

# Response to reviewer's comments

by P. Spichtinger and M. Krämer

First of all, we would like to thank both reviewers for their helpful comments, which lead to an improvement of our manuscript.

## 1 General comments

- We have rerun all simulations because of new results indicating a larger vertical extension of the cloud layers as we have assumed before (Jensen et al., 2013). Thus, we now have chosen a cloud thickness -or equivalently vertical extension of the box- of  $\Delta z = 200$  m instead of 50 m. The quality of the results did not change and the values in the  $N_i$ -pdfs differ only marginally.
- We added some references indicating that the existence of high-frequency waves with intrinsic frequencies can be corroborated by stratospheric balloon measurements (Vial et al., 2001; Hertzog and Vial, 2001).

## 2 Response to reviewer 1

1. Most generally, I am concerned that the waves are 'tuned' to be 'just right' for the model, putting it in a special situation where variability may cause the desired 'shutdown' behavior. What happens if a larger parameter space is used? Also, are there other impacts of either mixing (in the physical sense in an air parcel undergoing turbulence) or sedimentation that might affect the results? What limitations of the model might affect the results?

Actually, the waves were not tuned for the purpose to fit the pdfs as good as possible. We found the mechanism of stalling the nucleation events in idealized simulations by accident. The scenarios or better to say the parameter space of the dynamical features was then set to represent the conditions in the TTL as good as possible; we then found a quite good representation of the measurements from simulations with pure homogeneous nucleation (see fig. 8). Finally, our weighting procedure leads to a kind of best fit.

Beside the dynamical setup, which is close to the realistic situation in the TTL (see also added references, e.g., Hertzog and Vial, 2001) we could change the sedimentation setup; however, even if we change (1) the vertical extension of the cloud layer and/or (2) the sedimentation factor to very small numbers, the qualitative results are still the same. Actually, values  $f_{sed} = 0.5/0.9$  are representative for the cloud top layer and the middle cloud layer (see argumentation in Spichtinger and Cziczo, 2010), thus we stay with this parameter.

Mixing of air parcels, e.g. due to horizontal motions could lead to further reduction of ice crystal number concentrations, since already formed ice crystals could be spread into regions, which did not experience ice nucleation before. Thus, the spreaded ice crystals will consume the water vapour, thus quenching ice nucleation. This phenomenon as described in former studies (Spichtinger and Gierens, 2009b); the same feature could be seen if ice crystals are sedimenting from clouds formed above the considered cloud free air masses. These effects tend to decrease ice crystal number concentrations. Since it is not possible to investigate these effects in our setup and because they will decrease instead of enhance the formed ice crystal number concentrations, we will not discuss these effects further. We have added some text in the discussion about these two effects.

2. Some minor clarifications are necessary as noted below, and a slightly more robust treatment of the 'fitting' used to account for the quantitative relative magnitudes is appropriate as well.

We have added some text to clarify the approach of using a composition of different scenarios in order to represent the measurements with a even better agreement. However, we did not change our simple approach of choosing the weighting coefficients ‘by eye’, since we know from other studies that this approach leads often to very similar results than using sophisticated optimization techniques.

3. [Page 28110, L14: Wording here in the abstract is not exactly clear. Please clarify. "mixed" seems awkward.](#)

We re-wrote the abstract to clarify our intention and our method.

4. [Page 28110, L24: Cooling what? Surface temperature? Locally in the stratosphere?](#)

Actually, a increase in stratospheric water vapour would lead to (1) a cooling in the lower stratosphere and (2) a slight warming of Earth’s surface according to Forster and Shine (2002). We added some text for clarification and the reference.

5. [Page 28111, L8: Adiabatic cooling from upward vertical velocity...](#)

We re-wrote the sentence in order to include the temperature change resulting from vertical upward motions leading to adiabatic expansion and thus cooling.

6. [Page 28112, L24: "Is consistent" ? With what? Awkward phrasing.](#)

The microphysical parameterisations are developed in a consistent way, i.e. they based on general moment and the parameterisations of different processes (nucleation/growth/sedimentation) are consistent to each other. We added some text to clarify this issue.

7. [Page 28121, L10: Define low frequency here. What period, what amplitude updraught.](#)

Low frequencies are values  $\omega < 0.005 \text{ s}^{-1}$ . We added some text for clarification. Actually, this feature of producing high ice crystal number concentrations is already present for low temperature amplitudes in order of  $A_T \sim 0.25 \text{ K}$ .

8. [Page 28124, L5: What about observations of TTL vertical velocity from aircraft? Can you comment on how the distribution of your vertical velocities match them? Maybe a PDF?](#)

To our knowledge, there are no direct measurements of vertical velocities in the TTL available. Thus, we cannot compare our vertical velocity statistics with measurements. We added this statement in the text.

9. [Page 28125, L19: Can you comment on how tuned this makes the cases you are running? If you are not in this 'near saturation' space, then what happens in the model? No nucleation is one case, but in the higher nucleation case does the mechanism of superposition matter? It would be interesting to fill out the phase space a bit with the simulations.](#)

Actually, it does not really matter if the simulations start near saturation or at higher or lower relative humidity vales as long as nucleation thresholds are not immediately reached. In this case the boundary conditions of the setup would influence the results. We specified the environmental conditions in such a way that we precisely have nucleation at certain conditions, which are representative for TTL due to our climatology (as given in fig. 7). However, if we would change the intial conditions randomly such that nucleation will take place at other temperature/pressure combinations, which are still in the usual range for TTL, the results would not greatly change, neither qualitatively nor quantitatively. We have checked this by some former simulations. Thus, there is no need to enhance the parameter space.

10. [Page 28126, L3: Gravity waves 'lifted' by large scale motions? What does that mean?](#)

Large scale motions leads to slow vertical updrafts or more precisely to large scale lifting of whole layers of large vertical and horizontal extensions. Gravity waves are excited inside these layers and are travelling inside the layers (i.e. feeling environmental horizontal winds etc.). Thus, in our air parcel picture, the gravity waves, represented by sinoidal waves are “lifted”, resulting in our very simplified model simulation setup. We added some text to clarify this issue.

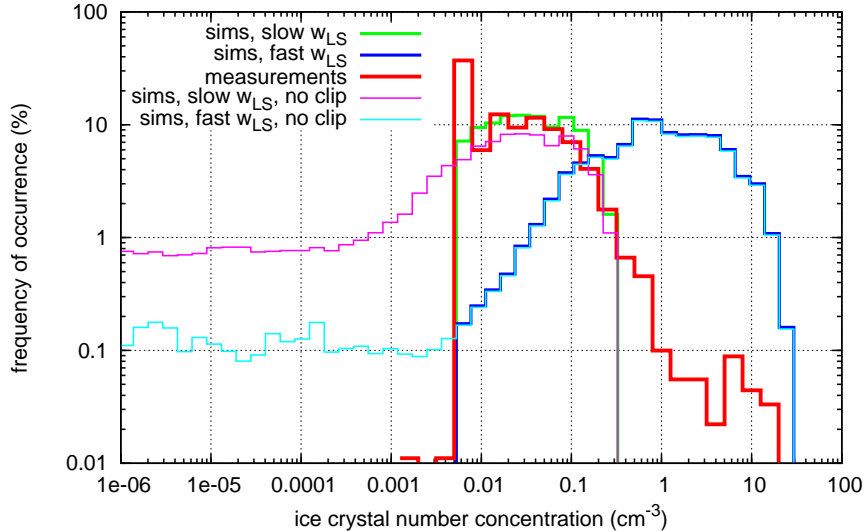
11. [Page 28127, L11: What are the resulting ranges of small scale vertical velocity?](#)

We added an appendix to report the maximum values of small scale vertical velocity, represented by their amplitudes  $A_w$ . Acually, the whole range of small vertical velocity can be seen from fig. 9. The

velocity pdfs are almost identical for the different scenarios ('slow', 'fast', 'all') because the large scale component is very small shifting the pdf only slightly.

12. Page 28128, L3: Might be useful to show the complete numbers as well (no clipping) since this affects the PDF.

There is an effect on the pdf, however, for comparison the clipped data is a better representation of what the instruments would measure. Thus, we prefer to stay with the clipped pdfs for the representation in the manuscript. Actually, there is a slight change for the 'slow  $w_{LS}$  pdf' but a negligible change for the 'fast  $w_{LS}$  pdf', see attached figure. We added some text about this feature.



13. Page 28131, L3: This logic is starting to be a bit circular. What does 'mixing' mean? See below. What are you mixing and how?

There is not really a circular logic. Actually, we have seen from our comparison of scenarios (e.g. 'pure homogeneous' vs. 'het/hom' for slow and fast large-scale updraughts) that (1) slow vertical updraughts lead to low number concentrations  $N_i < 0.1 \text{ cm}^{-3}$ , (2) the peak around  $N_i \sim 5 - 8 \text{ L}^{-1}$  could stem from simulations including heterogeneous nucleation and (3) high ice crystal number concentrations  $N_i > 1 \text{ cm}^{-3}$  must stem from high large-scale motions. Thus, we tried to compose the measured number concentration statistics by weighting the different components. We explain the 'mixing' in more details in the text.

14. Page 28131, L11: How did you choose the coefficients? What are you fitting exactly? Individual number concentrations? How is that 'mixed'?

We compose a new number concentration distribution by adding different scenarios with different weights; this means that we assume that all the simulated scenarios of pure homogeneous nucleation at slow large-scale motions occurs more often (i.e. relative frequency of occurrence  $p \sim 79\%$ ) than the other scenarios of heterogeneous/homogeneous nucleation at slow large-scale motions (i.e. relative frequency of occurrence  $p \sim 20\%$ ) as well as scenarios of heterogeneous/homogeneous nucleation at fast large-scale motions (i.e. relative frequency of occurrence  $p \sim 1\%$ ). We added some text to clarify this procedure. The choice of the weighting coefficients (or better to say the frequency of occurrence) was determined 'by eye'. Thus, this set of weighting coefficient is not unique; however, the features are quite robust (low concentrations from slow large-scale motions, high concentrations from fast large-scale motions, peak at  $N_i \sim 5 - 8 \text{ L}^{-1}$  from heterogeneous nucleation). Thus, it is very likely that a computational optimization would lead to a similar result. We added some text to clarify the details.

15. Page 28133, L1: Clarify: very small ice crystal numbers below  $0.01 \text{ cm}^{-3}$  due to hetero, moderate due to homogeneous and large due to homo with fast updrafts.

We slightly disagree with this statement, because very small ice crystal number concentrations could

also stem from pure homogeneous nucleation (see fig. 8). We added some text to clarify the interpretation of the weighting.

16. [Page 28134, L12: Restate the wavelengths.](#)

We follow the suggestion of the second reviewer in order to base our investigation more on frequencies than on wavelengths. Thus, we report here again the frequency interval  $\omega \in [0.005 \text{ s}^{-1} : 0.029 \text{ s}^{-1}]$ .

### 3 Response to reviewer 2

1. [p.28113, l.27: spontaneously](#)

Typo, changed.

2. [p.28114, l.4-5: repetition of the sentence in l.1-2](#)

This is not a repetition: In lines 1-2 we consider the nucleation rate for a solution droplet, which is dependent on temperature and water activity (i.e. relative humidity over water). In lines 4-5 we consider the Koehler equation, i.e. the equilibrium size of the solution droplets, growing from “dry” sulphate aerosol to aqueous solution droplets, depending on environmental conditions (i.e. temperature and relative humidity over water).

3. [p.28115: Using an adiabatic motion for the fast waves make sense but it is much less justified to assume that the slow large scale motion is adiabatic. As a matter of fact, the authors mention on p.28122 that the “ascents compensates diabatic heating” and on p.28123 that the stratification of the TTL is very high. Using a more realistic lapse would have perhaps a limited impact but this should be mentioned.](#)

The first effect of compensating diabatic heating leads to very small updraughts, thus these might be neglected. The second source for large-scale vertical motion is due to equatorial waves. Thus, for this component an adiabatic process can be assumed. Since the amplitude of these waves lead to higher vertical updraughts compared to diabatic heating, we might assume adiabatic processes for large-scale motions. Actually, this would be an upper limit for the vertical motions since a lapse rate with higher stability would lead to smaller vertical velocities, since then  $\frac{dT}{dz} > -\Gamma$  (or better  $|\frac{dT}{dz}| < \Gamma$ ). We have clarified this issue in the text.

4. [The authors should clarify how they characterize the amplitude of the wave. In the first part of the paper, they use vertical velocity but what an air box feels is not velocity or acceleration but pressure and temperature variations. The temperature variation is proportional to the ascent for stationary motion but the temperature range depends also on the frequency for a wave. Hence, the conclusion made at the end of section 5.2 that the vertical velocity does not matter to estimate ice crystal number concentrations is a somewhat straightforward consequence of the setup. In section 5 and the sequel, the amplitude of the wave is characterized by its temperature range. I would recommend to do it from the beginning.](#)

Thank you for the suggestion, we re-wrote the text for considering temperature (and pressure variations) instead of vertical velocity. Actually, this was already the way how waves influenced the air parcel or better to say the box model, but our description was somewhat misleading.

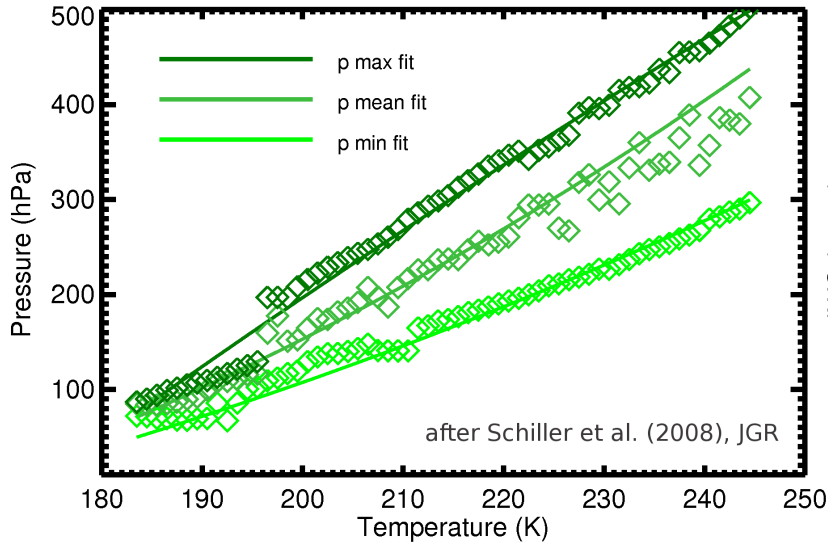
5. [p.28119: The amplitude of the wave can also be given in terms of vertical excursion which is near 100m for the set of chosen parameters.](#)

We added a description of the vertical displacement and the resulting amplitudes in the appendix.

6. [p.28119: The choice of the pressure initial level in the idealized simulation is 100 hPa which is not the level above which most cirrus are generated in the TTL. Is it because the effect of the wave would be less spectacular at lower levels?](#)

We have chosen the setting of  $p_{init} = 100 \text{ hPa}$  as a realistic pressure condition in the TTL ( $T < 200 \text{ K}$ ), as can be seen in the below plot of maximum, mean and minimum pressure versus temperature,

derived from in-situ aircraft measurements during 49 research flights (see attached figure after Schiller et al., 2008).



7. p.28120 and fig.4: I do not understand why the red curve does not oscillate around the blue curve in the upper panels before the onset of nucleation as it does after the system has relaxed to saturation. The relative humidity of the stationary case rather looks like the envelop of the case with superimposed wave.

The idealized simulations were designed to compare large-scale and small scale humidity changes. Since the high-frequency wave is producing overshoots, we chose the envelope humidity from the large-scale updraft for a better comparison. Thus, before nucleation is triggered, the maxima of the humidity overshoots (red) and the large-scale humidity (blue) have almost the same values.

8. p.28122: The authors might add the recent work of Flury et al. (2012) in support of the link between temperature and cirrus.

We added the reference.

9. p.28127: It would be useful to summarize how many cases have been investigated. By taking into account the level (5 values), the large-scale velocity (7 values), the wave amplitude (5 values) and its frequency (7 values), I find 1225 cases multiplied per 2 if heterogeneous nucleation is accounted. Is that correct and is each case given an equal weight in the mean curves shown on figs. 8, 10 and 11?

This is not totally correct, we have in total 4032 simulations and each case is equal in weight; this number is now reported in the text.

10. p.28127: It is unclear in which way the simplified case considered in section 3 differs from one of the "realistic" cases. If not, I would suggest to move all the discussion leading to (8) and this equation to an earlier position in the text.

The simplified case does in principle not differ from the 'realistic' cases; we have moved some parts to an earlier position and deleted some text.

11. p.28128: Is fig.9 showing something else than the superposition of sinusoidal pdf for waves defined by (8) and a set of amplitudes and frequencies given on p.2826 and p.2827. If not, this figure is useless and it is enough to mention that the largest encountered velocity is 3 m/s. In addition, I do not see the point to focus on the velocity if the next section 5.2 concludes that this quantity is not correlated with the number concentration. See above.

Indeed Fig. 9 shows the superposition of sinusoidal pdf for waves with different amplitudes. We agree that the figure is quite standard; however, we would like to show this figure because in the cloud microphysics community the use of the 1-1 relationship between number concentrations and vertical velocity is common and we want to insist that this is **not** appropriate in this case. Therefore we want to show the figure together with the statements in section 5.2.

12. p.28129, 1.25-26: Repetition of 1.9-10 in the same page.

We reformulated the text to avoid repetitions.

13. p.28131: There are many cases (2400?) which are first grouped into 4 families, with a more or less arbitrary separation of fast versus slow mean ascent and then combined to reconstruct the observed spectrum. The weighting within each family is not indicated but it does not seem to be related to any observation derived distribution of the parameters. Hence, it is unclear whether the combination shown as the black curve in fig.11 is optimal or even unique. The robust result seems, however, that combined heterogeneous end homogeneous nucleation with small mean ascent is required to explain the small and mid number concentrations.

The four families are motivated by the different scenarios given by two parameters, i.e. (1) slow vs. fast large-scale updraughts and (2) homogeneous vs. heterogeneous/homogeneous nucleation. Within each family all simulations are weighted equally. Since we have no constraints from measurements about this we have to make an assumption and equal weighting seems to be a reasonable choice. It might be that the choice of the weighting is not unique; however, there are three major points, which go into the weighting and these will always lead to a similar weighting:

- (a) The peak at  $N_i \sim 0.005 - 0.008 \text{ cm}^{-3}$  can only be reproduced by including a reasonable weight of heterogeneous/homogeneous nucleation scenarios.
- (b) The low number concentrations  $N_i < 0.3 \text{ cm}^{-3}$  must be produced by the combination slow large-scale updraughts plus high-frequency waves.
- (c) The high number concentrations are quite rare, thus the weight should be small.

We agree that superposition of high-frequency waves could also lead to higher number concentrations and these are not caught in our setup. This feature would additionally reduce the weight of fast large-scale motion events. This also seems reasonable, since the analysis of large-scale motions in the TTL determines a usual range of vertical velocities in the range  $w \leq 0.01 \text{ m s}^{-1}$  (see, e.g., Virts et al., 2010). We agree that the robust feature of this analysis is the high frequency of occurrence of the combination of slow large-scale updraughts with high-frequency waves for environment allowing pure homogeneous nucleation as well as heterogeneous and homogeneous nucleation.

14. p.28132: The explanation of the large number concentrations by fast ascent (with either homogeneous nucleation alone or combined with heterogeneous nucleation) is puzzling. This contribution is necessary to fit the observations but the proportion a4 is very small. This is possibly the less robust result of this work as we do not know how the simplifications made in the model are influencing the large concentration tail. If the result was true, the large concentration tail should exhibit considerable temporal and spatial variability in the observations. Is there any hint of that feature in the recent data of the ATTREX campaigns (Jensen et al., 2013)?

As stated above, the large-scale motions in the TTL should be in order of  $w \leq 0.01 \text{ m s}^{-1}$  as derived from measurements (see, e.g., Virts et al., 2010). In conclusion one could assume that the fast motions ( $w > 0.01 \text{ m s}^{-1}$ ) are quite rare (see also discussion to point 14 above). There is just indications from *in situ* measurements that events with high values of  $N_i$  are quite rare and these are sometimes embedded between layers of low number concentrations (see figs. in Jensen et al., 2013). Actually, to our knowledge there is no clear indication on temporal or spatial variability from observations.

15. p.28133-28134: Although there is a relation between wavelength and frequency for gravity waves, there is no spatial structure involved in this study. The discussion should be in terms of frequency and avoid the word "wavelength".

In our discussion about the measurements of gravity waves we have to use wavelength. For our model investigations we have tried to follow the reviewer's suggestion to avoid the term 'wavelength' and to use 'frequency' instead.

## References

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