Response to Review RC2 by Referee #2.

We thank the Referee for carefully reading our manuscript and for the comments and suggestions to improve the current work. In the following **the Referee's comments are repeated in black and our responses follow in blue.**

The paper Satellite observations of seasonality and long-term trend in cirrus cloud properties over Europe: Investigation of possible aviation impacts by Qiang Li and Silke Groß is highly interesting and robust.

It is relevant in showing how vertical profiles from satellites could give a better insight of the atmospheric estate, fill gaps in knowledge, and pose new scientific questions.

As general comments, I think it is detailed and many investigations are reported. The impression is that the reader can sometimes be lost in the progress of such reporting. I would suggest reducing the number of figures and focusing more on what is the main result. Figure 12 is the main message that probably authors would like to give as a take-home message, but this is somehow diluted by the presence of many analyses: these are relevant for reaching the main results but could be shortened and eventually reported as an appendix or additional material.

- ➔ Thank you for the general comments.
- ➔ It is stated in the manuscript that the cirrus morphologies and occurrence rates as well as the high degree of variability in their microphysical properties highly depend on the substantial differences in the meteorological conditions. To draw a final conclusion of the aviation impact on cirrus properties, it is necessary to discuss as detailed as possible how the cirrus properties vary depending on the meteorological conditions and their occurring heights. According to the comments, we have tried to reduce the figures of the results from the day-time observations and kept the description of them only in text.

Apart from this general comment, 3 are the points to be clarified /discussed/fixed in the paper:

- It seems that 2 different models are used for temperature and humidity during the investigation: ECMWF and GEOS. Why this difference? Why not use the same for the 2 analyses reported? Please clarify

=> The CALIPSO science team determine meteorological conditions (including temperatures) from the GEOS-5 data assimilation products (from GMAO) interpolated in space and time to the CALIPSO orbital tracks. The data are saved along with other original measurements of targets (including aerosols and clouds) by the onboard instruments. It is robust to use the coordinated data of meteorological conditions for comparisons with the cirrus properties. While in Figure 2 of the manuscript, the ERA5 data are used to give a general picture of the meteorological conditions of the research area including temperature, humidity, and wind fields. ECMWF is considered as the most advanced and most reliable model. In comparison to the GEOS-5 data, the ERA-5 data have a better resolution and accuracy of the forecast. Furthermore, they are run based on different data assimilation process and governing equations, the archive of meteorological information from one model in different year should be consistent. A general picture of meteorological conditions is enough for our analysis and should not bring any misleading.

- In the PLRD temporal behavior of fig 12, there is an anomaly in the 2010 and 2017-2019 (mainly 2018) period: is it possible that the big volcanic eruption affecting Europe in 2010 is

the cause of the 2010 anomaly? Is the aerosol/cloud misclassification in VFM a potential issue then? Which could be the reason for the lower PDLR in 2017-2018? Please discuss this point

=> Many flights in Europe were canceled because of the volcanic eruption in 2010. But the air travel disruption only lasted for a short term. From Figure 1 of the manuscript, the air traffic in Europe (incl. 42 countries and regions) in 2010 from April on did not show big anomaly (departure) from the corresponding periods in other years. The deseasonalization process was done on the data by computing the monthly climatological mean, subtracting them from each monthly record and finally adding the total mean of δp. The extreme values of δp might change the deseasonalized dataset significantly. The unexpected low values of deseasonalized δp in the last second half year of 2010 and first half year of 2018 are supposed to be due to the lower values of δp in August and November 2010 as well as in February-March 2018 compared to the corresponding months in other years, respectively. This has been discussed in the text that the occurring heights of cirrus are supposed to be correlated with the extreme values (see Figure 7). According to the height dependence of PLDR, the departures of PLDR in the mentioned months will lead to the anomalies in the deseasonalized datasets. The corresponding discussions have been added in the manuscript. Thank you for the comments.

- I am not a native English speaker, but the paper is somehow hard to read. I reported some revisions in the comments in the attached pdf, but these are just examples. Please revise the paper carefully in this sense.

=> Sorry for the bother. We will try our best to polish the languages of the manuscripts.

These and more detailed points are reported as comments in the pdf file.

=> The pdf file is the original manuscript.