Responses to anonymous reviewer #3

We thank all reviewers for their helpful advices and constructive comments about our paper. Their suggestions and criticism have led to a strongly revised and restructured version of our manuscript where we concentrated on two goals: (1) we develop a new method to identify microphysical properties of contrails, contrail cirrus and natural cirrus in the same meteorological conditions from in situ measurements, (2) radiative forcing of contrail cirrus and natural cirrus are derived by satellite observations based radiative transfer modeling in air traffic region favorable for contrails evolution. To this end, we have modified pictures and removed some of them, made the text more concise, added a supplement and wrote clearer explanations.

We thank the reviewer for his/her positive judgement on the manuscript and the helpful comments.

In the following we number the referee`s comments (RC) and reply (R) to them individually.

Summary of paper:

In this paper, the authors investigate an important problem, how to distinguish naturally formed cirrus from contrail cirrus. They use a set of HALO measurements from a flight in the ML-CIRRUS campaign to measure in-situ cirrus properties and gases. This is compared to SEVIRI observations and used combined with a radiative transfer model to estimate the radiative properties of the different cirrus types. This is difficult problem and one of interest to the readers of ACP. The authors have made a good attempt to address this problem, but I would suggest there are some aspects that should be improved before publication.

Main points:

RC1: The results on this work are based on three transects from a single flight. ML-CIRRUS flew through many contrails during the campaign, why is only this set chosen (and could the results/method be easily expanded to other flights?). It is noted that the control NO threshold varies, but is this simple to generalize? I don't think it has to be set manually.

R1a) author's response

We added the reason why only this set is chosen in the abstract, the introduction and Sect. 5 summary and conclusions as suggested by the 1st reviewer, to stress the motivation and significance of this case study. Essentially it was the "golden day" for contrail measurements during the ML-CIRRUS campaign, with predictable contrail conditions in the North Atlantic flight corridor NAF (Voigt et al., 2017, Fig. 4), and a persistent contrail cirrus situation over the North Atlantic region NAR with a blue ocean as background for better sensitivity of the satellite measurements. The contrail coverage and radiative effects with high variability in NAR are important and have been studied in many studies. We took profit of this contrail cirrus outbreak and precious measured data in our paper to derive radiative effects combining in situ measurements and satellite observations. This methodology will be applied to other datasets for future research about the contrail climate impact. Changes in the text are given as follows. The results/method can be expanded to other flights since the background NO thresholds are determined dynamically provided the influence by lightning and wildfires can be excluded. The radiative transfer model simulations that accompany the airborne measurements can also be extended to other suitable campaign flights.

R1b) manuscript changes

L14-18: "On that day, high air traffic density in the NAR combined with large scale cold and humid ambient conditions favoured the formation of a contrail cirrus outbreak situation. In addition, low coverage by low-level water clouds and the homogeneous oceanic albedo increase the sensitivity to retrieve cirrus properties and their radiative effect from satellite remote sensing. This allowed to extend current knowledge on contrail cirrus by combining airborne in situ, lidar and satellite observations."

L469-473: "We choose this contrail cirrus outbreak case because of the large contrail cirrus coverage and high air traffic density. As flight operation in all altitudes is not easily granted due to the high air traffic load in the NAR, the data presented here is also rare and unique in the sense that HALO was able to operate and acquire in-flight measurements of contrail cirrus perpendicular to the flight tracks of the NAR. From satellite remote sensing, few low-level water clouds and the relatively homogeneous oceanic background increase the sensitivity to retrieve cirrus properties."

RC2: A related point, but a lot of the statistics are given in counts, but it is not clear what a count is? Is each one an individual contrail, on SEVIRI pixel, or a second of aircraft time? These will all give different results for the accuracy of any method.

R2a) author's response

A count in the statistics is an aircraft measurement with a frequency of 1 Hz.

R2b) manuscript changes

L331-332: "In total, from 08:30 to 11:30 UTC for each aircraft measurement with a frequency of 1 Hz we have classified 49 contrail observations…"

RC3: The authors spend a considerable amount of time looking at Reff from CIPS. Looking at Strandgren et al (AMT, 2017), it doesn't appear that Reff is validated in that paper. In addition, the comparison to HALO Ref values (Fig. 8c) makes it look like CiPS doesn't have the variability to represent Reff. Does CiPS have the capability (or information) to retrieve Reff?

R3a) author's response

We agree with the reviewer's comment. R_{eff} is calculated according to the concept of $R_{eff} = c[\frac{IWC}{\sigma}]$, where c is 1.64 for ice particles of any shape according to Foot (1988), σ is volume extinction coefficient, and IWC is ice water content. This relationship is used for CALIOP (Heymsfield et al., 2005), which is the data with which CiPS was trained, and has been extended to $R_{eff} = 1.64[\frac{IWP}{IOT}]$. However, the reviewer is right, R_{eff} is not validated in Strandgren et al. 2017a and our first aim was to validate it in this study for contrail cirrus (and the few contrails). Unfortunately, we mixed up results of our evaluation with validation results for R_{eff} from CiPS such that the reader was confused. Thus, to sharpen the red line of our study we decided to remove the R_{eff} analysis from CiPS from the current study.

Reference

Heymsfield, A. J., Winker, D., and van Zadelhoff, G.-J.: Extinction-ice water content-effective radius algorithms for CALIPSO, Geophys. Res. Lett., 32, L10807, 10.1029/2005GL022742, 2005.

Foot, J. S.: Some observations of the optical properties of clouds. Part II: Cirrus, Q. J. R. Meteorol. Soc., 114, 145–164, 1988.

RC4: I am unclear if the extent to which these contrails can or should be considered as temporal evolutions. Fig 8 suggests that they could be a temporal evolution, but around line 300, it is suggested otherwise.

R4a) author's response

Thanks for your consideration. Satellite measurements vary in time and space. To make it clear, we removed Fig 8 and the corresponding description about temporal evolutions of contrails in the text.

RC5: This is more of a style thing, but I found the text could be broken up more (into paragraphs for example) to help the reader. There are several cases were a paragraph spans most of a page (e.g P16), which is too long.

R5a) author's response

Thanks for the suggestions, we adjusted the text accordingly. Around the original L410, and also in all other sections we revised the text and made it more concise and clearer.

Minor points:

RC6: L21 - consistency in the ordering of the cloud types would be nice (perhaps throughout).

R6a) author's response

The order of the cloud types was revised in the throughout text in the sequence of contrails, contrail cirrus and natural cirrus if they exist.

RC7: L163 - This would suggest the CTH is biased towards returning 10km? Does this affect the results?

7a) author's response

Thanks for pointing to this feature. The bias of CTH toward 10 km could lead to a less significant decreasing trend in Fig. 8(a). In Fig. 12, for radiative transfer modeling calculations CTH and CBH from lidar legs are used. These two figures were ultimately removed in the new manuscript version in order to focus and strengthen the manuscript. For Fig. 13, the effects of this bias were explained in original L704-706, "Notice however that the possible underestimation of CTH by CiPS in this area would result in the general underestimation of the LW RF results since a lower CTH reduces the contrast to the cirrus-free OLR. In turn, this would further shift cirrus net RF towards cooling."

RC8: L193 - I would not start a sentence with 'and'. Libradtran recommends this, I assume that is what you used?

R8a) author's response

The "and" has been removed.

RC9: L207 - Presumably this could be checked by looking at the contrail evolution in SEVIRI data

R9a) author's response

Thanks for pointing to this part. To clarify the time when contrails have formed, SEVERI RGBs are produced from 12 UTC of 25 March to 8 UTC of 26 March just before the HALO measurements. We confirmed that contrails identified in this study were induced at 3 UTC on 26 March (see the figures attached below). We removed the last part of the sentence accordingly.

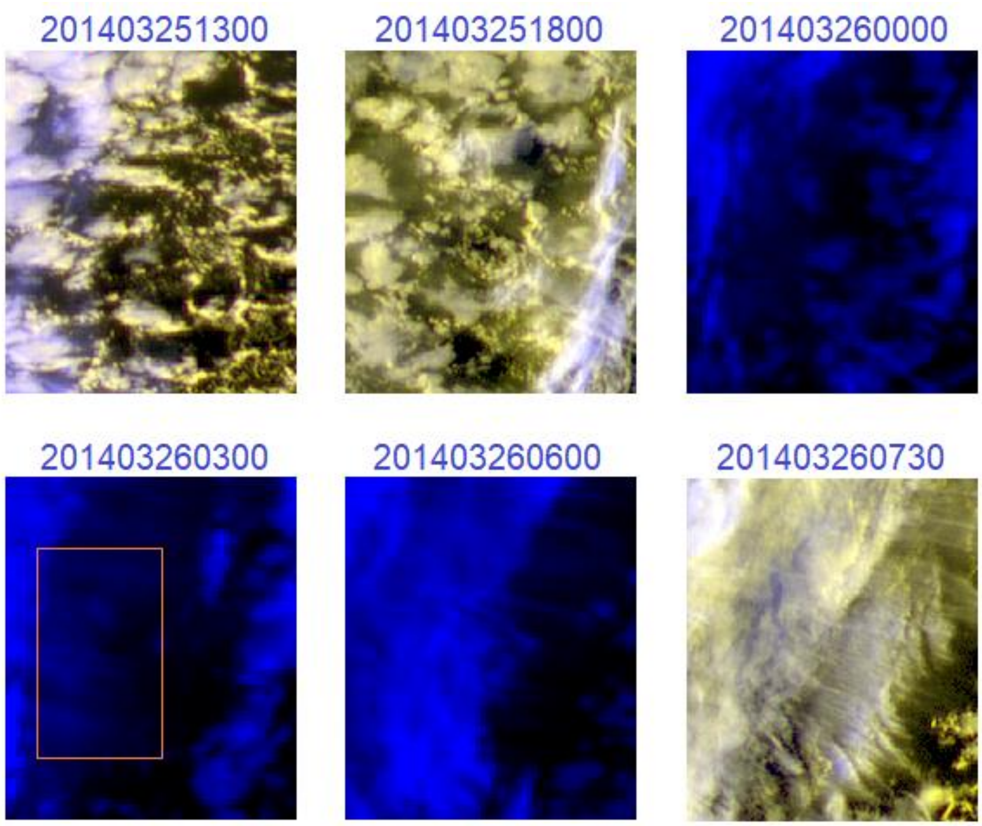

Figure A3: MSG/SEVRI RGB plots at a sequence of time on March 25 and 26 of 2014

R9b) manuscript changes

L209-211: "Considering that the peak of eastbound morning air traffic is approx. at 3 UTC (Graf and Schumann, 2012), under favourable conditions with low temperature and high humidity contrails induced from these aircraft are expected to form and live for hours such that they can be identified in MSG observations in the morning of the same day."

RC10: Fig 1. - This should indicate the study region. It is almost coincident with a MODIS overpass, which could be used for a high resolution check of the contrail properties.

R10a) author's response

We thank for your comments and plotted the flight path in Fig. 1 and adapted the caption to indicate the study region. As for a high resolution check of contrail properties using MODIS images, we have tested before that the MODIS overpass mismatches the HALO flight time between 8 UTC to 11:30 UTC on 26 March. The image at 10:40 and 10:45 UTC show the right edge of NAR. The other three time slots (12:20, 12:30, and 14:10) capture the contrails over the study region but with time difference.

R10b) manuscript changes

L216-217 (Caption): "Figure 1: (a) The false color RGB image from MSG/SEVIRI overlapped with the HALO flight track on 26 March 2014 at 10:45 UTC showing Europe and the Eastern part of the North Atlantic Ocean…"

RC11: L216 - What is the SEVIRI resolution at this location?

R11a) author's response

Approx. 3.5 km × 4.5 km sampling distance

R11b) manuscript changes

L219: "Due to its approx. 3.5 km × 4.5 km spatial resolution…"

RC12: L226 - The first use of NAR?

R12a) author's response

The first use of NAR in the main text is in L91.

RC13: L273 - Do these contrails line up with those observed in SEVIRI? That could give more confidence in the identification.

R13a) author's response

Yes, as suggested by the 2nd reviewer, a visual comparison between measured contrails from HALO instruments and observed ones in SEVIRI at 8:30, 9:30, 10:00, 10:30UTC replaced Fig.2 and a more in-depth analysis and discussion were included in Sect. 3.1, to stress the contrail observation from MSG/SEVIRI. However, as explained there the many lines that can be observed in the high resolution RGBs can be found only partially in the BTDs such that an automatic identification in the BTDs is not possible and a quantitative verification of the contrail locations in in situ and satellite data cannot be achieved.

RC14: L279 - I might have said the ice supersaturation was 'occasional' - the third flight has almost none (if I am reading Fig. 4 correctly).

R14a) author's response

Yes, this is true, especially in leg 3 where supersaturation is limited to small area inside the cloud. We adapted the text with 'occasional'.

RC15: L300 - I would make the temporal comparison (or lack of it) clear earlier (maybe in the flight description).

R15a) author's response

Yes, we updated Fig. 2 and extended the discussion about temporal collocation between HALO measured cirrus and satellite images in Sect. 3.1.

RC16: L306 - aircraft.

R16a) author's response

Updated in the whole main text.

RC17: L327 - Grammar. Also, is this expected? Could it be due to errors in the RH retrieval (or reanalysis)?

R17a) author's response

Thanks for indicating this grammar error. We corrected the sentence accordingly. The uncertainty of the RH retrieval from AIMS measurements is discussed in Sect. 2.1.

R17b) manuscript changes

L135: "…were used to convert water vapor concentration to RHi with an uncertainty of 10 % to 20 % (Kaufmann et al., 2018)."

Original L326-327: "Figure 5b also shows that over the entire flight path R_{eff} increases with RHi, as ice supersaturation supplies the water vapor for the growth of ice crystals, while subsaturated conditions lead**ing** to sublimation and evaporation." But ultimately the whole sentence was removed to shorten this version of manuscript.

Reference

Kaufmann, S., Voigt, C., Heller, R., Jurkat-Witschas, T., Krämer, M., Rolf, C., Zöger, M., Giez, A., Buchholz, B., Ebert, V., Thornberry, T., and Schumann, U.: Intercomparison of midlatitude tropospheric and lower-stratospheric water vapor measurements and comparison to ECMWF humidity data, Atmos. Chem. Phys., 18, 16729-16745, 10.5194/acp-18-16729-2018, 2018.

RC18: L333 - The previous sentence just noted that different aircraft might produce different NO amounts.

R18a) author's response

Yes, and it explained the reason why the ΔNO threshold could be generalized by using a dynamical NO background value.

RC19: Eq 1 - Using min would also include an impact of instrument noise. Have you thought about using a different measure, perhaps a statistic/algorithm that can remove outliers instead (e.g. RANSAC) for identifying the background?

R19a) author's response

We thank for your comments on a RANSAC algorithm to interpret outliers. We tested your suggested method and confirmed that all outliers correspond to the peaks of NO values, which stress the accuracy of our NO background identification. A supplementary explanation was added in the text.

R19b) manuscript changes

L311-312: "Notably, we use the RANSAC algorithm (Fischler and Bolles, 1981) to interpret NO outliers and confirm that they haven't hit the NO background but the peaks of NO values."

Reference

Fischler, M. A and Bolles, R. C.: Random Sample Consensus: A Paradigm for Model Fitting with Applications to Image Analysis and Automated Cartography. Comm. ACM. 24: 381–395. doi:10.1145/358669.358692, 1981.

RC20: Fig. 7 - I like the reduction in aspherical fraction in the contrail region, but is this a consistent effect, or just observed in one case?

R20a) author's response

Yes, it's a consistent effect that aspherical fraction reduces when encountering contrails. But finally, we remove Fig. 7 as it's far away from the main focus of the updated manuscript and because a more in-depths discussion would be needed which is out of the scope of this manuscript.

RC21: Fig. 9 - Given the retrieved Reff has an impact on the optical depth, does the lack of sensitivity to Reff also imply that CiPS is performing poorly when retrieving the IOT? That could potentially explain the difference in optical depths from the expected distribution?

R21a) author's response

The lack of sensitivity to R_{eff} will not influence the IOT retrieval in CiPS. In Sect. 2.2.1, we explained that CiPS consists of four artificial neural networks to detect cirrus with their transparency information and retrieves the corresponding CTH, IOT, and ice water path, respectively. R_{eff} is removed as it's not the direct output of CiPS but the calculations using IWP and IOT.

RC22: L468 - fast -> quickly.

R22a) author's response

Revised but ultimately the whole sentence was removed to shorten this version of manuscript.

RC23: L498 – north. Revised

R23a) author's response

Revised but ultimately the whole sentence was removed to shorten this version of manuscript.

RC24: L598 - derived how?

R24a) author's response

Thanks for pointing out this ambiguous description. R_{eff} profiles are derived using IWC and temperature from ERA5 according to the parameterization by McFarquhar et al. (2003) and Bugliaro et al. (2011, 2022). The equations are listed as follows.

"McFarquhar et al. (2003) is used which relates ice particle effective radius R_{eff} [µm] to ice water content IWC [kg/m3] and temperature T [K]:

$$b = -2.0 + 0.001\sqrt{273 - T}^3 \log((IWC/1000)/(50g/m^3))$$

$$r_0 = 377.4 + 203.3b + 37.91b^2 + 2.3696b^3$$

$$n_{ft} = (\sqrt{3} + 4)/(3\sqrt{3})$$

$$r_1 = r_0/n_{ft}$$

$$r_{eff} = (4\sqrt{3}/9)r_1$$

R24b) manuscript changes

L394-395: "For liquid clouds, the parameterization by Bugliaro et al. (2011, 2022) are applied for creating R_{eff} profiles using IWC and temperature from ERA5."

Reference

McFarquhar, G., Iacobellis, S., and Somerville, R.: SCM simulations of tropical ice clouds using observationally based parameterizations of microphysics, J. Climate, 16, 1643–1664, 2003.

RC25: L604 - I was initially skeptical of this, but looking further at CiPS, this doesn't seem so unreasonable. For readers unfamiliar with CiPS, you might want to note that the CiPS retrieval is only dependent on thermal IR channels (which makes it independent of the surface/low cloud properties).

R25a) author's response

Thanks for your kind understanding. I updated the sentence and emphasized that CiPS retrieval is only dependent on thermal channels.

R25b) manuscript changes

L398-399: "Since SEVIRI observations with CiPS are able to account for the entire cirrus cloud layers but are only dependent on thermal channels and not affected by low lying clouds..."

RC26: L618 - What is done for these situations? DO they occur often? Does it impact your results?

R26a) author's response

I see your points and formulated the argumentation. R_{eff} beyond the range of 5 to 60 µm are inexecutable in RTM calculations and not considered in the computations of radiative effects. 20 cases occur in total. It hasn't significant impacts on my results. As presented in updated Fig. 5, R_{eff} of natural cirrus and contrails always fall in the range where RTM could simulate.

R26b) manuscript changes

L414-416: "20 cases in total are removed but have a negligible effect on the estimation of radiative effects as R_{eff} of natural cirrus and contrail cirrus always fall in the range where RTM could simulate as indicated in Fig.5."

RC27: L635 - Is this likely? Perhaps some indication of windspeed at this time would be useful?

R27a) author's response

We thank for your significant advice and re-compute the simulations along the HALO flight track with windspeed from ERA5 as RTM inputs following Cox and Munk (1954a, b) and Nakajima and Tanaka (1983). The Figure A4 in this answer is the updated Fig.6 with the changes of corresponding sentences in the text.

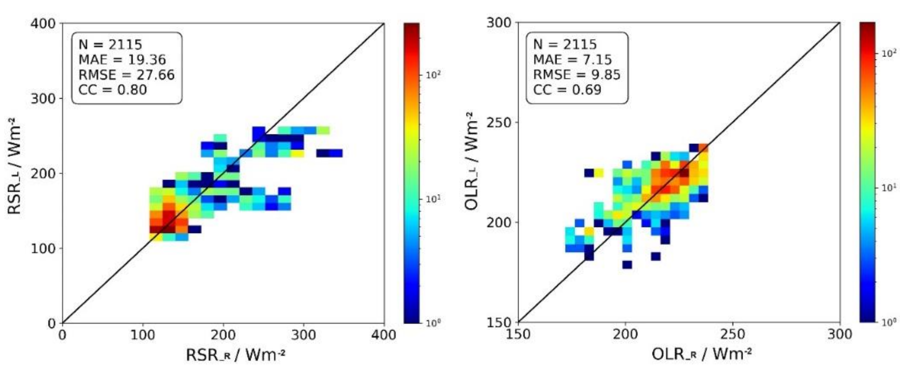

Figure A4: Comparison of TOA (a) RSR and (b) OLR from our RTM simulations (RSR_L, OLR_L) for probed ice particles and RRUMS algorithm results (RSR_R, OLR_R) for single SEVIRI pixel along the HALO flight on 26 March 2014. The mean absolute error (MAE), root mean square error (RMSE) and correlation coefficient (CC) are used as metrics.

R27b) manuscript changes

L396-397: "Besides, the albedo of ocean is parameterized following Cox and Munk (1954a, b) and Nakajima and Tanaka (1983), especially involving the wind speed from ERA5."

L430-432: "Furthermore, a smaller overestimation of RSR by the RTM compared to RRUMS is also observed for the smallest RSR values below 150 W m^{-2}, related to the bias of estimated ocean albedo but improved by the application of wind speed."

Reference

Cox, C. and Munk, W.: Measurement of the roughness of the sea surface from photographs of the sun's glitter, J. Opt. Soc. USA, 44, 838–850, 1954a.

Cox, C. and Munk, W.: Statistics of the sea surface derived from sun glitter, J. Marine Res., 13, 198–227, 1954b.

Nakajima, T. and Tanaka, M.: Effect of wind-generated waves on the transfer of solar radiation in the atmosphere-ocean system, J. Quant. Spectrosc. Radiat. Transfer, 29, 521–537, 1983.

RC28: L643 - I don't understand this measure of uncertainty or how it is applied here.

R28a) author's response

Thanks for pointing to this incorrect expression. A brief explanation ($RMSE_{RRUMS\_G}$/ mean (RSR_{RRUMS_G})) was added in the text. But ultimately this measure was removed as it's far away from the main focus of the revised version of the manuscript.

R28b) manuscript changes

Original L643-645: "We consider the ratio of the RMSE value of RSR from RRUMS against GERB ($RMSE_{RRUMS\_G}$/ mean (RSR_{RRUMS_G})) (Sect. 4.1) divided by the mean RRUMS RSR (ratio=0.19) as a measure for the uncertainty of RRUMS and neglect all RTM simulations that differ by more than this fraction from RRUMS." But ultimately the whole sentence was removed to shorten this version of manuscript.

RC29: Fig. 13c - Is this vertical velocity relevant? Can ERA5 simulate the cirrus vertical velocities at the small scale required for ice processes?

R29a) author's response

ERA5 might miss small scale variability but can give information about larger scale air mass motions that affect for instance relative humidity and temperature. Furthermore, it influences the macrophysical cloud properties for example CTH. Ultimately Fig. 7c was removed in the revised version of manuscript. Adapts to the text is as follows.

R29b) manuscript changes

L454-456: "The positive vertical velocity from ERA5 around that region implies the local downward motion of airmass to warmer temperature layers and the CTH also decreases."