Comments on “Effect of gravity wave temperature fluctuations on homogeneous ice nucleation in the tropical tropopause layer” by Dinh et al.

I appreciate the answer to my early comments on the paper. However I feel the changes are not substantial enough to help the reader understand the idealized conditions of the simulations. More clarification is required. Also I feel that that some of the author’s responses are not well represented in the revisited paper. Particularly I am concerned about the evolution of vertical velocity and the assumptions behind the theoretical development. My comments below refer to the revisited version of the paper.

In their response the authors assert that all simulations started at $S = 100\%$, and the system is brought to $S \sim 150\%$ by large scale movement. The only way I see this happening in Figure 6 is that the vertical velocity is constant for hours until the selected saturation threshold is achieved, then fluctuations appear resulting in ice nucleation, and after a few minutes, both fluctuations and the large scale vertical movement disappear. If this is the case it must be shown explicitly and justify why it is representative of the conditions in cirrrus. Such highly idealized velocity evolution does not seem particularly realistic. Also, are Figures $7-9$ produced using the same vertical velocity series as in Figure 6? Please explain.

Why do the authors stop their simulations after a few minutes (indeed some of the lines extent for 30 s only)? In most of temperature limited events in Figure 6 further nucleation events would take place since the ice crystal concentration is small. If that is not the case it must be shown explicitly, i.e., extend the simulation period to show that no further nucleation events take place.

In some of the answers to earlier comments the authors stated that:“ The updrafts used in our simulations are derived directly from the observed balloon temperature time series, and representative values are shown in Fig. 3 in our reply to Reviewer 1. They include both high-frequency motions with periods of several minutes (referred to as the wave component in Spichtinger and Krämer, 2013), and longer timescale motions (periods of several hours) that correspond to the large-scale component in Spichtinger and Krämer (2013)."

This must be shown explicitly. Show that the average vertical velocity of your simulations is indeed positive and not additional background ascent is required. My fear is that extended for a longer period of time the blue lines in Figure 6(a) would indeed result in net large scale ascent, but the red lines in a large scale downdraft. In such case the temperature limit would apply where both the fluctuating component of the velocity and the large scale ascent change sign during ice nucleation (since net ascent is required to generate $S \sim 150\%$ at t_0); a rather improbable situation.

The assumptions behind the theoretical development should be better stated. The theoretical results only apply to cases where the ice crystals are not efficient at removing supersaturation. This can be seen in the cases where the growth rates are substantial ($\alpha = 1$, higher temperature, high N_i) and the numerical model and the theoretical results diverge. It is clear in the development of Eq. (13) (the symbol in that equation must be $\approx$ instead of $=$) that both μ and J depend on S which can be reduced by growing ice crystals. Therefore they are constant only if the ice crystals do not reduce S (J changes several orders of magnitude over very small ΔS so any factor afecting S would have a big incidence on N_i). Later in section 5.2 it is mentioned that the relationship N_i vs. ΔT is independent of α which is of course not the case since α indirectly affects S.