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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Probabilistic description of ice-supersaturated layers in low resolution profiles of relative humidity" *by* N. C. Dickson et al.

Anonymous Referee #2

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The authors examined a large set of corrected high resolution radiosonde data from different locations in the UK and abroad in order to derive a relation between the mean humidity in thick pressure layers in the upper tropopause and the occurrence probability of ice supersaturated sublayers. This relation may prove very useful in determining the occurrence of ice supersatured regions (ISSR) in numerical weather prediction and climate models and is a useful contribution to the prominent topic of ice formation in the upper troposphere.

I suggest to publish the paper with only some minor issues that the authors may want to consider:

- The abstract seems a bit too long. I suggest to leave a large part of the first

  paragraph, approximately from line 6 to 12 to the introduction.

- Section 2.2, p. 2362 l.12 ff. I do not understand why the authors did not derive the tropopause altitude from the radiosonde data (i.e. the temperature profile) themselves, which is a straightforward task. This would significantly improve the analysis since the quality check that were carried out would be more robust and could also be applied for the tropics.

- Section 5.3, p. 2370 line 2: It is not quite clear to which entity the figures are related to.

- Section 5.4, p.2371, l.13: The tropics are generally defined as the region between the tropic of cancer (23.4°N) and the tropic of capricorn (23.4°S). I suggest to use these figures instead of -30° to 30°.

  Climatologically, things are more complicated since the border between tropical and arid zones are not at a constant latitude. This is of particular importance with regard to the discussion of the data from St. Helena, since there is a large number of observations suggesting that the frequency of occurrence of ice clouds and ISSR in the tropical tropopause layer is particularly high (e.g. Winker Trepte, GRL, 1998, 25, 3351-3354, Sandor et al. GRL, 2000, 27, 2645-2648, Immler, et al. J. Geophys. Res., 2007, 112, D03209) in contrast to the results reported in the current paper. It would be interesting to know whether this is because St. Helena's climate is dominated by subsidence (arid climate zone) and is therefore low in water vapor up to the tropopause, or whether this is a problem of the radiosonde data or the analysis algorithms. In the first case the paper would greatly benefit if data from a truly tropical station was used. In any case the authors should be more cautions with the conclusion that the derived S-shaped function is applicable for all climatic regions.

- Fig. 9 2nd line: replace '(' with 'in '

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2357, 2010.