Responses to the Editor

Title: A simple model to assess the impact of gravity waves on ice crystal populations in the tropical tropopause layer

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Authors: Milena Corcos, Albert Hertzog, Riwal Plougonven, Aurélien Podglajen

We would like first to thank the Editor and Reviewers for taking their time to assess our manuscript. The editor's first questions and comments were very kind and constructive, and it was fruitful to revise our paper in this context. In the following document, we have addressed all the concerns raised by the Reviewers, and have detailed the corresponding changes in the manuscript. We hope that our explanations answer all the questions.

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1 Reviewer #1

Q1 - *How do the fluctuations from the balloon data compare with the earlier literature? Is there something special about the observed waves for ice nucleation and growth, or is any old fluctuation with a standard deviation of 1.5K food enough?*

[5] and [7] highlighted the importance of accurate temperature-variability information to assess the effect of small-scale dynamics on in-situ cirrus clouds. These small scales, which are mostly associated with gravity waves, are particularly ubiquitous in the tropical band, and it would be too simplistic to solely characterize wave-induced temperature fluctuations by the mere standard deviation of their timeseries.

First, the intrinsic-frequency spectral decomposition of those fluctuations is important, since waves with different timescales will interact differently with microphysics. Furthermore, a number of studies have highlighted the intermittent nature of gravity waves: [2] for instance showed that the variance of wave-induced cooling rates is associated with a few wave packets, i.e. the cooling-rate timeseries cannot be considered as a statistically stationary random process. Hence, observed timeseries of temperature fluctuations are key to understand the interactions between waves and microphysics, and [4] for instance discussed related effects, like the "shadowing effect".

We have added in Section 4 of the paper a sensitivity study to assess how changing the wave-induced temperature standard deviation modify our results. In particular, solely enforcing that each model-box temperature timeseries has a 1.5 K standard deviation (whereas in the wave simulation, this was only true in average over all the model boxes), while keeping the intermittent nature of wave fluctuations, suffices to produce different ice number densities and ice populations.

Observations of temperature fluctuations therefore allow a more accurate assessment of gravity-wave effects on microphysics than simple synthetic timeseries based on the single standard deviation.

Q2 - *Could you run your model with fluctuations from some of the earlier literature? That would separate the effect of temperature fluctuations from the effect of the details of the model.*

Our microphysical models uses the nucleation scheme described by [6], and is based on the box model from [3]. The details of the model are well described in this literature. As stressed in Section 2.2.1 of the paper, we have furthermore validated our model by comparing our produced ice densities with those of [9] in the constant ascent scenario (see Figure 1 here), and have found the same order of ice-crystal concentrations. We could have run it in additional scenarios, but this seems outside of the scope of this article.

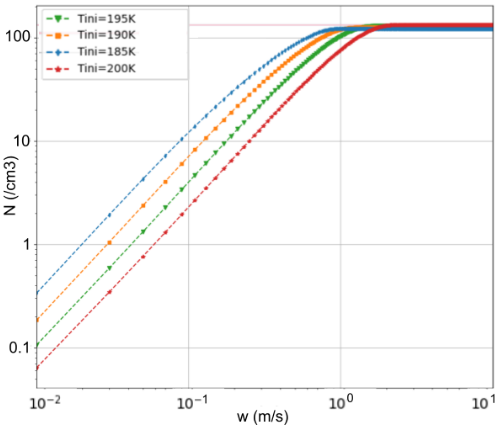

Figure 1: Ice number densities at nucleation as a function of vertical speed

Q3 - *What happens with different amplitudes of waves?*

We have included further sensitivity experiments in the new version of the paper, which are mostly described in the discussion Section. We actually ran simulations where the temperature fluctuations of each air parcel have a fixed standard deviation of 0.5 K, 1.5 K and 3 K. This was enforced by rescaling the raw balloon observations. On the other hand, in the W simulation presented in the original version of the article, only the mean standard deviation (computed over all air parcels) was 1.5 K, but standard deviations were varying by a few percents from one air parcel to the other.

This notably enables us to highlight the strong sensitivity of ice crystal numbers to wave amplitudes, while, on the other hand, the ice crystal size distribution is mostly sensitive to the presence of waves and much less to their amplitudes.

Q4 - *How do your "wave" cases compare to earlier literature with waves?*

Figure 3 of [5] describes differences in ice number concentrations in simulations that include gravity waves, either directly from superpressure-balloon observations or through ERA-interim trajectories (with additional treatments). They compare their ice crystal concentrations to ATTREX observations in their Figure 3. Our Figure 11 is an equivalent of that figure, and shows a better comparison with ATTREX measurements: in particular the observed peak occurrence between $10 - 100$ L^{-1} is well reproduced, while the simulated peak is > 100 L^{-1} in [5]'s simulations. We think that this is one important result of our approach, and we highlight it in the paper.

Another comparison can be performed with the work of [8], who use a model that includes sedimentation as well. [8] report on differences between their "no-wave", "one wave", and idealized wave spectrum simulations in their table 1. They furthermore distinguish two cases: either with or without interactions between vertical levels. Our no-wave results are very similar to their "no-interaction case". Yet, in better agreement with ATTREX observations, we find smaller crystals in our wave simulation ($1.6\,\mu$m) than in their "wave" cases ($8 - 9\,\mu$m). This can be explained by the lower amplitude of temperature oscillations they have used (0.5 K), as well as possibly by the 80-m vertical width of their boxes that allows more growth of ice crystals, more growth of ice crystals being continuously in a given (supersaturated) environment.

Q5 - *Have you run just one model case, or have you run ensembles of your model using different subsets of the observed temperature fluctuations?*

This comment is in line with question Q2 raised by the second Reviewer (see also our related response). We actually ran multiple simulations for the "wave" case, and presented only one in details in this article, as an example of ice production. There were no significant differences in terms of overall ice characteristics between these simulations. This is actually an expected result since, in every simulation, a timeseries of temperature fluctuations is randomly chosen for each model box. Hence, each of our simulations is itself an ensemble of air parcels. It is consequently the overall statistical properties of the applied temperature fluctuations that matters, rather than the details of any given timeseries. This is now stressed in Section 2.2.2 of the paper.

Q6 - *The 15 meter vertical resolution is almost certainly too coarse. You should run with a finer resolution and see if the results change.*

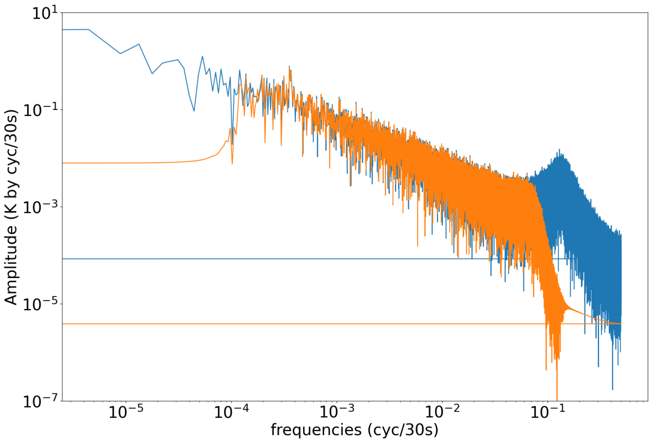

Figure 2: (Blue) Observed and (Orange) filtered temperature power spectra vs intrinsic frequency.

We are limited numerically regarding the geometry of the model, especially if we want to run simulations with a wide enough vertical range. We have nevertheless compared simulations with a 2 m and a 15 m vertical resolutions with fewer air parcels, and it gave similar results. So we have decided to keep the 15 m resolution for this article, which allows us to represent a larger domain with more air parcels.

Q7 - *I'm not sure that it is appropriate to filter the data to remove temperature fluctuations faster than 15 minutes. From Figure 5 an aerosol that freezes can significantly change its diameter in 15 minutes. Once the ice crystals are 5 or 10 microns fast fluctuations don't matter much, but for the crucial growth from a freezing particle up to 1 or 2 microns fast fluctuations may matter. You should run a case with faster fluctuations. From the manuscript line 84 the data may be valid to about 5 minutes – why not run a 5 minute filter and compare that to a 15 minute filter.*

The balloon's own motions constrains the range of frequencies that we can use. Superpressure balloons actually oscillate around their buoyancy equilibrium level with a period close to $2\pi/N$, with N the Brunt-Väisälä frequency (typically, $2\pi/5\,\mathrm{min}$ in the lower stratosphere). Periods below 15 min are contaminated with the balloon motions, so that we are unable to extract the wave driven temperature fluctuations from the observations at those periods. We therefore apply a low-pass filter to the observed timeseries to damp motions with frequencies higher than $N/3$. The result of this filter can be seen in figure 2. Note that we do not completely remove the fluctuations at those periods. This is nevertheless an obvious limitation of our observations. This is now recalled in Section 2.1 of the paper.

Specific comments :

line 32 - *This isn't stated quite right. It is the competition between homogeneous and heterogeneous freezing that is highly sensitive to cooling rate.*

In our understanding, both the competition between homogeneous and heterogeneous nucleation, and the crystal properties in pure homogeneous nucleation are sensitive to cooling rates (see e.g. the difference in our simulations between the NW and W simulations).

line 109 - *The manuscript cites Jensen and Toon (1994) for the assumption of using a monomodal aerosol, but that paper was for constant cooling rates. A distribution of sizes can become more important when there are temperature fluctuations.*

Regarding the aerosol distribution, we have indeed followed the method used by [5] who used a monomodal aerosol distribution to look at the effect of gravity waves. Our simulations used a monodisperse aerosol distribution with $r_a = 250$ nm, consistent with observations performed during the balloon flights. We focus here on the impact on gravity waves, leaving sensitivity to aerosol distribution out of the scope of this article. We nevertheless briefly discuss in Section 4.1 the results of [1] who emphasized the dependency of nucleation rates to the size distribution of aerosols. We have modified the text in line 109 to mention this discussion.

line 362 - *The comment about reasons for dehydration above the tropopause is interesting.*

Thank you!

Figure 2 -*The cooling rate is only meaningful if the averaging time is specified. Given the power spectrum of atmospheric temperature fluctuations, shorter intervals have higher cooling rates. This figure cannot be interpreted without more information.*

The Lagrangian cooling rates are not averaged in our study. They are nevertheless obtained from the finite differences between successive temperature fluctuations:

$$\frac{dT'}{dt} = \frac{T'(t + \Delta t) - T'(t)}{\Delta t} \tag{1}$$

with $\Delta t = 30$ s is the sampling period of temperature observations onboard the balloons. The temperature observations (T) are those obtained with Eq. (3) in the paper. This is now recalled in the paper in Section 3.2.2.

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2 Reviewer #2

Q1 - *Although I like your strategy to present the model simulations, i.e. contrasting the case of no gravity wave with the case including a gravity wave, I think that the no-wave description could be significantly shortened since this case is well-known. In addition, you describe the "preliminary experiment" in section 3.2.1 but I see no reason why this is needed. What do we learn from that experiment? I suggest to remove that preliminary experiment or, if you prefer to keep it, move it to an appendix.*

Regarding the NW experiment, we followed your suggestion and shortened it. For the preliminary WFT experiment, we describe it in order to justify the initial conditions for relative humidity in the W experiment that is different to that used in the NW simulation. It allows us to run a simulation closer to a longer term equilibrium state, easily reached thanks to the gravity waves.

Q2 - *In the discussion section you also address the dehydration of air parcels. I was pretty unsure if this discussion (and the figure 13) is only based on the single W experiment that you describe in prior sections or if it is based on a whole ensemble of simulations. In any case, I think that you should carry out more than one W experiment, i.e. a whole ensemble of simulations, and base the discussion on the results of the ensemble. To construct the ensemble you could play around with some initial conditions and (maybe more importantly) with the balloon-based timeseries of temperature perturbations. Using the ensemble, you could produce a "mean spatial ice crystal pattern" and illustrate that as in figures 8 and 9, a "mean ice crystal size distribution", a "mean ice supersaturation", and also a "mean dehydration".*

This comment is in line with Question Q5 of the first Reviewer (see also our related response). The key point is that each air parcel in our model experiences its own temperature timeseries, randomly chosen in the balloon observations. While this setup has obvious drawbacks (it for instance does not represent the correlation between temperature fluctuations for nearby air parcels), it has the advantage of maximizing the variability of the temperature-fluctuation histories in a single simulation. One simulation can thus be considered as an ensemble of 420 different air parcels. We have actually checked (in a limited number of experiments) that our results consequently do not depend on the chosen timeseries for each air parcel. This is now recalled in Section 2.2.2 of the paper. We nevertheless note in the Discussion Section that the WS0.5 experiment is somewhat more sensitive to the choice of individual air-parcel temperature timeseries, as there is relatively few nucleation events in this simulation.

Q3 - *Description of the model physics in section 2.2.1: In each timestep, you need to (i) compute the sedimentation and (ii) apply the wind-shear module. Which order of these two steps do you use? Is there a dependency on the order of these two operations?*

At each timestep, we calculate the fraction of sedimenting ice crystals in each air parcel, and redistribute the crystals column-wise accordingly. Once in a while, we shuffle this column-wise redistribution because of the wind shear. The wind-shear (or more exactly the mixing module) is thus applied infrequently, i.e. every 500 s. This is now recalled in Section 2.2.1 of the paper.

Specific comments:

Lines 128-131 *What do you mean by "has moved to that immediately above or below"? In my understanding, the horizontal wind causes the given row to move horizontally (as depicted in figure 1), but how do you then rearrange the rows to have again all parcels (vertically) aligned (i.e. again arrive at the configuration of* t_0 *depicted in figure 1)?*

This sentence was first confusing, and we have rephrased it to: "Namely, during a time δt, a given row of air parcels (at a given altitude) has been horizontally advected by $sh\,\delta t$ with respect to that immediately above or below." When this horizontal displacement exceeds $L/2$, we apply the mixing module, which actually comes down to getting back to the initial geometry (vertically aligned columns), except that we have shuffled air parcels in every row. This is now clearly stated in the paper.

Lines 143-145 *Given that there is no correlation between the timeseries of two neighboring parcels, do you observe any artifacts? Is there any information about the (de)correlation length of these perturbations?*

We have indeed run an opposite experiment (called STS for Single Time Series), where wave-induced temperature fluctuations are perfectly correlated in the vertical. The table below sums up the differences between this simulation and the W simulation presented in the paper.

	Min size (μm)	Max size (μm)	Mean size (μm)	Mean concentration (/L)	Mean mass (μg)	Cloud fraction (%)
STS	0.25	7	1.2	1521	0.1	30
W	0.3	13	1.6	1385	0.5	98

Table 1: Ice production comparison between the W and STS simulations.

Note that only the cloud fraction is significantly altered.
The temperature variability in both simulations is yet not realistic. A better representation of wave variability would be in-between the two experiments. Which vertical de-correlation to apply is however hard to assess: gravity waves that we observe in the balloon dataset indeed do not show dominant modes with specific wavelengths, but rather a wide spectrum with different scales. We have therefore decided to detail in this article the no-correlation W case, so as to describe the effect of maximised wave variability and obtain statistically robust results.

Figures 2,3,4,8,9 *I would appreciate an indication of the cold-point tropopause in the figures that show a height-coordinate*

This was corrected in the figures.

Lines 186-189 *Is this true? If the solution particles do not sediment out of the layer (as in your model) then homogeneous nucleation should always be possible given that the relative humidity is high enough.*

In the NW experiment, the initial relative humidity is set just below the nucleation threshold. As the gradient of temperature changes with altitude, the initial production of ice (during the first nucleation event) will only depend on altitude. The larger cooling rates encountered in the lower levels (further away from the cold point) will produce more ice crystals, and the levels above the cold point will not nucleate at all since the temperature increases and therefore lowers the initial relative humidity. Furthermore, for air parcels that nucleate some crystals, these will fall out and lower the relative humidity below the nucleation threshold. Subsequent nucleation events are thus less probable as the air parcels are closer to the cold point: the weaker cooling rates near the cold point will not be high enough to trigger nucleation again. Therefore, the ice production in the following nucleation events depend on altitude as well.

Line 206 *Where can you see in figure 5 that the sedimentation timescale is 1/10hr? The diagram only extends to a radius of about 12 microns where the timescale is about 1/100hr?*

The scale of the radius is log-scale. The corresponding half life time of 12 microns is approximately 1/10 hr.

Line 207 *I don't understand that sentence. After the nucleation the ice crystals virtually do not sediment. What exactly means "50h=450m"?*

Thank you for the correction, this was a typo mistake. *h* is the vertical dimension of an air parcel. We corrected the article.

Figures 4, 9 *I would prefer having the number concentration indicated by colors since the mass is already related to the radius.*

Indeed, we still prefer to keep the graphs representing the mass as they highlight better the growth of ice crystals in time and altitude.

Figure 5 *In the figure you write "half life" and in the caption it is "half time".*

It was corrected in the article.

Lines 276-281 *Note that the background aerosol is kept constant with height, hence nucleation is exclusively controlled by the competition of existing ice crystals for water vapor and the cooling from the gravity wave (+ascend).*

This was added to the article (line 180 and 285) as it is indeed important to understand our results. We kept a constant concentration of aerosols as we are not looking at their impact in this paper.

Technical corrections:

Line 23	The bracket of the reference only encloses the year (further occurrences) .	corrected
Line 60	It should read "homogeneous".	corrected
Lines 63-66	I would prefer having the numbers of the section instead of, e.g., "the following section".	corrected
Table 1	The row "Duration of simulation" has a missing entry in the column WFT.	corrected
Line 289	It should read "zero" instead of "null".	corrected
Line 406	I think "cloud condensation nuclei" should read "ice nuclei".	corrected

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