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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 #3

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General comment:
In this study the impact of high-frequency gravity waves on homogeneous freezing of solution droplets is investigated. The authors use measurements from isopycnic balloon flights for deriving realistic trajectories of air parcels. They run a bin microphysics parcel model along these trajectories in order to reproduce the (very) low ice crystal number concentrations in the tropical tropopause layer. In addition they investigate the underlying equations for ice nucleation analytically in order to provide theoretical explanations of the successful numerical calculations.

This is an appropriate contribution to ACP, especially the theoretical considerations are

interesting and give a deeper insight into the issue of ice nucleation at TTL conditions. However, some major issues must be clarified before this manuscript can be accepted for publication. Therefore I recommend major revisions of this manuscript. In the following I will explain my concerns in detail.

Major points

1. There is a fundamental error in the equation (17). As far as I can see from the original reference, the logarithm of the nucleation rate is represented by a third order polynomial in $\Delta a_w = a_w - a_w^i$ and NOT in $S \cdot a_w$ (at least with the implicit definitions of a_w and S). It is clear from simple calculations that $\Delta a_w \neq S \cdot a_w$, thus the derivation of the equations (18) - (20) is incorrect. Probably, it is possible to rederive similar expressions; however, in the present state the calculations are wrong. I have to express here that the main conclusion of the study remains unchanged, although the theoretical interpretation must be clarified.

2. Although it is stated that beside the reference simulations with $T_0 = 195$ K simulations with $T_0 = 180$ K and $T_0 = 210$ K were carried out, the representation of the results is quite minimalistic; they are just shown in figures 5/6. Maybe you should try to present the resulting (low!) ice crystal number concentrations in a kind of statistical matter. In addition you should try to scan the parameter space (T_0, p_0) in a bit finer resolution in order to have a better representation of the realistic cases. You should also try to compare these results at least qualitatively with measurements (e.g. as shown in Spichtinger & Krämer, 2013).

3. A major issue for the formation of low ice crystal number concentrations in the study by Spichtinger & Krämer (2013) was the occurrence of very slow background updrafts on order $O(0.01\text{ms}^{-1})$. You should try to use such background velocity fields for your realistic trajectory simulation.

Minor points:

1. There should be more accurate definitions of the used quantities. For instance, S is never defined although I assume that S is the saturation ratio with respect to ice. Also the scale height is not well-defined, is it the usual value $H \sim 8$ km?
2. Page 8774, line 16: How reasonable is the time resolution of the trajectories in order to get a good representation of the relevant small-scale gravity waves? Please explain this in relation to the frequency of gravity waves, which might be expected.
3. Page 8775, lines 5-7: Which resolution of ECMWF data is used for deriving the background temperature profile? Is it good enough for your considerations?
4. For the background aerosol (heterogeneous IN concentrations, aerosol particles for homogeneous nucleation) you often quote measurement studies, which were carried out mostly in the extratropics; since you want to address tropical tropopause layer, you should make clear that these measurement values are also reasonable for this tropical study.
5. Sedimentation of ice crystals is not just the effect of removal of ice crystal number concentrations; in combination with other processes (nucleation and diffusional growth) a kind of dynamic equilibrium might occur (see e.g. investigations by Spichtinger & Cziczo, 2010 or Wacker, 1995). Probably for your simulation it is ok to omit sedimentation, but you should motivate this in a more convincing way, e.g. arguing about terminal velocities of very small ice crystals.
6. The accommodation coefficient for the reference case seems a bit low ($\alpha = 0.05$); Skrotzki et al. (2013) indicate that the usual values are more in the range ($0.1 \leq \alpha \leq 1$). How large is the difference in the simulations between e.g. $\alpha = 0.5$ and simulations with $\alpha = 0.05$?

7. I think, your lower boundary of the nucleation rate J_ϵ is quite small, i.e. the probability of freezing a typical solution droplet at these conditions is probably zero (with respect to machine epsilon). It would be nice to add the time evolution of the nucleation rate for the different scenarios in figure 3; this would help to understand why the ice crystal number concentration is changed that drastically.
8. Page 8780, lines 14-23: I not understand what you want to say, please explain this in more details.

Technical comments

The figures are quite hard to read. Actually, the figure captions could be extended. In figure 3 the different curves (all represented with the same colour, i.e. blue or red) should be labelled.

References

Spichtinger, P. and D. J. Cziczo, 2010: Impact of heterogeneous ice nuclei on homogeneous freezing events in cirrus clouds. J. Geophys. Res., 115, D14208, doi:10.1029/2009JD012168.

Wacker, U., 1995: Competition of precipitation particles in a model with parameterized cloud microphysics, J. Atmos. Sci., 52, 2577-2589.

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