Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC3
https://doi.org/10.5194/acp-2022-537-RC3, 2022

Comment on acp-2022-537

Anonymous Referee #3

Referee comment on "Observations of microphysical properties and radiative effects of a contrail cirrus outbreak over the North Atlantic" by Ziming Wang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-537-RC3, 2022

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

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 generalise? I don't think it has to be set manually.

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.

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?

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.

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.

Minor points

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

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

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

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

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.

L216 - What is the SEVIRI resolution at this location?

L226 - The first use of NAR?

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

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

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

L306 - aircraft

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

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

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?

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?

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?

L468 - fast -> quickly

L498 - north

L598 - derived how?

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).

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

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

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

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