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Comment on acp-2021-172

Anonymous Referee #1

Referee comment on "Changes in cirrus cloud properties and occurrence over Europe during the COVID-19-caused air traffic reduction" by Qiang Li and Silke Groß, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-172-RC1>, 2021

Summary of Paper

The authors examine changes in cirrus cloud properties during the civil air traffic slowdown in March and April 2020 (due to COVID-19 air travel restrictions). CALIPSO vertical feature mask (VFM) data over western Europe are compared with data from similar periods from 2014 through 2019, and mean cirrus properties including cirrus cloud occurrence, average thickness, and particle linear depolarization ratio (PLDR) are determined for both 2020 and the earlier years. Cirrus cloud occurrence was found to decrease 30 percent in 2020 compared to three other years (2014, 2017, 2019) with no air traffic reductions and similar meteorological conditions. The calculated average cloud thickness was also less in 2020 (1.18 km) than in the previous years (1.40 km), and the PLDR values were reduced during air traffic slowdown. CALIPSO observations over China and USA were also examined to confirm the impact of air traffic reductions on cirrus cloud properties.

General Comments

Although the authors provide some solid evidence that cirrus cloud properties changed during the COVID-19 induced air traffic slowdown, several confusing aspects of the paper's presentation detract from the paper's value. For example, the authors present bi-monthly (March and April) in Figures 1 and 8, but only monthly (April) data for the remaining figures. Are the authors conflating the March and April data as one distinct period of air traffic activity? As far as I can tell, air traffic volume was changing throughout March over Europe, so conditions between the two months may not be as similar as presented in the paper. Second, it is not clear why or how the years 2014, 2017 and

2019 are chosen as the closest analogues to 2020, Those years might be the closest to 2020 in terms of mean 500-hPa geopotential height, but each is noticeably different. If the bi-monthly (or monthly, it's not clear which is used) mean meteorological properties are sufficient for judging the similarity of meteorological conditions for each year, why not use the 6-year (2014 through 2019) mean instead? The 6-year mean appears to be the best match to 2020 in Figure 1. Third, which data source are the authors using to decide the proper analogues? The temperature and humidity data from Figure 2 suggest some meteorological differences between GEOS-5 and the NCEP/NCAR reanalyses from Figure 1 (which is not surprising), so which do the authors consider to be more reliable? Fourth, why include years 2015, 2016, and 2018 in Table 3 but not elsewhere in the paper? I believe the paper would be improved if the authors addressed these ambiguities, and perhaps only presenting data from April.

Figures 3 and 4 are also unclear. The results of Figure 4 seem to contradict Figure 3! Figure 4 implies that the cirrus thickness distribution is skewed more toward thinner (less than 1 km) clouds in 2020 when compared to the other three years, yet Figure 3 shows that the decrease in occurrence rates in 2020 compared to other years is the largest for the thinner clouds. How is that possible? How can the relative frequency of thin clouds increase in 2020 while the occurrence of thin clouds decrease the most (compared to all other thicknesses) in 2020?

The discussion about the PLDR data over China and USA seems to be speculative, especially in terms of the meteorological conditions over both regions and in comparison to Europe. Do the authors know that USA and China have similar meteorology in 2014, 2017, and 2019 compared to 2020? If not, then Figure 8 implies that meteorology is not important for determining PLDR! Do the authors have any air traffic data to support the claims on lines 3-10 of page 12? Several factors are claimed to affect PLDR in the discussion (cirrus cloud height, the magnitude of air traffic, meteorological conditions) but these effects at best only described vaguely in this section.

P12,L3-5: "We next focus on the results observed over USA and see slightly larger values in 2017 for both months which may be due to the variations of meteorological conditions in different years and is comprehensible." Have the authors checked this claim to be sure? Like much of this section, the discussion here is vague and speculative.

Typographical errors and minor objections

P4,L23: What are "radiative forces"?

P7, L2-3: The research area has already been defined earlier in the manuscript so the mention of the lat/lon box is here is superfluous.

P7,L9: Please use higher altitude rather than larger altitude

P9, L15-16: "But the decreases of the PLDR with height were only found at altitudes larger ~10km in 2020." Is this referring to Mar 2020 only? This seems to contradict Figure 7.

Several typographical errors were noticed in the text. Some of them are listed below although this is not exhaustive (please proofread the paper).

P3,L16 Constellatin

P4,L22 comsisting

P6,L8: Observering

P6,L20: propierties

P7,L9: differes

Table 2, column 2: Medain

P11,L14: quartiel

P12,L26: ocurrence

P13,L3: funciton

P13,L4: referece

P13,L5: charaterized

P13,L7: yeras