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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Effect of gravity wave temperature fluctuations on homogeneous ice nucleation in the tropical tropopause layer" *by* T. Dinh et al.

Anonymous Referee #1

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1 General comments

Measurements of ice crystal number concentrations in the TTL cirrus usually yield values that are substantially lower than a theory based on the assumption of homogeneous nucleation would predict. This so called "ice nucleation puzzle" (Spichtinger and Krämer, 2013) can be solved by assuming temperature fluctuations (caused by fluctuations of the vertical wind) with time scales similar to the nucleation time scale (e.g. triggered by gravity waves). So far, simulations using idealistic temperature time series have been used to demonstrate this. The present authors want to go a step

further and use measured time series of temperature. I endorse this goal.

The balloon measurements from which the time series are obtained, must be filtered at the high frequency (short period) end, at a period of 10 min. That is, processes that are faster, cannot be treated with this method. Unfortunaltely, homogeneous nucleation is such a quick process and to my opinion the authors miss their goal.

It seems, however, that the authors found a trick to circumvent this problem, namely to choose an extremely low nucleation threshold. This trick works insofar as it extends the nucleation time scales to a few minutes up to an hour (sect. 4.3.1). However, this is achieved only for a high price. Usually the threshold is chosen in a way that the nucleation rate is practically zero below the threshold and many orders of magnitude larger above it. In this paper the nucleation rate at the threshold and some percent above is practically zero as well (see Figure 3). It seems that this makes results differing from corresponding results from other papers, qualitatively and quantitatively. This choice of threshold and the consequent differences from results from other papers are not discussed at all; instead the authors claim consistency with other results, a view that I cannot support.

My recommendation is therefore to accept the paper only after a major revision (addition) where the authors demonstrate either that their nucleation results are similar and consistent with those of other authors (e.g. Kärcher and Lohmann 2002, Spichtinger and Gierens 2009) or that those other results are wrong.

This is a pity, because the paper does contain an interesting concept, i.e., the distinction between vapour- and temperature-limited nucleation events. I like also the analytical derivation in section 5.

2 Major comments

1) P. 8777, l. 20: I am surprised of the low critical supersaturation that you assume at 195 K. Looking at Fig. 3 of Koop et al. (2000) it seems that the critical supersaturation at 195 K is much higher. Using Eq. 4 from Kärcher and Lohmann (2002) I calculate $S_0 = 1.645$.

2) Figure 2: It might be that the low critical supersaturation or your assumption of a monodisperse aerosol leads to a much higher sensitivity N_i vs. w. From Kärcher and Lohmann (2002) I assume that $N_i \propto w^{3/2}$ in most cases. Figure 2 shows a relation that is rather $N_i \propto w^{5/2}$ for low w. Also the number of ice crystals is much (factor 30 or so) larger in your model than for instance in Kärcher and Lohmann or Spichtinger and Gierens (2009, Fig. 7, top panel). These differences require an explanation.

3) Figure 3: To my opinion we see her another strange result of the choice of an extremely low nucleation threshold. As the top right panel shows, we are above the nucleation treshold from t_0 on, but it needs 12-13 min before the curve in the bottom panel indicates an N_i of 0.001 per liter, and it takes still 10 and more minutes until all ice crystals are formed. The simulations suggest that ice formation occurs on a time scale of half an hour or so. Compare this to figure 1 from Spichtinger and Krämer where a time scale of 140 sec is indicated. How can you state that these results are consistent?

3 Minor comments

1) P. 8774, l. 8/9: "whole equatorial area" sounds exaggerated considering that there are only 2 balloons.

2) Eq. 1: R should be R_a.

3) Section 3, par. 4: Please explain why sedimentation would reduce INC. If crystals

get lost from the parcel by sedimentation, another nucleation event could occur earlier than without sedimentation. Why should this not happen?

4) Beginning of sect. 4: I do not understand how you can mention adiabatic motion and pressure variations in the first sentence, and assume constant air pressure in the second. Does "constant pressure" just mean that your parcels are sufficiently flat?

5) Figure 6 and sect. 4.3.2: It is not easy to understand why N_i (210 K) is higher than N_i (180 K) as a function of $S_{max} - S_0$ and vice versa as a function of $T_{min} - T_0$. A more detailed explanation would be welcome.

6) Eq. 19 and following text: if t^* is the point in time where $J = J_{max}$ and $S = S_{max}$, then dS/dt should be zero.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 8771, 2015.