

Response to Reviewer 1

I am grateful for the reviewer's thoughtful assessment of my study and for providing me with constructive comments. [Replies added in blue](#). Line numbers refer to the preprint.

Major

Introduction and connection to previous approaches

While the statistical representation of mesoscale temperature fluctuations in coarse resolution climate models has a long history (e.g. Kärcher and Burkhardt, 2008; Wang and Penner 2010; Podglajen et al. 2016), it would be very helpful to explain more explicitly the limitations of previous approaches and how the present work addresses these. It is solely mentioned in the introduction that the paper is extending the study of Jensen et al. 2026, but it is not clear how it relates to other previous works on the field. I suggest that the author explicitly positions the present approach within the broader context.

[I agree that previous attempts to parameterize wave effects in the cloud schemes of global models should be more explicitly mentioned in the paper. After line 26 in the introduction, the following paragraph is added:](#)

[“Several global models parameterize the effect of orographic gravity wave forcing on cirrus formation](#)

(Dean et al., 2007, Joos et al., 2008, Lyu et al., 2025). By contrast, little progress has been made to account for cloud-scale updraft speeds and associated temperature fluctuations due to non-orographic GWs. One climate model (Wang & Penner, 2010) incorporates a link between such fluctuations and cirrus formation, but the prescribed fluctuations are not aware of changing atmospheric conditions influencing GW propagation and properties (Kärcher & Podglajen, 2019, section 5.1).”

The limitations of current formulations will become more evident after considering the changes made in response to the next comment.

Stationary assumption and generalization

The author discusses limitations of the present scheme arising from the stationarity assumption for the distribution, e.g., trapped waves or intermittency close to convection. Are these limitations unavoidable? I think it is natural to ask whether the current approach can be related to GW parameterizations already used in models. For example, the variance of the vertical velocity fluctuations used in the WCP should depend on the GW source in the model, and in this way could introduce additional physical constraint on the WCP. Transient subgrid-scale GW parameterizations should be here the natural choice to couple to WCP, since it was demonstrated for some of them that they can produce high intermittency due to convection.

The limitations mentioned above are not unavoidable,

but require approaches that are more specifically tied to the underlying GW physics, concerning e.g., intermittency and spectral slope / frequency composition changes (see lines 359-365). The schemes investigated here can be employed to drive cirrus (ice nucleation) parameterizations in such conditions, but only treat background conditions (section 2.5) away from GW sources, which are not source-specific.

I think closely connected to this issue is the question of how the present approach can be generalized. It was constructed by using a time series of 240 days of point measurements intended to represent the TTL region. Is this approach applicable to other regions where we do not have or only sparse measurements. As pointed out by the author high-resolution global simulations might be helpful here, but even at kilometer scale resolution simulated convectively generated GW fluxes are not fully converged. And even if they were, the fluxes are likely to change in a globally warming world. So, I think an approach relating MTF to a GW parameterization might be promising here.

The schemes presented in section 2.3 can be used in background conditions outside the TTL. However, due to the lack of SPB data at midlatitudes, experimental verification is not (easily) possible and the approach must be used with caution. As long as WCPs are non-interactive and do not consistently cover all distances to the source regions, they should be considered as intermediate solutions to improve the model representation of cirrus and not be seen as robust

solutions in global simulations.

To include this aspect in the manuscript, the paragraph line 384-387 is modified as follows:

“Current wave-cirrus parameterizations are intermediate solutions aiming at improving the model representation of cirrus in process-based and global models. They do not account for specific wave sources, because they describe universal background conditions away from wave source regions. In particular, they do not cover intrinsic frequency composition changes depending on the distance to wave sources and effects of wave trapping. Ideally, ice nucleation schemes in global models rely on wave-cirrus parameterizations that are fully interactive with the model’s resolved atmospheric state and faithfully reproduce associated spatially and temporally variable properties of gravity waves from all sources.”

Section on summary and conclusions

I think this paper makes an important first step towards improving parameterizations of wave-cirrus interactions, but some brief discussion of future directions would be very valuable here. In particular, which next steps remain to bridge the gap between a Lagrangian parcel model and a parameterization applicable in coarse resolution climate models.

The Lagrangian parcel model framework links GW-

induced vertical wind speed fluctuations to ice microphysics on the process level. It can thus be used e.g., to validate parameterizations of ice nucleation in cirrus already used in global models that are driven by such fluctuations. To address the reviewer's remark, a sentence is added before line 384:

“Lagrangian parcel models may be used to validate parameterizations of ice nucleation in cirrus that are driven by parameterized cloud-scale updraft fluctuations (Lyu et al., 2025).”

It is a question outside the scope of this study whether the community should invest more in the improvement of coarse resolution global models or start tackling this issue with the next generation high resolution global models. Regarding the latter, lines 380-384 suggest that analyzing how well vertical wind speed fluctuations due to GWs are represented in the first place would be a first step. Any concrete further steps to improve the situation are likely model-dependent.

Minor

I think in the title should be stressed that this study is only about TTL cirrus formation, since mid-latitude cirrus or cirrus due to sub-grid orography are missing.

While the WCPs can be applied in conditions outside the TTL, the test cases focus on TTL cirrus formation. I agree that the present title could be more specific in this regard and therefore suggest a modified title:

“Simulating TTL cirrus formation: Modeling frameworks for gravity wave representation”

I.24: provides --> provide

done

I.30 awkward wording. Why is the limited knowledge responsible for high-frequency of occurrence and dehydration?

The last part of the sentence in line 33+34, “ due mainly to ... formation stage” is deleted.

I 38-41: This is probably valid only for the TTL. Since this is a main assumption it should be also reflected in the title of the paper.

High frequency waves are also present in the extratropics and not tied to convection. The schemes discussed in section 3 are not wave source-specific. I agree that the title could be more specific, though - see reply to comment above.

I 48-49: probably should be something like: The study provides new knowledge that could help improve our understanding of how air becomes dehydrated before entering the stratosphere and how this process may change in a changing climate?

The sentence is replaced by: “In this way, this study

provides new knowledge that could help improve model predictions of how air becomes dehydrated before entering the stratosphere.”

I 106-108: how to account for an initial offset in the triangular model, why this was not done.

This section was revised upon request of reviewer #2.

I 127: What is "ice frequency statistics" here?

Changed to “probability distributions of nucleated ice crystal number concentrations” to avoid ambiguity. Note that part of this sentence has been moved to right before section 2.1.4 due to minor edits introduced to accommodate the changes suggested by reviewer #2.

Sec 2.2 Where is DICE described? In App. A there is citation of Kärcher and Koop, 2005, but this is another model. Is DICE spectral bin model? This is important to compare with Jensen et al. 2026.

Yes, DICE is a spectra bin model regarding ice, but treats growth of aqueous particles in a Lagrangian manner. I agree to add information about how particle growth is treated, adding in line 398:

“While solution droplets grow to their exact sizes, ice growth is simulated across a fixed size structure with geometrically progressive bins (fixed bin volume ratio) and variable bin centers.”

As parcel models are pretty basic tools in aerosol and cloud physics, the elements of which are provided in earlier publications, I refrain from presenting a more detailed description, with the exception of section 2.6 that provides specific details about how the GW forcing is implemented, and of course App. A that presents a summary of salient model features. The reference in line 395 refers to the treatment of two liquid aerosol types and the one in line 400 in the same paragraph to the treatment of variable deposition coefficients, both of which are non-standard features. The same is true for and reference in line 418 and the water activity in Eq. (A1), which is directly used into calculate the homogeneous freezing (most other models use thermodynamic equilibrium assumptions).

Sec 2.5 A naive question: the stochastic process for w is used as forcing to produce temperature fluctuations. In general w and T are related by the polarization relations of GWs. So why not model T directly by a stochastic process? It is stated that the resulting statistics are Gaussian (in accordance with observations), despite the Laplacian statistics of w . Would a Gaussian process for T work?

A Gaussian process for T would work in situations where observations confirm normally distributed MTF. Non-Gaussian distributions are mainly caused by mountain waves or a found closer to deep convective cells, as briefly addressed in section 2.5.

While one could model temperature fluctuations directly,

we work with vertical wind speed fluctuations for two main reasons:

- (1) homogeneously nucleated ice number concentrations scale with the cooling rate, i.e., w ;
- (2) w is the more fundamental variable from which temperature fluctuations can be derived.

Because temperature is the variable driven by up- and downdraft fluctuations, it needs to be damped to avoid unrealistically large power at low frequencies, see Eq. (7). The resulting relationship between w and T fluctuation amplitudes derived by Kärcher & Podglajen (2019) (their equation 11) holds for GW forcing in “background” conditions. This may change in situations leading to non-Gaussian MTF due to nonlinear interactions between momentum and energy.

Fig. 6: What would the parameterization of Kärcher & Lohmann 2002 produce when forced by w , since this parameterization is widely used in models?

When driven by Laplacian updraft fluctuations, the KL02 scheme would basically replicate the functional form of the ice frequency distribution (as shown in Figs.5+6), see Kärcher & Ström (2003), as it is based on the underlying physical relationship that related n to the updraft speed.

However, the analytical KL02 parameterization overestimates the low ice concentrations, as it does not account for the quenching effect (see App. B), hence, the distribution slope towards the low concentrations

would turn out to be flatter. You'll find a detailed discussion of this effect in Jensen et al. (2026).

line 331: is sedimentation off in the model?

Yes. Line 331 is reworded to read: "..., the statistics simulated with a parcel model may not reflect ..."

line 336-337: awkward wording

The part of the sentence in line 337f, "where the impact of gravity waves on homogeneous freezing is largest." is deleted.

376-378: The high-frequency turbulence fluctuations are filtered out in the present study, but turbulence and GW activity often co-occur, e.g. shear of the GW will enhance existing turbulence. So turbulence might in addition modify the nucleated n .

I agree and believe this is what is stated in lines 374-378.

Data availability on page 20 is incomplete. See also the comment about DICE above.

The data generated to discuss the results in this study will be made available on Zenodo prior to final publication in ACP. The DICE source code is not publicly available. I have introduced a combined code and data availability statement.

Additional references

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