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Comment on acp-2022-537

Anonymous Referee #2

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-RC2>, 2022

Summary of paper:

The authors use a combination of aircraft and satellite measurements, and radiative transfer modeling to analyze a band of thin ice cloud in the North Atlantic air corridor on 26 Mar 2014. (Young) contrails, contrail cirrus, and natural cirrus within the cloud layer are distinguished by in situ measurements of ice particle number concentration and NO gas concentration. The optical thickness, effective radius, and radiative forcing are computed for each cloud type within the cloud band.

General comments:

The goals of the paper are scientifically important and worthy of study, but the authors do not characterize the three cloud types in a convincing manner. The cloud types are defined from in situ measurements, but the authors appear to conflate individual contrail properties to the entire layer at the point of observation. It is not clear what distinguishes a (young) contrail from contrail cirrus, even without the context of the overall cloud band. Several if not most of the contrails appear to be at least two hours old and would likely be visible in the satellite imagery, yet I could find no attempt by the authors to use MSG/SEVIRI satellite observations to classify (or determine the history of) any possible contrail cirrus cloud. In fact, the authors seem to claim that such a distinction is not possible, with an example of an ambiguous contrail encounter between 0843 and 0845 UT to demonstrate the current difficulties in discriminating between young contrails and contrail cirrus. Thus, it seems as though the separation of the cloud observations into different types is essentially meaningless. Add on top of that the difficulties in assigning the properties of individual contrails to the entire cloud layer, the overall usefulness of classifying different points of the cloud as contrail, contrail cirrus, and cirrus is minimal.

Although it is clear that new and better definitions of aviation-induced and -influenced ice clouds are necessary, I'm not sure how the authors can proceed to strengthen the paper. Perhaps a more careful study of the numbers and ages of the contrails within a layer may allow for a more useful definition of how much a cirrus layer is influenced by aviation.

Specific comments:

The exposition of the research in the paper is not always easy to follow. For example, it is hard to see the details of the flight path in Figure 2, especially in the blue lines in the lefthand RGB-composite images. The lack of clarity makes it difficult to compare the flight path to the lidar data from Figures 3 and 4. Crucially, the authors never directly inform the reader about the flight path details (including three lidar legs and three in-situ legs) until Figure 6, leading to much confusion for the reader in Section 3.2. Several of the following comments highlight similar difficult-to-follow text.

Line 235 (Figure 2): It is suggested here, but not entirely clear, but have the blue flight segments in Figure 2 been adjusted to account of the 12 minute difference between the nominal satellite time and the actual time of the image acquisition?

Section 3.1: The peaks in backscatter during Leg 2 look like individual contrails. It is not clear how the lidar observations compare with the HALO aircraft flight path. Figure 2 suggests that most of the flight legs are perpendicular to the NAR corridor traffic but some legs around 0800 are parallel to NAR corridor traffic. What direction is HALO flying relative to NAR corridor during Legs 1, 2 and 3 in Figure 3?

Line 311: "properties collected during the three legs." Which three legs? The legs described in Figures 3 and 4? Don't the authors state that those are WALES measurement legs and thus "can neither be directly inter-compared nor directly compared to in situ observations taken in between"? How is the reader to know that Figure 5 possible, unless the authors tell the reader beforehand that there are 3 lidar legs and 3 in-situ legs?

Figure 6: The reader cannot discern any (young) contrails (blue color) in Figure 6d. I suggest this be removed from the figure. Lines 404 through 406 state that only 1 percent of the observations are (young) contrails.

Line 400: The discussion about number concentration (N) at this point appears muddled.

"Ncas occurrences decrease by more than 2-3 orders of magnitude from 0.03 to 0.78-0.84 cm^{-3} ." Shouldn't this read occurrences increase by more than 2-3 orders of magnitude"?

Section 3.2.1: The discussion in this section implies that most of the contrail cirrus observations are from contrails at least 2 h old. How old are the (young) contrails estimated to be?

Lines 407-409: "We finally remark that MSG/SEVIRI satellite observations are left unused for this classification since the distinction between contrail cirrus and natural cirrus from satellite observations is inherently difficult due to the typical characteristics of young contrails - large N - cannot be measured by passive sensors." This statement conflates (young) contrails with contrail cirrus, and would thus make all of the previous discussion from Table 1 classifying each cloud type meaningless.

Section 3.2.2: This section is poorly worded and misleading. We are not simply looking at the temporal evolution of cloud properties, but variables changing in time and space. The following sentences explain that and thus contradict the beginning sentence. The description of how the SEVIRI measurements are classified according to the HALO observations is a bit unclear. Given that the SEVIRI and HALO measurements might be displaced by as much as 7.5 min, and the total time of the (young) contrail observations is around 110 s (1 percent of 3 h), it is not surprising that the Reff measurements between HALO and CiPS are not correlated, and that no significant difference between the IOT between contrail and contrail cirrus was found. Even without the fall streaks, it seems unlikely that the properties of individual (young) contrails can be determined from the SEVIRI data generally.

Lines 453: Most estimates of contrail optical thickness from polar orbiting IR sensors are from clouds at least 2 h old, and thus may not be the young contrails that are implied here. The estimated age of the contrails is not mentioned until line 476 after much discussion about IOT estimates of "contrails". The terms contrail and contrail cirrus are being mixed together and it is unclear what the authors are talking about in this section.

Lines 478-479: "From the point of view of optical thickness, the entire cloud seems to be homogeneous without remarkable differences among the cloud types defined in Sect. 3." This statement reinforces the overall lack of utility of the cloud types.

Figure 8: The caption in this figure is not helpful. The cloud properties measured by CiPS are necessarily "at SEVIRI spatial resolution" while the Reff measured by HALO are concurrent and collocated aircraft measurements.

Line 437 (Figure 8): What times is HALO in the northern part of the race track? Why make

the reader determine these times on their own from Figure 6, but not the southern part of the race track?

Lines 528-529: "Finally, we jointly assess radii variations of natural cirrus and adjacent contrail cirrus from CiPS with simultaneous HALO measurements for this NAR case." Please tell the reader that this section refers to Figure 8.

Lines 535: "The temporal variability of CiPS Reff along the flight path..." The authors again appear to neglect that even satellite measurements are functions of both time and space. Simply say "The variability of CiPS Reff...". Also, say "than that of collocated in situ Reff" instead of "than that of simultaneous in situ Reff". If the data are averaged over the MSG/SEVERI pixels, they can't be simultaneous. One quantity is time averaged while the other is not.

Section 4.4 and Figure 13: Why include this section? Isn't this redundant because the authors have already compared collocated satellite and HALO observations? Why are the various regional quantities computed until 18 UT when only HALO and SEVIRI observations from 0830 to 1230 UT were presented earlier in the paper? Why does a positive vertical velocity imply "the local downward motion of air mass to warmer temperature layers". Doesn't positive vertical velocity mean upward motion of air masses?

Typographic errors and other minor issues:

Lines 162-163: "For CTHs larger than approx. 8 km, CTH has an absolute percentage error of 10%, with underestimation for CTH > 10 km at 50° N and overestimation for CTH < 10 km at the same latitude." What does this mean? That CTH is underestimated when the measured CTH > 10 km but overestimated with the measured CTH < 10 km?

Line 188: I don't think that "detailly" is a valid word. Perhaps "in detail" would be better here, or simply say that both water and ice clouds are represented in the model (It is assumed that they would be represented realistically as possible by the model.)

Line 199: Change "transit to" to "transition into".

Figure 2: Time series of contrail cirrus... sounds better than "Temporal variation of contrail cirrus" in the figure title.

