# Review of paper "The representation of gravity wave activity in models of cirrus ice formation" by B. Kärcher

This manuscript addresses an important problem on how to represent the effects of unresolved gravity-wave (GW) variability on cirrus-cloud formation in process-based and global models. The proposed approach is novel in that it uses SPB observations to formulate stochastic processes representing mesoscale temperature fluctuations, which are then used to force a Lagrangian parcel model.

Overall, I find the manuscript clearly written, and the results are generally well presented and explained. I believe the study has the potential to improve the representation of wave–cirrus interactions in models. However, several aspects of the manuscript would benefit from further clarification.

**Major issues**:

- **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.
- **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.
  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.
- **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.

**Minor issues**:

- 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.
- l.24: provides --> provide
- l.30 awkward wording. Why is the limited knowledge responsible for high-frequency of occurrence and dehydration?
- l 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.
- l 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?

- l 106-108: how to account for an initial offset in the triangular model, why this was not done.
- l 127: What is "ice frequency statistics" here?
- 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.
- 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?
- Fig. 6: What would the parameterization of Kärcher & Lohmann 2002 produce when forced by $w$, since this parameterization is widely used in models?
- line 331: is sedimentation off in the model?
- line 336-337: awkward wording
- 376-378: The high-frequency turbulence fluctuations are filtered out in the present study, but turbulence and GW activity often co-inside, e.g. shear of the GW will enhance existing turbulence. So turbulence might in addition modify the nucleated n.
- Data availability on page 20 is incomplete. See also the comment about DICE above.