Review of "A new parameterisation for homogeneous ice nucleation driven by highly variable dynamical forcings?" by Kosareva et al. (gmd-2024-193)

This study further develops a parameterization for estimating homogeneous ice nucleation for cirrus clouds forced by gravity waves. The work builds on a previous study by Dolaptchiev et al. (2023), which laid out the framework for predicting the number concentration of freshly nucleated ice crystals. Here, this parameterization is amended by a relationships that considers the initial ice mass, which is shown to depend on the initial ice concentration and the dynamical forcing. While the science presented here seems reasonable, its presentation requires significant revision. While most of my comments require rather minor requests for clarification, I cannot support the publication at the current stage.

Major Comment

Does the proposed parameterization of the initial ice mass (10) consider all relevant parameters? How do the authors know that the initial ice concentration and the dynamic forcing are the most relevant parameters? Considering that (10) causes a mean error of 30 % (ll. 191 – 192), there must be some parameters that influence the initial ice mass that are not considered in (10). From a simple adiabatic parcel perspective, I would assume that the linear size of the ice crystals should have an impact. I understand that the authors aimed to address this by considering m_\ast (ll. 126 – 128, 193 – 198) – but with little success. Maybe, there is a better estimate for the size of the initial ice size? More comments on this issue are necessary.

Minor Comments

Ll. 1 – 2: For what models is the parameterization developed? Two-moment cloud microphysics in numerical weather or climate prediction models?

L. 3: What does the "correction" correct?

L. 4: The "double-moment ice microphysics" are used in the ensemble simulations or with the newly developed parameterization?

L. 10: Are the "system results" the ensemble results mentioned before?

Ll. 11 – 13: The extension to other dynamical forcings than gravity waves is not discussed in this study.

Ll. 25 – 31: Add a reference to Kärcher et al. (2024) on the impact of turbulence on homogeneous ice nucleation.

L. 38: Be specific about what the "deposition coefficient" refers to.

Ll. 39 – 40: To what "mass" does the "mean mass variability" refer to?

Ll. 45 – 46: To what does the "wide range of initial conditions" refer to? What is varied?

Ll. 58 – 59: While I agree that assuming a spherical shape simplifies the description of ice crystals, this comes with a lot of caveats. The authors should comment on this.

Ll. 56 – 58: Here, "n_i" and "q_i" are used to refer to the ice crystal concentration and the ice mixing ratio, respectively. In Eqns. (1) to (3), and other places, "n" and "q" are used. Are there differences between these variables? If not, why do the authors use a different notation?

Eqns. (1) and (2): The term containing "J exp(B(S-S_c))" should not be identical in these equations. I believe it should be multiplied with "n" in Eqn. (2).

Eqn. (2): What is "p"?

L. 67: Based on Tab. A1, "S_c" seems to vary. Is this variability considered in the ensemble simulation? Or is a constant value assumed? What causes the variability?

Ll. 70 – 71: Eqn. (1) does not consider ice crystal growth due to deposition.

Ll. 74 – 76: I would not consider a parcel simulation as “realistic”. Maybe, one could qualify them as “detailed”.

Eqn. (5): Why is the ratio “p/p_{si}” opposite to the one used in Eqn. (2)?

L. 92: Multiple terms on the right-hand-side of the equation need to be defined.

L. 94: To what does the “prototype parameterisation” refer to?

Eqn. (7) ff.: Does this capital “N” refer to ice crystal concentrations such as the minor “n” used before? Why is a different notation introduced?

Eqn. (7): While I understand that this equation comes from Dolaptchiev et al. (2023), a few words on what is represents and how it is derived are necessary.

Ll. 101 – 109: While the first sentence indicates that this discussion refers to Fig. 1, I got lost in the subsequent sentences to what these statements refer to. Please clarify.

Fig. 1: What does the blue line indicate? What do the dash-dotted lines show?

Fig. 2: I believe the dashed gray line has the same meaning as the blue line in Fig. 1. Why are different colors used?

Ll. 126 – 128, 193 – 198: For what is “m^\ast” relevant? Later, it is stated that this quantity might enable a better parameterization. However, this quantity is not used any further as the improvement is minor. Why is this parameter introduced in the first place? As outlined in my major comment above, a few more details on the parameters chosen for Eqn. (10) are necessary, and they probably also justify the usage of “m^\ast”.

Ll. 129 – 130: The authors should state earlier in the manuscript that solving the full system (1) to (3) is not the standard approach. The reduced system (4) to (6) is the commonly solved version.

Ll. 140 – 145: Define “ICON”, “WKB”, and “MS-GWab”.

Fig. 4: Comment on the increase of m_0 with n_{init}, and the decrease of m_0 with F(t_0). What are the physics behind this behavior?

L. 207: Is “initial n” identical to “n_{init}”? The corresponding statement should also be true for low F(t_0). Should this be added?

Ll. 229 – 231: Why does the prediction of q_{i,post} fail so much? I assume that q_{i,post} = N_{post} * m_0. However, N_{post} and m_0 are predicted quite well. Thus, I guess there is a inherent co-variability between N_{post} and m_0 that is not captured in the parameterization.

Ll. 245 – 246: State the equations to which the full system refers to. What is the “coupled system”? Is it identical to the reduced system (4) to (6)? Why does the “coupled system” not include Eqn. (4)?

Ll. 256 – 257: Do the “deviations in the forcing F(t_0)” refer to the change in F(t_0) with time? Or is there an error in F(t_0)? Since F(t_0) seems to be an external input, I cannot imagine the large errors that are presented here. Please clarify.

Ll. 280 – 281: Why is Eqn. (4) not considered?

Ll. 290 – 292: All distributions for q_{i,post} seem to deviate quite substantially from the reference. Even for the shortest timestep.

Technical Comments

Ll. 16 ff.: The way other works are referenced is not in accordance with the Copernicus style guide.

L. 33: The “is” is not needed.

L. 39: Change “have” to “has”.

L. 68: “Extner” to “Exner”.

L. 91: Do not start this sentence with “Where”.

L. 136: Change “gravity waves” to “GW”.

L. 202: Change “bystability” to “by stability”.

References

Dolaptchiev, S. I., Spichtinger, P., Baumgartner, M., & Achatz, U. (2023). Interactions between gravity waves and cirrus clouds: Asymptotic modeling of wave-induced ice nucleation. *Journal of the Atmospheric Sciences*, *80*(12), 2861-2879.

Kärcher, B., Hoffmann, F., Podglajen, A., Hertzog, A., Pluogonven, R., Atlas, R., ... & Gasparini, B. (2024). Effects of Turbulence on Upper-Tropospheric Ice Supersaturation. *Journal of the Atmospheric Sciences*, *81*(9), 1589-1604.