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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 #2

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This work uses a parcel model to study ice nucleation of ice crystals at conditions relevant for cirrus formation. The authors perform a set of simulations forced by observed vertical velocity time series obtained from balloon measurements. In agreement with other studies, the authors suggest that non-persistent cooling rates may lead to low concentration of ice crystals even if homogeneous freezing is the dominant mechanism of ice formation. They also suggest that the ice crystal concentration is determined by the absolute drop in temperature rather than the cooling rate. The paper is well written and the subject is of interest to the atmospheric community. However the authors base their conclusions on a very limited representation of the cloud evolution. The condi-

tions for which their analysis applies are also overly restrictive and it is not clear that their study can be used to understand the evolution of real clouds. It does not seem that these issues can be easily resolved still maintaining the conclusions of this work. Therefore I do not recommend this paper for publication in ACP.

1 General Comments

The authors carefully set up their simulations so that the vertical velocity changes sign at least once before substantial nucleation rates are reached. This is a clever way to show their expected result but begs the question of how realistic the setup actually is. As noted by another reviewer, if the filtering of the balloon measurements is applied differently, or if higher temperature fluctuations with periods below 10 min are allowed, the simulations may result in higher number concentration. In fact the authors claim that they can obtain small crystal number even for high vertical velocity, something that is never shown.

A related issue, and maybe the most significant one, is the selection of the initial conditions. All runs start on the verge of ice nucleation $S_0 \sim 150\%$. It is unrealistic to assume that each parcel starts from a very high supersaturation. One may ask, how do these parcels become such highly supersaturated in the first place? Starting from ($S_0 = 100\%$) would any of the vertical velocity time series tested result in cloud formation? From the shape of the temperature perturbation profiles in Fig. 4 it seems that they wouldn't. In reality there must be some underlying vertical movement bringing the supersaturation up to the initial conditions selected by the authors. Such movement (disregarded by the authors) is the actual driver of cloud formation, not the superimposed vertical velocity fluctuations. The analysis based only on the latter is flawed.

The authors omit important works (and in fact repeat some of the conclusions of those works) that may have helped in their analysis (e.g., Barahona and Nenes, 2011;

Jensen et al., 2010, 2012; Cziczo et al., 2013; Murphy, 2014; Shi et al., 2015). For example just as in this work, other works have shown (e.g., Jensen et al., 2010) that homogeneous nucleation could produce both, low and high ice crystal concentration. Similarly, field campaigns (e.g, Krämer et al., 2009) show high and low number concentration of ice crystals. Any comparison between field campaign data and model results should be done on a statistical basis. A limited set of parcel model simulations over very restricted conditions should not be used to draw conclusions on real clouds. Other aspects of the problem should be evaluated as well. Could the authors setup not only reproduce low crystal numbers but also the sustained clear sky supersaturation and the small ice crystal size of TTL cirrus?

2 Specific Comments

Line 11, page 8771. Such high vertical velocities are not shown.

Line 15, page 8771. This conclusion has been already stated in several papers (e.g., Barahona and Nenes, 2011; Jensen et al., 2010; Murphy, 2014).

Line 5-10, page 8773. A concentration of 100 L $^{-1}$ is just a nominal number, not a threshold that represents a limit between homogeneous and heterogeneous ice nucleation. Further evidence of the predominance of heterogeneous ice nucleation comes from field campaign data (e.g., Cziczo et al., 2013).

Page 8774-8775. The authors should show a plot of the vertical velocity time series associated with these measurements. Also explain why measurements from only two balloons are assumed as representative of the dynamics of the TTL.

Page 8776 Lines 15-20. This is an important issue. Many interesting dynamics occurs from the sedimentation of ice crystals (e.g., Barahona and Nenes, 2011; Murphy, 2014). In particular, sedimentation would allow the build up of enough supersaturation

for homogeneous ice nucleation to occur. Thus the assumption that sedimentation would further decrease ice crystal concentration is erroneous.

Page 8777 Lines 1-5. Water vapor variability does not necessarily result from temperature fluctuations. In fact field campaigns have shown that temperature fluctuations are only partially responsible for the generation of supersaturation in the TTL (Diao et al., 2014).

Page 8779, Section 4.2. It is not clear how supersaturation can be generated in the first place without some persistent cooling (see general comments).

Page 8784, Eq. (20). It must be mentioned that this is only true for negligible ice crystal concentrations. In reality what the authors are defining as "temperature-limit" events is just a low ice crystal concentration regime, and has been introduced before (Kärcher and Lohmann, 2002).

Page 8786, Line 20-25. According to this, the processes bringing up supersaturation to the level used in the initial conditions are the actual control of ice nucleation (see general comments).

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