Review of amt-2023-134 by Cristina Gil-Díaz et al.

This study makes use of five years of ground-based lidar measurements of cirrus geometrical and optical properties that are retrieved using the established two-way transmittance method over Barcelona. Optical and geometric properties are compared against other studies in a meta-analysis. Relationships among the different optical properties, and thermodynamic properties from radiosoundings, are examined.

The methodology is fairly sound but the authors should also consider in more detail the effects of conditional-sampling in their methodology as their method (two-way transmittance technique) is also only applicable to a non-random subset of the data.
The authors make conclusions about several relationships (e.g. latitudinal dependence of lidar ratio, and a relationship between depolarization ratio and cloud optical depth) that do not appear to be supported by the presented data. These conclusions should be reexamined. The authors also need to clearly distinguish between what is concluded based on this study, and their hypotheses about the causes of any observed relationships.
With these issues, I am left uncertain as to the novel contribution of this article to our understanding of cirrus properties or to our understanding of lidar remote sensing of cirrus. No new insight into the two-way transmittance technique is provided. Nor are the differences between the two-way transmittance method and the results of the operational MPLNET or CALIOP algorithms evaluated. The general conclusion seems to be that cirrus properties over Barcelona are similar to other studies. This is not without merit, but AMT does not seem to be the most appropriate venue for this manuscript in its present form, as AMT focuses on the development, intercomparison and validation of measurement techniques. As such, I recommend rejection of the manuscript.

I have made some suggestions to improve the manuscript and raise specific technical issues below.

Specific Comments:

Line 27:
The introduction is a little misleading beginning with Aerosol-radiation as well as aerosol-cloud interactions, despite neither being the subject of this paper. I suggest that the first paragraph be adjusted to begin by discussing the importance of cirrus as is done halfway through the paragraph.

Line 41:
I suggest the second paragraph omit the first two sentences and begin with "Cirrus clouds can form by different …"

Line 53:
I suggest that the third paragraph could be made stronger by being arranged in an argument that motivates this work as follows:

1. Ice cloud microphysics and their relationship to optical/radiative properties is complex.

2. Remote sensing of cirrus properties requires the assumption of a crystal habit or adoption of a particular empirical model, which complicates the results.
3. Lidar provide the ability to infer cloud optical depth etc. without making such assumptions.

This provides the same background but also more clearly motivates the importance and use of lidar remote sensing.

Line 68:
Lidar systems do not measure vertical profiles of extinction, in general, but in some cases can retrieve it.

Line 113:
Multiple scattering contributions do not depend only on the receiver field of view. The other relevant factors should be mentioned and additional references should be provided to justify this choice (e.g. Shcherbakov et al. 2022).

Line 234:
This is not the definition of the cloud optical depth. The optical depth is the vertical integral of the volume extinction coefficient. Definitions need to stay consistent to preserve meaning. This equation should be modified to explicitly include the multiple scattering correction, with the note that it is assumed to be negligible.

Equations:
Please use the same notation for integrals over range/altitude. The vertical coordinate variously appears as x, u, and z which is confusing.

Line 278:
This does not seem like a very precise convergence criterion.

Line 313:
I am not sure about these criteria. Does this eliminate the possibility of multiple layers of cirrus? Shouldn't we want to know the properties of both layers?

Line 322:
A success rate of 55% indicates that a significant fraction of data are omitted from the analysis. Any systematic reason for the omission of the data might substantially alter the resulting analysis. For example, it is stated at Line 318 that cases with high lidar ratio, typically with high levels of noise, are discarded. If this noise is caused by low signal strength due to strong attenuation (rather than noise in the lidar signal itself or solar noise), then this indicates a systematic sampling bias that should be discussed.
It is not clear whether the cirrus category in Figure 3 only includes the 203 cases, as a COD is derived, or whether the success of the two-way transmittance method is judged based on the lidar ratio. I suggest separating results into "non-cirrus, successful cirrus, failed cirrus" cases.
It was stated earlier that the two-way-transmittance test will fail for very optically thin clouds (i.e. subvisible). Some justification is required for why the statistics of subvisible cirrus should

be treated as representative. Uncertainties should be propagated to establish the precision of these retrievals.

Figure 4:
Again, the daytime/nighttime contrast should be partitioned by retrieval failure or success.

Figure 5a: The bins are not particularly clear. I suggest logarithmically spaced bins as well.

Table 3:
The meaning of the quantity after the +/- needs to be defined. Is this the standard deviation? Or the standard error in the mean?

Line 430:
I would disagree with the conclusion that the lidar ratio has a generally increasing trend towards the poles. Instead, my conclusion would be "the variability at different sites appears negligible relative to the variability at each site."

Line 452:
It needs to be clarified whether this correlation is between COD and the other cirrus properties or between log10(COD).

Figure 6:
The grey shading does not appear to be the 95% confidence intervals of the linear regression. I would expect uncertainty in the slope of the regression to produce diverging bounds on the relationship in a "><" shape, unless the standard error in the slope is negligible compared to the standard error in the intercept, which I would not expect to be the case for the shown data.

Line 470:
My reading of Figure 6 (bottom) is opposite to the authors in that there is no significant relationship between LCDR and COD. The r-squared value is 0.03.
The reference entry for Chen et al. 2002 appears to be incorrect.
What I presume to be the correct reference (below) suggests a decreasing relationship between LCDR and COD. This study is distinct in that there is no significant relationship.

Figure 7:
The caption refers to a known range of lidar ratio for cirrus clouds being less than 40 sr. Some references are required for this. The authors should bear in mind that in situ measurements of lidar ratio are not column averaged, while what is reported here is an effective column lidar ratio.

The authors should comment on the possibility that the MPLNET cloud classification used to define cloud in this study is misclassifying aerosol as cloud and that that contributes to the low depolarization ratios.

Line 480:
Should the thermodynamics not also be a major indicator here? How many cirrus clouds even have cloud-base temperatures that are above the homogeneous nucleation temperature?

Line 482:
I suggest focusing on the warmest temperature within the cloud (i.e. cloud base) for this determination, rather than mentioning altitude.

Line 499:
“It could be said” is vague. Just state the result.

Line 502:
No weather patterns were examined, so this should not be a conclusion. Rather, it is a hypothesis about the differences between sites. The latitudinal dependence does not seem significant.

Line 507:
The average height of cirrus is probably not 1.1 km.

Lines 510-517:
This is too long for the conclusions and repeats information. Moreover, several hypotheses about the cause of the results are presented as strong conclusions.
For example, the lidar ratio increases with COD because of turbulence). Turbulence was not measured and this attribution cannot be concluded.
The linear depolarization does not appear to have any relationship. Certainly, there isn’t any cause to attribute any relationship to increases in aggregation, as opposed to e.g. micro-facet roughness of the crystals.

References:
Wei-Nai Chen, Chih-Wei Chiang, and Jan-Bai Nee, "Lidar ratio and depolarization ratio for cirrus clouds," Appl. Opt. **41**, 6470-6476 (2002)

Shcherbakov, V., Szczap, F., Alkasem, A., Mioche, G., and Cornet, C.: Empirical model of multiple-scattering effect on single-wavelength lidar data of aerosols and clouds, Atmos. Meas. Tech., 15, 1729–1754, https://doi.org/10.5194/amt-15-1729-2022, 2022