereis, H., and Zöger, M.: ML-CIRRUS: The Airborne Experiment on Natural Cirrus and Contrail Cirrus with the High-Altitude Long-Range Research Aircraft HALO, Bull. Amer. Meteor. Soc., 98, 271 – 288, 2017. doi:10.1175/BAMS-D-15-00213.1

21. 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.

We delete the corresponding sentence.

22. 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.

We rewrite section 4.3 and use other satellite images with much better resolution and more precise information about the algorithm.

We replace Fig 8 (now Fig. 10) with the following:

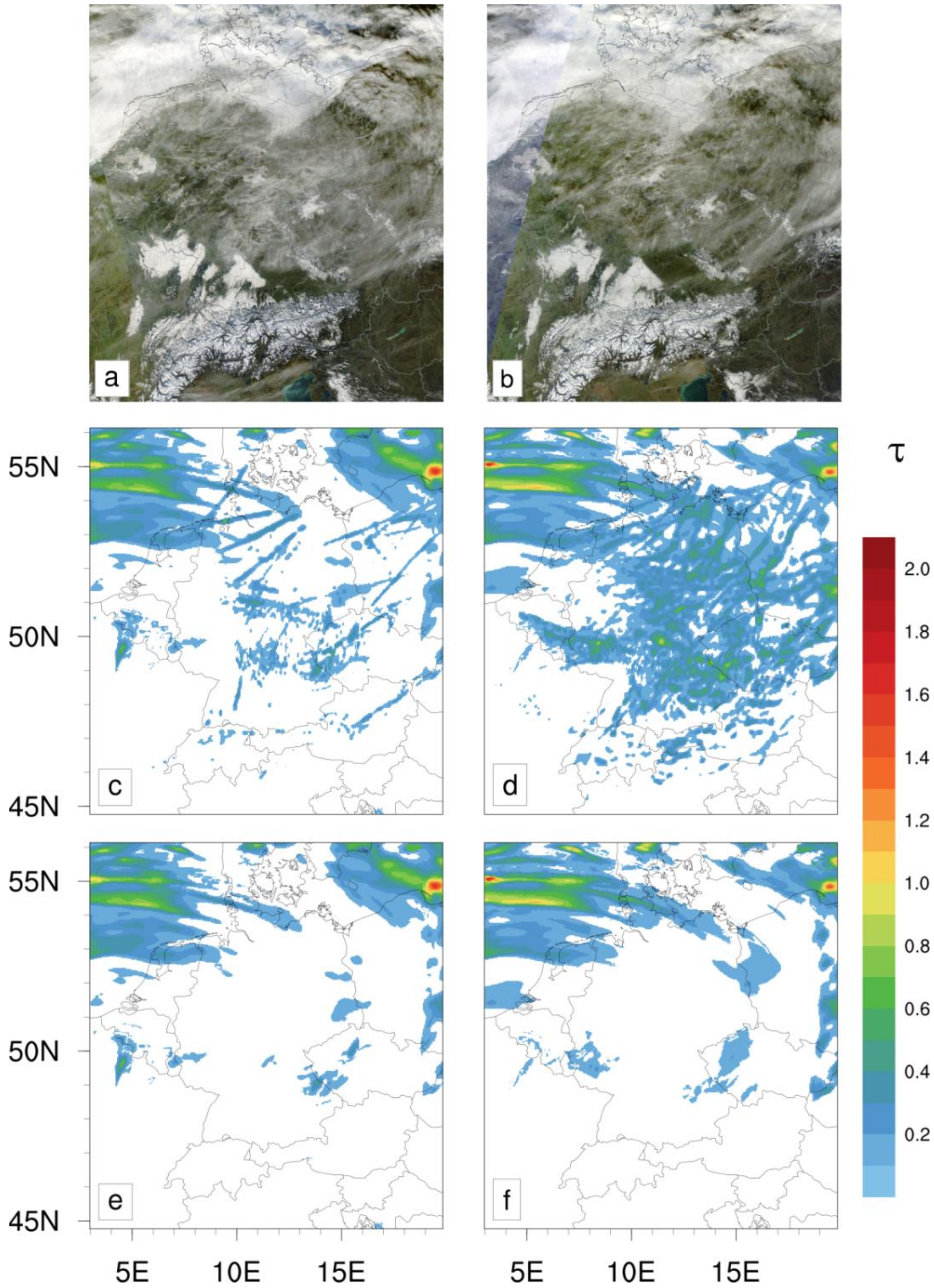

Figure 10. Top row: satellite image (MODIS True Color - Corrected Reflectance) (NASA/GSFC/ESDIS, 2018); center row: optical depth at 1.115 µm of all ice clouds for the aviation simulation; bottom row: optical depth of all ice clouds for the reference simulation; left column: 10 UTC; right column: 12 UTC.

In the following, satellite images (created with Global Imagery Browse Services (GIBS) NASA/GSFC/ESDIS, 2018) are shown in Fig. 10 for a qualitative assessment of the simulations. The panels a and b show the "MODIS Terra Corrected Reflectance True Color" at 10 UTC and 12 UTC for the simulated day, respectively, both with a resolution of 250 m. The "True Color" composition consists of MODIS bands 1, 4 and 3 (NASA/GSFC/ESDIS, 2018). Beside a cloud bank over the North and Baltic Seas and fog over Southern and Western Germany, a considerable number of line-shaped contrails and diffuse cirrus clouds are present across both satellite images. Main contrail clusters are found over Central Germany for both situations. Contrails can also be identified over the Netherlands and Belgium, over the Czech Republic, and south of the Alps. At 12 UTC, the rather widespread high-level cloud cover seems to have decreased compared to 10 UTC.
Comparing Fig. 10a and Fig. 10e, obviously, the reference simulation underestimates the coverage of high clouds in the center of the domain, which, in large parts, consists of contrails and contrail cirrus Fig. 10c. Clearly, the amount of cloud cover seems to be underestimated also in the aviation simulation at 10 UTC. This discrepancy is probably due to the fact that air traffic was switched on at 08 UTC and earlier flight movements are disregarded in our simulation. Hence, the simulation evaluation at 10 UTC neglects all contrails older than 2 hours. The comparison at 12 UTC is more favorable and observations match much better with the aviation simulation than with the reference simulation.
…

Acknowledgements:
We acknowledge the use of imagery from the Land Atmosphere Near real-time Capability for EOS (LANCE) system and services from the Global Imagery Browse Services (GIBS), both operated by the NASA/GSFC/Earth Science Data and Information System (ESDIS, \url{http://earthdata.nasa.gov}) with funding provided by NASA/HQ.

23. 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.
Done

24. Page 22, line 11: What is a "nominal capacity"?
The nominal capacity (or nominal power) is the power that an electrical device can produce (or handle) when operating in a reasonable manner as proposed by the manufacturer.
We change the paragraph from:
A nominal capacity is assumed, consisting of a specific PV module and PV inverter combination. The tilt and orientation of the PV module as well as the technical specifications are taken from Rieger et al. (2017).
to:
For a specific combination of a PV module and a PV inverter combination, a nominal power and a reasonable tilt is assumed. These technical specifications are taken from Rieger et al. (2017).

25. 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.
The early contrail ice crystal loss is part of the initialization in COMSO-ART which is based on LES results. By setting lambda_Ns = 1 we simply disregard any losses that appear in a young contrail. This sensitivity test is not affected by COSMO grid resolution and plume mixing within COSMO.

26. 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.

We add in page 23, line 10:
For a single case study, it was shown, …

Additional references:

Bi, L., and P. Yang: Improved ice particle optical property simulations in the ultraviolet to far-infrared regime, J. Quant. Spectrosc. Radiat. Transf., 189, 228-237, doi:10.1016/j.jqsrt.2016.12.007, 2017.

Caiazzo, F., Agarwal, A., Speth, R. L., and Barrett, S. R. H.: Impact of biofuels on contrail warming, Environ. Res. Lett., 12, , doi:10.1088/1748-9326/aa893b, 2017.

Duda, D. P., Minnis, P., Nyuyen, L., and Palikonda, R.: A case study of the development of contrail clusters over the Great Lakes, J. Atmos. Sci., 61, 1132-1146, 2004.

Heymsfield, A. J., Lawson, R. P., and Sachse, G. W.: Growth of ice crystals in a precipitating contrail, Geophys. Res. Lett., 25, 1335-1338, doi: 10.1029/98GL00189, 1998.

Iwabuchi, H., Yang, P., Liou, K. N., and Minnis, P.: Physical and optical properties of persistent contrails: Climatology and interpretation, J. Geophys. Res., 117, D06215, doi:10.1029/2011JD017020, 2012.

Petzold, A., Busen, R., Schröder, F. P., Baumann, R., Kuhn, M., Ström, J., Hagen, D. E., Whitefield, P. D., Baumgardner, D., Arnold, F., Borrmann, S., and Schumann, U.: Near-field measurements on contrail properties from fuels with different sulfur content, J. Geophys. Res., 102, 29867-29880, doi: 10.1029/97JD02209, 1997.

Schumann, U., Penner, J. E., Chen, Y., Zhou, C., and Graf, K.: Dehydration effects from contrails in a coupled contrail-climate model, Atmos. Chem. Phys., 15, 11179-11199, doi:10.5194/acp-15-11179-2015, 2015.

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