Review of
Impact of gravity waves on the motion and distribution of atmospheric ice particles
by Aurélian Podglajen et al.

General comment:
In this study a new mechanism for influencing ice crystal distributions in the tropical tropopause layer (TTL) is proposed, a so-called "wave-driven localization" of ice crystals. The mechanism is investigated with an idealized (toy) model, reproducing the main features, with numerical simulations and comparisons with observations from a recent campaign (ATTREX).

Overall, this is a very interesting study including dynamical effects from waves on different scales on ice microphysics and ice clouds in the TTL; thus this is an adequate and meaningful contribution to ACP. However, there are some issues which should be clarified before the manuscript can be accepted for publication. Therefore I recommend major revisions for the manuscript.

In the following I will explain my concerns in detail.

Major points

1. Description and analysis of the simplified ODE system:
   Generally, it is a very meaningful approach to formulate a simple model for representing the important processes and to use this model for a rigorous analysis; this is also a very interesting and important result of this study.

   However, this part of the manuscript should be revised and partly rewritten, since it is very difficult to follow the line of arguments. This is mostly due to the very irritating notation, which is changed in the section several times. For instance, new coefficients as α_G are introduced but only partly used. Sometimes the text refers to "the first" or "the second" equation, but it is not really clear, which equations are meant. In fact, the restriction for the relative humidity to be equal to 100% does not follow from the "second" equation (19) but from the requirement for the equilibrium point, that the derivatives must be zero and thus the radius can only be constant, if the cloud is in thermodynamic equilibrium.

   Beside the confusing (but nevertheless correct) description of the model system and the linearization, there is a major problem for the correct analysis of the nonlinear system. The qualitative behaviour of the equilibrium point in the linearisation can only be transferred to the original nonlinear system, if the eigenvalues have non-zero real part (hyperbolic points). Thus, for the saddle point the argumentation is correct. For points with eigenvalues of zero real part (non-hyperbolic points), the quality of a centre point (in the linearization) cannot easily be transferred to the non-linear system (see, e.g., Verhulst, 1996 or Hirsch et al., 2013).

   I would suggest (also in terms of simplification of the notation) to rewrite the system using new variables $x = \Psi,\ y = r^2$ and constants a, b, c, d:

   $$\dot{x} = -c - dy \qquad (1)$$

   $$\dot{y} = -a \sin x + b \qquad (2)$$

   This abstract formulation helps to see the formal structure of the equations. In fact, it can be seen easily that the system is Hamiltonian with a Hamilton function as follows (transformation $q = x,\ p = y$):

   $$H(p, q) = -cp - \frac{d}{2}p^2 - bq - a\cos q \qquad (3)$$

Using the Hamilton function, the stability of the elliptic point as well as the existence of the periodic solutions can be determined easily. In addition, the Hamilton function might be used for the calculation of trajectories, since solutions are given by $H(p, q) = \text{const.}$, and maybe also for determining the domain of attraction around the elliptic point. This might be interesting in the sense, how many ice particles are really influenced by the mechanism, or better, how close the particles must be to the elliptic point to be affected.

Finally, the representation of the solutions and their stability points in figure 2 is quite difficult to understand; the behaviour of the trajectories is not completely clear. In fact, it seems that some trajectories disappear at $r = 0$, which is probably meaningful (evaporation). However, it is not clear what happens in several parts of the phase space, e.g. in the case $RHi_c = 0.85$ in the range $\frac{\pi}{2} \leq \Psi \leq \pi$ or in the range $\frac{5}{2}\pi \leq \Psi \leq 3\pi$. Please reproduce the figure with some zooms around the equilibrium points and less trajectories for explaining the schematic behaviour of the solutions (phase portrait). Maybe this representation can be connected with the important and illuminative physical interpretation in section 2.2.4.

2. Neglecting water vapour depletion by ice crystals:

   For the formulation of the model equations (11) and also the simplified model (eq. 19), the depletion of water vapour by crystal growth is neglected. I can understand that for the analysis of the model this is a convenient simplification. However, it should be estimated how large the effect on the background fields as well as the solutions really is. This should be done analytically and/or using numerical simulations. Probably, the effect is really small and the assumption is meaningful but this must be shown.

   The whole study treats ice crystals, which are already there, i.e. the formation of ice crystals is not taken into account. However, in principle ice crystals are formed in the low temperature regime of TTL at high supersaturations ($RHi \sim 130 - 170\%$, depending on the formation mechanism). Thus, the assumption of ice crystals in a region at thermodynamic equilibrium seems to be quite strong. For me two different scenarios might be possible, if we start with ice nucleation:

   (a) If only a few ice crystal form, they are not able to deplete enough water vapour for reaching equilibrium and thus the described mechanism does not work, until the ice crystals have grown to larger sizes and have fallen out into a region with relative humidity close to ice saturation. It is not clear if for large ice particles (radius close to 100 µm) the described mechanism will be efficient. Please, comment on this.

   (b) If many ice crystals are formed, they will deplete the water vapour without growing to larger sizes (because they are many) until the system reaches equilibrium. Then the described mechanism can play a role. In this scenario, please describe, how large the effect of wave-driven localization is in comparison to quenching of water vapour.

Minor points:

1. Figure 1: Aspect ratio of the phenomenon
   In the example of figure 1, the vertical extension is of order $O(3\,\text{km})$ whereas the horizontal extension is of order $O(10^3\,\text{km})$; thus the aspect ratio is very small, please indicate this in the text and also in the figure caption.

2. Page 4, lines 7-15 and following next page:
   It seems that the effect of wave-driven localization is mainly effective for waves with quite low frequencies (Kelvin waves). Please comment this in the text.

3. Constraining the value of deposition coefficient:
   Actually, Skrotzki et al. (2013) does give a recommendation for a value of the deposition coefficient, based on a collection laboratory experiments, model simulations and a synthesis of both, i.e. $\overline{\alpha}_d = 0.7$ and $0.2 \leq \alpha_d \leq 1$. Thus, the used value of $\alpha_d \sim 0.5$ is in the recommended range. Please reformulate the text accordingly.

4. Expression for the saturation mixing ratio:
   The correct (but still approximate) formula for the saturation mixing ratio is $q_{\text{sat}} = \epsilon \frac{e_{\text{sat}}(T)}{P}$ with the ratio of molar masses of water and air, respectively, $\epsilon = \frac{M_v}{M_a}$.

5. Figure 4 and text:
   In this figure the time evolution of the particles' position is shown. It would be nice to quantify how many particles from the initial distribution at 0.0 days really survive in a position close to the elliptic point. A similar statistics would be interesting for the simulations in figure 5 and figure B1 in the appendix.

6. Page 15, line 15 and equation (15): Slow down of ice crystal sedimentation
   It is stated here that the sedimentation is reduced significantly by wave advection. Can you quantify this statement, i.e. by which fraction is the sedimentation reduced for distinct conditions?

7. Validity of several approximations
   For the formulation of the model equations some approximations are made without much information about the validity of the approximation, e.g. the assumption of spherical particles (Stokes' flow for sedimentation, eq. 17) or the linearisation of the saturation vapour pressure (eq. 18). Please indicate (at least in the appendix) the validity of these approximations quantitatively. On the other hand, the full growth factor for ice crystals is used, including kinetic and ventilation corrections and latent heat release. Since the model is used in a very small part of the phase space (radius $5\,\mu\text{m} \leq r \leq 100\,\mu\text{m}$, very cold temperatures in the TTL) not all corrections are really meaningful or necessary. Thus, there is a kind of discrepancy between approximations on one hand and very accurate treatment of processes on the other hand. Please resolve this discrepancy in a meaningful way.

References

Verhulst, F., 1996: Nonlinear Differential Equations and Dynamical Systems. Second edition, Springer, Heidelberg.

Hirsch, M., S. Smale, R. Devaney, 2013: Differential Equations, Dynamical Systems, and an Introduction to Chaos. Academic Press (Elsevier), Amsterdam.