

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-721

Anonymous Referee #1

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-RC1>, 2022

The authors use a data set of airborne lidar and in-situ measurements to study the effect of aviation on the optical and microphysical properties of natural cirrus clouds. In an earlier study by some of the same authors, lidar measurements of the particle linear depolarization ratio (PLDR) were used together with backward trajectories to identify cirrus clouds that likely formed in regions of high and low air-traffic density. The present work follows up on the previous study by adding the analysis of coinciding in-situ measurements of the ice crystal size distribution and the ice crystal number concentration (ICNC) related to the two PLDR modes to the investigation of an indirect aviation effect on mid-latitude cirrus clouds. The authors conclude that cirrus clouds that are affected by aviation as indicated by higher values of PLDR also show larger effective ice crystal radii and lower ICNC compared to unperturbed, low-PLDR cirrus.

The authors address an interesting topic that is certainly relevant to the readers of ACP. They have a unique set of airborne observations at their disposal. However, the study itself as well as the presentation of the results need considerable improvement before they can be accepted for publication in ACP. Below is a list of major and minor comments to the authors.

Major comments

- The text is quite repetitive, often imprecise, and sometimes just confused. Please refer to the minor comments below for details. The authors should review the text carefully. Language editing is urgently needed before publication.
- The description of the data set and methodology is rather sloppy:
 - What is the procedure for ensuring that lidar and in-situ instruments have probed the same cloud? How have sections of the flights during ML-CIRRUS been selected for the analysis presented here?

- Does the data set include all cases of coincident lidar and in-situ measurements? What is the volume of the data set of coincident lidar and in-situ observations? Is it a few minutes or several hours? This information is also missing in the corresponding plots.
- Instead of providing information on the water vapour measurements (which are not used at all in this study), the authors could define the backscatter ratio for lidar non-experts or state what typical cirrus PLDR values are.
- How are regions of high and low air-traffic density defined and how is the connection made to the measurements? You could provide a quick review of the procedure in Urbanek et al. (2018) and clarify that your analysis is the continuation of their work based on the same cases. If this is clear, you wouldn't need to reproduce an already published figure that isn't really necessary here (as is obvious from the fact that it isn't discussed at all). In any case, figures shouldn't be reproduced from an earlier publication.
- The section on the in-situ measurements is quite confusing and ends with a statement that *comparison of the data sets shows good agreement*. While we don't know what that means or how the data of the different instruments have been combined, it seems that the consideration of data from NIXE-CAPS is sufficient for the purpose of this study. Please stick to the necessary data to keep the study simple.
- The analysis of cirrus measurements for different temperatures could already be motivated and outlined in the methodology section. The availability of in-situ measurements of temperature and humidity is first mentioned in line 168. Why are these measurements and the corresponding instruments not listed in the data section?
- The authors should revise the presentation of their data and results:
 - Table 1 is a reproduction of parts of Table 3 in Voigt et al. (2017). It would be more useful for the reader if the table was to include information on the inferred parameters for different cases rather than generic mission information. I would like to see, for instance, number of data points, length of measurements, mean PLDR, mean ICNC, mean Deff, etc. For readers of a scientific paper, dates – as used later in the plots – are much more tangible than arbitrary mission IDs.
 - Figure 1 is a reproduction of Urbanek et al. (2018). Why not just state that their cases will be used for closer inspection in this study? In any case, the authors should briefly review the approach in Urbanek et al. (2018) for allocating high and low PLDR cases to regions of high and low air-traffic density, respectively.
 - It is not clear what the readers should take from the lidar plots in Figures 2 and 5. It would be more straightforward to combine Figures 2c and 2d with Figure 3 (and analogous Figures 5c and d with Figure 6) into key plots that present all frequency distributions. In any case, please choose a plot mode that allows the readers to separate the different cases. Curves without fill would be an obvious choice over overlaid bar plots. Please also add information on the data sets to the plots if you decide against presenting them in a table.
 - Figure 4 could be omitted. Its content is fully described in a few sentences. Instead, it would be nice to get more information about the cirrus classification scheme either in the discussion of the second case study or already in the methodology section.
 - I suggest to motivate the analysis of clouds at different temperature already in the methodology. Figure 8 could be discussed before Figure 7. In addition, the left panel in Figure 7 seems unnecessary in light of Figure 8 and its discussion. Stating the message of that figure in the text is sufficient. It would be very helpful if median values were also marked in the plots in Figure 8, for instance by vertical lines.
- The data show differences in ICNC and Deff for clouds with different PLDR. The value of this finding would be much increased if the authors where to present the result of a significance test.

Minor comments

- line 36: Bräuer et al. (2021a) doesn't seem to be related to effects of biofuel. Please remove.
- line 38: An increase in ICNC is also found in Marjani et al. (2022): <https://doi.org/10.1029/2021GL096173>.
- line 40: IN should be INP as introduced in line 275.
- line 53: Redundant. This has just been clarified in the previous paragraph.
- line 55: ML-Cirrus or ML-CIRRUS?
- line 57: pristine really means air-traffic free or with low air-traffic density. Please clarify throughout the manuscript.
- line 64: less cirrus formation? Do you mean a reduction in cirrus occurrence?
- line 69: energy forcing = radiative forcing?
- line 75: They also found... Not clear what is stated here.
- line 151-156: This introduction to the first case would be more trustworthy if the authors had provided a review of Urbanek et al. (2018) earlier in the manuscript. Please provide link to Urbanek (2018).
- line 160-164: Consider putting this into a table.
- line 164: It is not clear how embedded contrails have been identified.
- line 170-173: As raised in the major comments, this statement indicates that data from NIXE-CAPS should be sufficient for the purpose of this study then.
- line 175: Significant digits: give either exact numbers or rounded values, but not a combination of both.
- line 183-186: Please revise the description for precision. Is it a cloud or a flow? What's the dimension related to several kilometres?
- line 199-205: Consider putting this into a table.
- line 254: This cannot be seen in the current Table 1. It could be, if the authors were to include a table that presents the values of the considered parameters for the different cases.
- line 303: It would be nice to list these open questions as motivation for further research.