Review of

Ice-supersaturation and the potential for contrail formation in a changing climate

by Irvine and Shine

General comment:

In this study the change of ice supersaturation in the tropopause region due to climate change is investigated. For this purpose CMIP5 model output is used; for consistency, ERA interim data are investigated and compared with model results for the present day period 1979-2005. Changes in ice supersaturation frequency are presented and discussed for different regions (polar, midlatitudes, tropics). Generally, this is an interesting topic and the study provides new and interesting results about the change of ice supersaturation in the tropopause region. Therefore this manuscript is a suitable contribution for Earth System Dynamics. However, several issues should be clarified before the manuscript can be accepted for publication in ESD. Therefore, I recommend (major) revision of the manuscript. In the following I will explain my concerns in detail.

Major points

1. For the investigations the authors use just daily data for one pressure level (250hPa) for the investigation of ice supersaturation in the tropopause region. They argue that most of the relevant flights will occur around this pressure level. There are at least three concerns, which should be discussed by the authors:
   (a) It is not clear how the pressure level 250hPa is represented in the model data. Obviously, the model levels will have a certain extension representing a vertically thick layer. The authors should indicate which vertical extended layer is represented by the level 250hPa; is it a layer centred at $p = 250$hPa with vertical extension of 50hPa (since they indicate other pressure levels as 150, 200, 300hPa, etc.), i.e. representing the range $225 - 275$hPa?
   (b) From MOZAIC/IAGOS measurements (see e.g. `http://www.iagos.fr/web/`) it is known that a large portion of long-distance flights is located in the range $p < 250$hPa or even in the range $p < 220$hPa. Thus, an investigation of pressure level 250hPa might just give a part of information relevant for contrail formation. The authors should think about extending their study including the pressure level 200hPa, since most of the relevant long-distance flights would be covered by these two levels. Of course, the question about the vertical extension of the pressure layer is related to this issue.
   (c) The use of daily data might also cause some underestimation of ice supersaturation frequency. Our knowledge about life cycles of ice supersaturation is quite limited. It is often assumed that large scale dynamics with time scales of days triggers ice supersaturation in the tropopause region. However, recent studies (e.g. Irvine et al., 2014) indicated that Lagrangian life times of air parcels in supersaturated conditions might be smaller than 24 hours. Thus, the authors should describe carefully, how this influences their investigations; probably, just a lower limit can be derived from their evaluations. A similar issue constitutes the use of monthly mean data for other vertical layers.
2. The temperature criterion for the definition of ice supersaturation seems a bit artificial and might lead to artificial biases. It is true that the temperature limit of $T = 233K$ coincides almost with the Schmidt-Appleman criterion, although the limits would be possibly situated at lower temperatures (see e.g. Gierens et al., 1997, figure 1). However, for the pressure level of 250hPa I would expect such low temperatures (i.e. $T < 240 - 245$K) that the frequency of occurrence for pure supercooled water should be very small if not almost zero (see e.g. Pruppacher and Klett, 2004, fig. 2-33). The introduction of the temperature criterion could result into an artificial bias for the data evaluation, as already indicated by the authors. Since some models seem to tend to higher temperatures in the tropopause region, the frequency of occurrence for ice supersaturation could be masked by the temperature criterion. Thus, it is not clear how robust the results are. Therefore I would suggest additional evaluations:
   (a) The authors should carry out the same data evaluation with no temperature criterion or with a changed criterion (e.g. setting the threshold to $T = 238/243$K). This should provide a hint about the robustness of the results. The existence of ice supersaturation is not only important

for persistent contrails but also for the formation of natural clouds, thus investigations without a temperature threshold would provide additional information.

(b) If the authors would prefer to stay with the temperature criterion of $T < 233$K, they should introduce a second data category, i.e. $T \geq 233$K and carry out the same investigations for this category (maybe with the additional constrain of $T < 243$K or similar constrains to avoid liquid water). This would give an answer about the robustness of the results, too. In addition, they could study the transition between the two cases, which would also provide additional information about potential contrail formation (concerning the Schmidt-Appleman criterion).

3. The authors discuss the results in a quite qualitative manner. However, the origin for changes in relative humidity and thus in the frequency of occurrence of ice supersaturation remains unclear. The authors should try to investigate, which variables contribute to increase/decrease of ice supersaturation dominantly. For instance, it is not clear if changes in temperature or in specific humidity contribute most to changes in ice supersaturation. It is not clear to me, if the available data is good enough for investigating such quantitative issues, but the authors should at least comment on that issue.

Minor points:

1. The representation of the thermal tropopause is usually not very good in climate models. Actually, the vertical gradients are usually weaker than in nature due to coarse resolutions. Thus, it is not clear to me how a misrepresentation of the tropopause height in the models might influence ice supersaturation in the tropopause region. Maybe the impact is not that strong, but it is not clear at all. The authors should discuss this issue in more details, regarding the quality of representation of this transport barrier in climate models.
2. A more quantitative evaluation of the 2D distributions of annual ISS frequency should be carried out (figure 2).

Technical comment:
The colour bar for figure 2 is very hard to read. Please change it by including more colours for a better discrimination of ISS frequency.

References

Gierens, K.M., U. Schumann, H.G.J. Smit, M. Helten, G. Zängl, 1997: Determination of humidity and temperature fluctuations based on MOZAIC data and parametrization of persistent contrail coverage for general circulation models. Ann. Geophys. 15, 1057-1066.

Irvine, E. A., B. J. Hoskins, and K. P. Shine, 2014: A Lagrangian analysis of ice-supersaturated air over the North Atlantic. J. Geophys. Res. Atmos., 119, 90-100, doi:10.1002/2013JD020251.

Pruppacher, H. and Klett, J. 2004: Microphysics of Clouds and Precipitation, Kluwer Acad. Pub., Dordrecht, 954 pp.