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Comment on acp-2022-721

Anonymous Referee #2

Referee comment on "Investigating an indirect aviation effect on mid-latitude cirrus clouds – linking lidar-derived optical properties to in situ measurements" by Silke Groß et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-721-RC2, 2022

Investigating an indirect aviation effect on mid-latitude cirrus clouds- linking lidar derived optical properties to in-situ measurements, by Gross et al.

General comments

The authors investigate collocated in situ and lidar observations for two classes of high and low particulate linear depolarization ratio (PLDR) measured by the WALES lidar during the ML-CIRRUS field campaign as identified by Urbanek et al. (2018). The in situ measurements of interest are ice effective diameter and ice number concentration, and measurements in high PLDR (formed in air traffic regions) and low PLDR (formed in "pristine" regions) cirrus clouds are compared. After a quick presentation of the ML-CIRRUS campaign, of the WALES lidar system, and of the in situ instrumentation, comparisons are shown for two contrail cirrus clouds, two warm conveyor belt cirrus clouds, and finally for all flights, for which the comparisons are shown at 10 temperatures between 208 and 217 K. The authors conclude that in the 210-215 K temperature range, chosen to have a "sufficient contribution of both cloud types", high PLDR mode clouds have larger effective diameter and lower number concentration, which is "an indication for more heterogeneous freezing due to aviation induces emissions".

Even though not clarified in the abstract and in the introduction, this paper is an extension of the work presented by Urbanek et al. (2018), who found 2 classes of cirrus clouds with higher and lower PLDR, and who showed that they formed in busy air traffic regions and in regions with low aviation emissions, respectively. Urbanek et al. (2018) stated that heterogeneous freezing on emitted exhaust particles could explain the lower super saturations and higher PLDR that they found.

The lidar results derived from Urbanek et al. (2018) are well presented, which is convenient for the reader, but I was expecting to see more solid material about the in situ measurements. Section 2.3 about the in situ instrumentation does not discuss the expected performances of the instruments. It is not clear if NIXE-CAPS alone is sufficient for this study. I was expecting detailed presentations and discussions of the “combined” and “coordinated” lidar and in situ measurements, with discussions regarding the spatial and temporal collocations of the in situ and lidar legs. In the two case studies, the authors compare cirrus clouds of the same type and discuss the relevance of the comparisons. No conclusion could be drawn for the second case study (sect. 3.2) because temperatures differed by about 10 K (223 K and 232 K). In section 3.3 where all flights are combined for the comparisons, the relevance of the comparisons is not discussed. The authors present comparisons vs. temperature (Fig. 8), but only between 208 and 217 K (10 temperature values). As a matter of fact, not all in situ data were used. The authors need to detail which flights were selected for Fig. 8, why, how many PSDs per temperature range in the high and low PLDR modes, etc… I am not convinced that different number of samples justifies ignoring the cases which do not match the expectations. This might indicate that other phenomena come into play. I understand that such comparisons are challenging and that the campaign was not designed for this type of analysis.

In my opinion, the analyses presented in the manuscript are incomplete, and this manuscript does not represent a sufficient contribution to scientific progress to be accepted for publication in ACP. However, the scientific question is important, and perhaps the following suggestions and questions will help the authors pursuing this effort.

Specific comments

Abstract:

- Lines 20-22: this is misleading. These findings were published by Urbanek et al. (2018).
- Lines 22-23: in my opinion, this is an overstatement.

Introduction:

- Line 83: I strongly suggest to clarify that the cirrus clouds formed in air traffic regions or in pristine regions are identified according to the classification established by Urbanek et al. (2018) which is based on lidar measurements of PLDR. Lines 55 to 59 could be moved here.

Section 2 method

Section 2.1:

- Only 8 flights are listed in Table 1 (which should be introduced in the text), whereas 16 flights are shown in Fig. 1, with combined remote sensing and in situ observations for all these flights if I understand the text correctly. Please confirm that only the 8 flights listed in Table 1 are relevant for this study and explain why. For clarity, only the 8 flights listed in Table 1 should be shown in Fig. 1.
- Later in the paper, the authors refer to Table 1 when discussing number of observations in various conditions. I suggest providing for each flight information such as the number of PSDs, temperature and altitude range, PLDR range, etc…A suggestion is to add a dedicated table at the beginning of Section 3.
- Please define the PLDR: ratio of which quantities?

Section 2.3

- How many instruments were involved for this study? How do NIXE-CAPS, CAS-DLR and CIPg-UniM compare in terms of sensitivity range? Which instrument(s) was/were not available when only data from the NIXE-CAPS instrument were available and what are the possible consequences for this study? Please specify when you state that "comparison of the data sets for all other days showed a good agreement".
- Please define the effective diameter Deff and explain how it is computed from the PSDs. I anticipate that assumptions are necessary. I could not find the Schumann et al. (2010) reference.
- Line 139: the data are averaged over 5 s intervals. Can you comment on the number of PSDs for each flight? This piece of information should be provided.

Section 3 results

- It might be worth clarifying or reminding that the goal is to compare in situ measurements in two classes of cirrus clouds exhibiting large and low PLDR as established by Urbanek et al. (2018). The authors actually investigate the microphysical properties.

Section 3.1

- Can you explain why the cirrus with embedded fresh contrails observed on April 7th, 2014 is not somewhat affected by aviation exhaust. I could not have access to Stettler et al. (2013), but nevertheless, I think that this deserves an explanation in this paper.

- Line 171: why care about a missing instrument on 7 March? Unless you meant 7 April? Why no impact on the results if an instrument is missing?
- Is it possible to point to the regions with embedded fresh contrails in the lidar plots? Are these regions identified from observations or from the model?
- The authors should present in details the spatial and temporal collocations of the lidar measurements and in situ measurements presented in this paper. I note that in situ measurements are shown in Wang et al. (2022) for the 26 March, 2014 flight. The authors should clarify which in situ legs are used in this work, and how they were chosen. The number of PSDs for each flight, altitudes and temperatures could be added in Figure 3 for clarity.
- I see large Deff > 100 um on April 7th even though median Deff is smaller than on March 26th. This should be acknowledged. Authors should discuss in this section the various possible reasons for the larger N on April 7th compared to March 26th. For instance, it seems that the in situ measurements were higher in the cloud on April 7th, and perhaps closer to an embedded fresh contrail? Plots showing N vs. Deff for each case could be useful for this discussion.

Section 3.2

- Lines 183-186: are you describing liquid origin cirrus clouds (e.g. Luebke et al., ACP, 2016)?
- The authors should present in details the spatial and temporal collocations of the lidar and in situ measurements. Same comments as for the previous case study.

Section 3.3

- I believe that Fig. 7 is not really useful here and that Fig. 8 is the most interesting. That being said, the warmest temperature is 217 K, which indicates that the comparisons presented in section 3.2 (223 and 232 K) are not included. I really do not understand. I see that Fig. 8 uses data from CAS-DLR/CIPg-UniM and NIXE-CAPS, but having only NIXE-CAPS did not seem to be an issue for the case study presented in section 3.1. Please justify this choice and detail which flights were used to create Fig. 8 and which cirrus types. I suggest to give the number of PSDs at each temperature for the high and low PLDR range to avoid vague discussions.
- I see Deff larger in the high PLDR mode (dark blue) than in the low PLDR mode (light blue) only at 210 K and between 212 and 214 K. N is smaller in the high PLDR mode except at 215 K and 217 K. I am not convinced that the different number of samples justifies ignoring the cases which do not match the expectations. This might indicate that other phenomena come into play.
- Line 247: The "tendency towards larger Deff with temperatures" is consistent with numerous publications found in the literature, which should be cited.

Discussion and conclusions (should be section 4?):

- Lines 265-266: respectfully, I think that this is an overstatement. I see these findings only at 210 K and between 212 and 214 K.