Review of
Determining stages of cirrus life-cycle evolution: A cloud classification scheme
by Urbanek et al.

General comment:
In this study a classification scheme for stages of cirrus cloud life cycle is presented. The scheme is based on LIDAR data in combination with meteorological data (temperature and pressure) from ECMWF. In a case study of orographic cirrus clouds as measured during the ML-CIRRUS campaign the scheme is applied and the results are interpreted.

Generally this is an interesting and important contribution to ice cloud research; thus, this study is an appropriate contribution for AMT. However, there are some issues, which must be clarified before this manuscript can be accepted for publication. Therefore, I recommend major revisions of the manuscript. In the following I will explain my concerns in details

Major points

1. Classification scheme and interpretation of results

   The general aim of the scheme is not really clear to me. I recommend that the authors give a bit more information about the aim and the possible use of the scheme.

   In general, I agree with the discrimination between regions of potential ice nucleation, moderate supersaturation and subsaturation, since this reflects the different thermodynamic states of the system. However, the role of the class HET is not clear to me and seems to cause severe problems:

   (a) Since heterogeneous ice nucleation is not well understood, and ice nucleation on solid particles depends on many details, a general nucleation threshold (as e.g. for homogeneous nucleation, but see minor comment below) cannot be determined. This problem is already reflected in this scheme by the use of 2 different parameterisations and their difference of about 20-30%. Therefore, the definition of the class $\mathrm{HET}_{\mathrm{in/out}}$ is quite arbitrary, since the lower bound is very fuzzy.

   (b) For cloud free air the class might be useful, since then the possibility of heterogeneous nucleation could be estimated. But again the arbitrary thresholds of heterogeneous nucleation make it very difficult to use this information in a meaningful way.

   (c) For cloudy data, this class might lead to severe misinterpretation of the data. In the text it is suggested that for data points of $\mathrm{HET}_{\mathrm{in}}$ heterogeneous nucleation takes place or even ice crystals in this category stem from heterogeneous nucleation. This suggestion is not correct because of the problem stated in (a): The nucleation threshold is not well-posed, thus it might be that using a low threshold no heterogeneous nucleation takes place (since the IN need higher supersaturation); thus, the interpretation of ongoing nucleation would be wrong. In the case study the lower threshold is used, but it is not clear if this is really the right one.

   These problems weaken the classification scheme in a serious way; therefore I recommend either to remove the class $\mathrm{HET}_{\mathrm{in}}$ completely or even to refine the representation using different heterogeneous nucleation thresholds as a standard. Perhaps additional information could be given in addition to the coarse classification HET. If the class HET is kept in the scheme, its use, benefits and problems should be described carefully.

There is another issue regarding the interpretation of the classification. The scheme is based on measurements, i.e. on an Eulerian viewpoint, since the time evolution cannot be seen. If ice crystals were found in the class $\mathrm{HET_{in}}$, they are not necessarily formed by heterogeneous nucleation. The classification just can tell some information of the actual state of possible nucleation, but not about the particles, which are already in the air mass. For instance, sedimenting ice crystals could be found in the air mass, but they were formed at higher altitudes under completely different conditions. The authors should mention this problem, since confusing Lagrangian and Eulerian viewpoint could lead to completely wrong results.

2. Analysis of case study

   The demonstration of the classification scheme was carried out using a very special case of orographic cirrus clouds. In general this is ok, but the interpretation of the case could be more specific.

   (a) Probably weak sedimentation
   Since the cirrus cloud was obviously formed by a (strong) wave, probably sedimentation was not a big issue, since many small ice crystals were formed. The region at the top of the cloud showing very high backscatter ratios is a hint into this direction. Maybe the authors could use the analysis of the trajectory in order to estimate the vertical velocities, which might be interesting for homogeneous nucleation.

   (b) Descent of the cloud
   The authors claim that the descent of the ice cloud is probably triggered by large-scale downdrafts. However, this could be corroborated using ECMWF wind data, which are available; this would also strengthen the argument for the occurrence of region DEP and SUB. In addition, they should estimate sedimentation velocities of ice crystals for typical sizes in order to rule out the case of sedimenting ice crystals leading to this cloud descent.

   (c) High supersaturation without ice nucleation
   In the measurement time 14:34-14:36 high ice supersaturation occurs (at least higher than RHi_{het}) without ice nucleation. This might point to the possibility that either heterogeneous nucleation at high thresholds or even only homogeneous nucleation are the preferred nucleation types in this situation. Again, this points to the weakness of the definition of HET regions without a concise threshold for heterogeneous nucleation. What about measurement errors in relative humidity (of order of 10-15%)? Might it be possible to reach higher values of RHi?

Minor points:

1. Page 2, lines 8-12: in situ vs. liquid origin ice crystals
   The discrimination between these two types is based on thermodynamics

   - liquid origin: freezing of existing water droplets at water saturation
   - in situ: freezing of solution droplets or heterogeneous nucleation at ice supersaturation but below water saturation

   Maybe this could be mentioned in the text. Please also add the reference Wernli et al. (2016), since the classification (in situ/liquid origin) is also used in this study.

2. Page 2, lines 19-25: vertical structure of ice clouds
   The description is probably only valid for stratiform cirrus clouds, formed by in situ formation

mechanisms. For liquid origin ice clouds and for clouds with strong dynamics (waves or instabilities) the structure might be different. This should be mentioned in the text.

3. Use of ECMWF data
   Which kind of ECMWF data is used and how? Is there a mixture of analysis data (available every 6 hours) with short term forecasts? Please explain this in more details.

4. Measurements of temperature during ML-CIRRUS
   As far as I remember, during ML-CIRRUS temperature profiles were measured with the MTP instrument. Why do you not use these measurements instead of coarse resolution ECMWF data?

5. Page 4, lines 32-33: Reference for sufficient amount of solution droplets
   A suitable reference for the occurrence of sufficient solution droplets, i.e. sufficient soluble aerosol particles, would be Minikin et al. (2003).

6. Page 5, lines 1-5: Representation of homogeneous nucleation
   The representation of homogeneous freezing of solution droplets and the derivation of freezing thresholds is very short and misleading for non-experts; it should be expanded. The volume nucleation rate depends on water activity, i.e. $J = J(\Delta a_w) = J(RHi, T)$ and the nucleation rate ω is composed by using the volume of a solution droplet $V = \frac{4}{3}\pi r_0^3$ with a size $D = 2r_0$, i.e. $\omega = JV$. Koop et al. (2000) made the (arbitrary) setting of $\omega = 1\,\text{min}^{-1}$, which means that all solution droplets of radius $r = r_0$ freeze within a timestep of one minute ($\Delta t = 1\,\text{min}$) with a probability of $P = 1 - \exp(-\omega\Delta t) \approx 0.63$. However, the choice of ω is quite arbitrary and should be mentioned, while $D = 2r_0 = 0.5\,\mu\text{m}$ might be a reasonable choice of a typical size. This should be mentioned in the text.

7. Page 9, line 4: Gravity waves are not really small-scale dynamics
   The statement of gravity waves as small-scale dynamics is a bit weird and should be rewritten; maybe mesoscale dynamics is a better classification, since small scale is more associated with turbulence.

Technical comments

1. The colour bars in almost all figures are not easy to read. Especially for figures 4 and 6, colour bars with more colours and/or clearer increments should be used.

2. In figure 5, the difference between regions HET and HOM cannot be seen, since the colours are too similar.

3. In figure 6 the trajectory could also be shown for clarification of the derivation.

References

Minikin, A., Petzold, A., Ström, J., Krejci, R., Seifert, M. and co-authors. 2003. Aircraft observations of the upper tropospheric fine particle aerosol in the Northern and Southern Hemispheres at midlatitudes. Geophys. Res. Lett. 30, 1503. DOI: 10.1029/2002GL016458.

Wernli, H., M. Boettcher, H. Joos, A. K. Miltenberger, and P. Spichtinger, 2016: A trajectory-based classification of ERA-Interim ice clouds in the region of the North Atlantic storm track. Geophys. Res. Lett., 43, 6657-6664, doi:10.1002/2016GL068922.