Response to Review RC1 by Referee #1.

We thank the Referee for his/her careful review and for the suggestions for an extended analysis. In the following, the Referee's questions and comments are repeated in black and our responses follow in blue.

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.

- ➔ This is right that air traffic started to reduce from about Mar.10, 2020 and reach the lowest level until Mar.23, 2020 (nearly 80% lower than 2019). While air traffic kept at that level during the whole month in April 2020 (Source: https://www.eurocontrol.int/Economics/2020-DailyTrafficVariation-States.html). Following the comments, we changed the presentation of 500 hPa geopotential height for a monthly one in March and April, respectively.

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 similiarity 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.

- ➔ The reviewer is right, that we choose these years based on the 500-hPa GPH. We now included also different mean meteorological parameters in our consideration (e.g.

Temperature, RHi and vertical velocity). We see that the different years are quite different and show a large variability in the meteorological situation. In March, 2018 and 2020 are the outliers with higher temperatures and lower humidity at altitudes above 10 km. The difference of meteorology in 2018 can be also seen in the 500 hPa GPH. In April, however, the only 'outstanding' year is 2016 with warmer and drier airmass above 9 km and 2020 is well within the spread considering mean meteorological parameters.

➔ To avoid any misunderstanding, we furthermore extended out analysis for all the years from 2014 to 2020 for our comparison in the revised manuscript.

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?

➔ The reviewer is right that temperature and humidity data from Figure 2 and the 500 hPa GPH in Figure 1 may show inconsistency since the meteorological data (temperature and humidity) were derived from different pressure levels. For the original manuscript, we compared the temp and humidity data from GEOS-5 because the data are saved in the CALIPSO data and hence easy to use. In the revised manuscript, we compare the meteorological data (temp, RHi, and vertical velocity) derived from ERA-5 to give a more general picture along the altitude range covering our observations. Anyway, we have extended our analysis to all the data from 2014 to 2020 for a more comprehensive comparison with a spread of meteorological variations.

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.

➔ Thank you for the suggestion. In the revised manuscript, we now include all the years from 2014 to 2020 in our analysis. In order to make the description in the manuscript smooth, we will present only the April results (cirrus occurrence and PLDR) and mention the March results in text and tables. Although the meteorological conditions are quite different, the results still strongly support our perspectives. Only the meteorological conditions in 2016 led to a stronger reduction in cirrus cloud occurrence. However, the cirrus clouds in 2016 did not show the reduction in PLDR as it was found in 2020.

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?

➔ We show the cirrus thickness distribution in relative frequency (100% in total). Here, the strong reduction of cirrus thickness with a mean of 1.18 km was found in March 2020 (instead of April, sorry for the confusing). We showed the cloud thickness in both March and April in the revised manuscript. The reduction in March is supposed to be stronger than expected from only the aviation change, since the meteorological conditions with warmer and drier airmasses in March 2020 were less favorable for ice cloud formation than the previous years.

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!

- ➔ The reviewer is right, we do not check if the same years can also be used for a comparison between 2020 and the selected years (2014, 2017, and 2019). We hence extended our analysis to the previous years from 2014 to 2019 without pre-selection. However, we agree, that the meteorological conditions are not important for the changes in cirrus PLDR.
- ➔ Furthermore, following the recent results from *Righi et al.* (2021), we would also not expect strong changes over the China region as the aviation effect on cirrus clouds in this region is in general very low.

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.

- ➔ Yes, the sources of air traffic data we refer to are from www.airlines.org/dataset/ and www.eurocontrol.int/covid19 (last access: 25 June 2021).

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.

- ➔ The reviewer is right! We reformed the discussion.

Typographical errors and minor objections

P4,L23: What are "radiative forces"?

- ➔ Sorry for the typo (radiative forcing). Changed.

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.

- ➔ Thank you. We reformed the sentence accordingly.

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

- ➔ Thank you. Changed.

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.

- ➔ Yes, this is the results of March 2020 (not shown).

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

- ➔ Thank you. The typos have been corrected.

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