



EGUsphere, referee comment RC1
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Comment on egusphere-2022-628

Anonymous Referee #1

Referee comment on "Satellite observations of seasonality and long-term trends in cirrus cloud properties over Europe: investigation of possible aviation impacts" by Qiang Li and Silke Groß, EGU sphere, <https://doi.org/10.5194/egusphere-2022-628-RC1>, 2022

In this work 10-year lidar measurements of cirrus clouds with CALIPSO are analyzed to determine inter and intra annual variability and trends of their occurrence and optical characteristics, namely depolarization. Correlation with atmospheric temperatures and air traffic are also explored.

A seasonal cycle was detected both in occurrence and optical properties, with larger values of both parameters in winter. A positive trend in the deseasonalized depolarisation time series and a negative trend in the deseasonalized occurrence rate time series were also demonstrated.

As the author claims that there exists a positive trend in air temperature at cirrus altitude, and since depolarization is temperature dependent to a certain extent, they remove such effect from the depolarization time series. This is done by applying a linear regression model on the depolarization-temperature dependence and subtracting the model from the depolarization time series. The time series of the depolarization residuals again shows a positive trend. The author links this positive trend to an increase of air traffic over the time window of the dataset, this latter estimated from the increase in its contribution in CO₂ emissions.

The work is interesting and important and certainly deserves to be published. However, there are two aspects that, in my opinion, need further study before publication. First, I must note that the work is not fully convincing in studying the interannual trend of air temperatures. In fact, while the seasonal trend of temperatures is clear, a decadal trend is not. I think this is the biggest problem of the work, which otherwise demonstrates a sufficient maturity to ensure its publication. For this reason, I encourage the authors to dwell more on the study of the decadal trend of the air temperature, to accompany their findings with any supporting literature or otherwise to make their claims on such topic less assertive.

A second aspect that has not been sufficiently analyzed concerns the lack of an analysis if the observed trends are due only to the increase in cirrus originated by contrail, or rather to changes in the microphysics of cirrus clouds in general. I do not know (I doubt with sole lidar data) if there is the possibility of dividing the dataset into two categories, based on the proximity of the observations to air corridors, or on the vertical and/or optical thickness of the cirrus clouds, or their horizontal (i.e. along satellite track) extent in order to identify the cirrus clouds originating from contrail with respect to the "natural" others. If such categorization were possible, the work would undoubtedly benefit from a non-aggregated analysis. In any case, I believe that the question should still be discussed. One final note, I as a non-native English speaker, found English of the text rather tiring to read. I would suggest having the text proofread by a native English speaker.

In the following, more detailed annotations on the text (page, line)

(1,4) "...are supposed..."

(2,56) typo "depdendence"

(3,68-69) Unclear. Could rephrase as : "The light emitted by lidar AND BACKSCATTERED BY SPHERICAL PARTICLES exhibit the same orientation of polarization as the incident light if it is scattered WHILE THE POLARIZATION CHANGES and different polarization if scattered WHEN THE LIGHT IS BACKSCATTERED by non-spherical particles such as cirrus ice crystals."

(3,71) I would cancel , "e.g., non-spherical mineral dust particles with high values of $\delta\rho$ " or add other types of non-spherical aerosol that have different mean values of $\delta\rho$ (Biomass Burning aerosol, Sea Salt, etc..)

(3,75) Pristine habits are mainly driven by the temperature at which a crystal forms and, maybe to a lesser extent, by the humidity of the air. However the internal cloud dynamics and lifetime duration and stage strongly influence the shape of crystals as well. That should be mentioned.

(4,112) Typo „difficulat“

(4,112-113) „However, there is an aviation fingerprint with two maxima during eastbound and afternoon westbound traffic in the area we are focusing on here.“ maximum of what? what is the area we are focusing on?

(4,114) „Therefore..." the lack of clarity of the previous sentence makes its consequences unclear.

(4,116-118) This should be shifted upward.

(5,135) „extreme lower“. Lowest?

(5,139-140) Could you comment on the statistical significance of such results? At face value it does not seem high.

(5,141-144) Please discuss the statistical significance of the findings.

(5,149-153) I honestly don't think this assertion is sufficiently supported by the above analysis. Tests for the presence of trend, confidence intervals for the trend, etc. should have been performed. In the absence of such tests (which however I encourage the authors to conduct) I suggest reformulating the sentence in a less assertive form and / or recalling any studies in the literature supporting these same conclusions.

(5,156) To my knowledge, the common cruising altitude for most commercial airplanes is between 10 and 13 km . typically, aircraft fly around 10-11 km.

(6,175) "Please note..." How do you exclude deep convection cirrus? Please specify the methodology.

(10,288-295) "Generally...temperatures." As in the subsequent analysis the nonlinear regression models have not been used, there is no need to quote them here. Unless you provide a justification for the choice of the linear instead of the non linear one.

(13,354) See my comment on (5,149-153).