Review of

Investigating the Development of Persistent Contrails in Ice Supersaturated Regions with Cloudy Backgrounds Using ICON-LEM

by Sajedeh Marjani et al.

General comment

Persistent contrails might have an impact when placed into pre-existing ice clouds, changing the environment and the characteristics of the clouds and thus the life cycle of ice clouds. However, the qualitative and quantitative effect of such interactions is unclear. This study aims to investigate and to quantify the interactions of contrails and cirrus in the ICON-LEM model, thus providing important insights for anthropogenic and natural ice clouds.

However, in the current state this study is not appropriate for considering publication in ACP. Actually, I recommend rejection of the manuscript, and maybe reconsideration after a real major revision. In the following I will only explain my major concerns, but I will not go into details of the representation.

Major issues

1. The used model configuration is not appropriate for the investigation

   There are several issues with the chosen model configuration, which essentially makes it very difficult (or even impossible) to determine the properties of contrails embedded into ice clouds, and their interactions.

   Generally, I have to say that the model description lacks of many details. For instance, it is not stated how ice crystals in contrails are treated, also in contrast to crystals in natural clouds; the choice of crystals' shape would change processes as evaporation/growth and also sedimentation. Thus, many important settings or choices for the parameterisations are just not stated.

   - Classes of ice

     For a meaningful investigation of contrails and embedded in ice clouds, one has to use separate classes of ice, i.e., one for natural ice clouds (formed by homogeneous/heterogeneous nucleation and other processes) and one for the placed contrail. From the very brief description of the cloud physics scheme I can only assume that the authors use just one class of ice. This is a major drawback, since key features cannot be diagnosed anymore (e.g. sizes of contrail ice vs. natural ice) or even worse, processes and process rates are artificially changed. For instance, once the contrail (with a number concentration of $N \sim 150\mathrm{cm}^{-3}$) is placed into the ice cloud, the mean quantities are completely changed, since two very different modes are mixed. Assuming a natural cloud with small ice number concentration and large crystals (thus sedimenting ice particles), the embedded contrail would immediately lead to very small ice particles in the respective grid cell (by adding concentrations), which cannot sediment. Thus, sedimentation is artificially switched off just by placing the contrail. The same issue occurs for growth/evaporation of crystals, which is completely changed, but for all ice crystals, not only for the contrail part. Therefore, a model which does not distinguish between different ice classes with very different modes (small/many crystals vs. few/large crystals) is not suitable for such investigations.

     I suggest to implement a two-class ice scheme, based on the existing ice particle class in ICON. Otherwise, all investigations are biased by this artificial mixing of two very different ice classes, and thus not really meaningful.

   - Parameterisation of ice physics

     The description of the ice physics parameterisation is very short, the authors only refer to the standard two moment scheme by Seifert & Beheng (SB06), extended by the Kärcher et al. (2006) scheme (and the Phillips 2008 scheme). While the SB06 scheme is standard and was used for the ICON-LEM before, it seems that the ice physics scheme was extended using the Kärcher scheme.

This scheme was developed for coarse resolution models (e.g. climate models) with a typical time step of about 30 min (as they stated in their article). It is not clear to me if this scheme is really appropriate for the high resolution model with a time step of 2 seconds. Actually, I would assume that for such a small time step one can resolve nucleation events directly (maybe with some time sub stepping).

The authors should check and explain if and why the Kärcher scheme really works for their high resolution approach, and might consider to implement a nucleation parameterisation resolving the nucleation events.

- resolution of the model

  The authors claim that they will use a high resolution for resolving contrails and their interaction with natural cirrus. First, they never state clearly which resolution they use. For the horizontal grid spacing two different numbers are stated here and there (154m vs. 156 m), but for the vertical the resolution is not clearly stated, a number of approximately 150m is reported for the utls region. This raises the question why they use the same resolution for the horizontal and the vertical extension. Since the atmosphere is anisotropic, one would expect a finer resolution in the vertical. Or putting the other way round: For resolving a contrail with a vertical extension of about 500m at the initial state of placement in the model, one would have expected a finer resolution, while for the horizontal extension the resolution seems to be OK.

  The authors should explain their choice of resolution and maybe change it to a more appropriate setting.

2. Basic physics processes

   The investigation and evaluation of the model results are on a very descriptive level, but lacks of using basic physics for the quantitative inbestigation. Actually, there are only few physical processes for driving the system ice clouds, and for all of them there are characteristic features. The processes are

   - nucleation (i.e. formation of natural ice particles)
   - growth and evaporation
   - sedimentation

   and as a forcing the vertical upward motion leading to adiabatic cooling and thus to a source (or even a sink) of supersaturation. After nucleation/placing the contrail only the processes growth and sedimentation play a role. For growth/evaporation one can easily determine time scales, as e.g. derived from the mass growth equation $dm/dt = c(S_i - 1)m^{\frac{1}{3}}$. Assuming a constant supersaturation one can solve the equation analytically, or estimate the growth time scale directly. For sedimentation, the (mean) size/mass of the ice particles determines the terminal velocity (here, one would need to know the shape of the particle) and thus one can estimate typical fall distances in the model (as compared to the vertical thickness of the layers). Finally, the vertical upward motion directly determine the sources/sinks for supersaturation, thus one can diagnose directly if the crystals should shrink or grow.

   I completely miss a quantitative analysis using such basis physics consideration. Actually, all features explained in a very descriptive manner in the different simulation can be explained just by the basis processes, e.g., if there is a vertical updraft from below, there is a source of supersaturation, leading to growth of ice particles (no evaporation at the level) and thus enhanced sedimentation – large particles can fall to lower levels, since they can survive longer in subsaturated air . . . and so on.

   All the evaluations should be based on quantitative investigations using time scales and other key features of the physical processes.

3. Simulation scenarios

   The authors use 8 different scenarios, which are completely simulated using the nested approach and all the investigations. However, it is never stated anywhere, why they choose these cases. For me it is completely unclear what the authors want to show or investigate with these different cases. In consequence it remains completely unclear what differences in the meteorological conditions have an influence on the life cycle of the contrail- ice cloud system. This kind of arbitrary choice is reflected in the description of the results. There is not really an attempt to make a kind of generalization of the features one have found in the different cases.

The authors should rethink their choice of simulation scenarios, also in terms of evaluations of the results using the physics based investigations as mentioned above. Maybe it would be better to use more idealized scenarios in order to determine the impact of contrails under certain, clearly prescribed, conditions, before using realistic scenarios.

4. Presentation of results

   It is really difficult to understand what the key findings of the study really are. The different simulations are described with plain text and some numbers in the text, however, a clear picture is lacking. The figures are also not really convincing. For instance, it is not really appropriate to use model levels (or even difference in model levels) as vertical coordinate.

   The whole section of results should be rewritten along the main physical features (guided by the physical processes and their interactions) in order to highlight the major results.