Impact of gravity waves on the motion and distribution of atmospheric ice particles: reply to reviewer 2

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We would like to thank the reviewer for his/her very insightful comments and suggestions regarding our manuscript. Please find below our point-by-point reply.

1. ***Reviewer*** — 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 intro- duced 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.

   Authors — We have rewritten this section to clarify the notations and the associated mathematical explanation, following the reviewer's suggestion (see also below).

2. ***Reviewer*** — 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:

$$\dot{x} = -c - dy(1)\dot{y} = -asinx + b(2) \qquad (1)$$

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 $q = x$, $p = y$):

$$H(p, q) = -cp - p^2 - bq - a \cos q \tag{2}$$

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 $H(p, q) = const.$, 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.

Authors — We are very grateful to the reviewer for the suggestions, especially for pointing out the Hamiltonian structure of the system. We agree that the analysis near the elliptic point was not rigorous (although supported by the numerical calculations); this is now corrected with the Hamiltonian formulation.

We have adopted the simplified notations suggested by the reviewer, except for the introduction of the new variables x and y. We believe indeed that the physical meaning of the equations is better presented by expressing them with the physical quantities, and chose to prefer that point over a clearer presentation of the mathematical structure, which we put in a new appendix.

3. ***Reviewer*** — 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.

   Authors — The reviewer raises an important limitation of our study. However, evaluating the impact of vapor depletion requires assumptions on the ice crystal number and/or the ice nucleation process, which would be arbitrary. Testing the sensitivity to different ice crystal number concentrations is beyond the scope of our study and would weaken the focus of the work. We hence prefer to avoid this discussion, and emphasize that the approximation is realistic for very low ice crystal number clouds. Furthermore, this idealized context notably enables us to highlight the role of the wave-localization effect, which on its own is able to maintain clouds at $RH_i \simeq 100\%$.

4. ***Reviewer*** — 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 Î¼m) 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.

Authors — We agree with the reviewer: we make the assumption that there are already some ice crystals in the region of thermodynamic equilibrium to start with. However, as mentioned above, including ice nucleation would require additional assumptions which we would like to avoid. Furthermore, it is not reasonable to include ice nucleation without also including the spectrum of high frequency gravity waves that influence its outcome (Spichtinger and Krämer, 2013). When those higher frequency waves are considered, the two scenarios proposed by the reviewer are not the only possibilities for crystals to get in regions of thermodynamic equilibrium of a lower frequency wave. Indeed, adding smaller scale waves might increase the relative humidity from $\sim 100\%$ to the heterogeneous or homogeneous threshold and thus trigger nucleation in those regions. For consistency, including nucleation would then also require a rigorous treatment of those waves. Although it might be possible to extend our framework to include the noise induced by high-frequency waves through deriving stochastic differential equations, this would require significant additional work and would also complicate the message of the paper. We thus prefer to leave those considerations for future studies and restrict ourselves to a discussion of the realism and possibility of generating ice crystals in the thermodynamic equilibirum region in the first place. This point is now discussed in Sect. 3.

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

   Authors — This is specified in the revised manuscript.

6. ***Reviewer*** — 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.

   Authors — Actually, the only requirement is on the vertical phase speed of the wave. However, the integrated effect on the displacement will be larger for low frequency waves (Kelvin waves or equatorial Inertio-Gravity waves), which is now specified in the text.

7. ***Reviewer*** — 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. $0.2 \leq \alpha_d \leq 1$. Thus, the used value of $\alpha_d = 0.5$ is in the recommended range. Please reformulate the text accordingly.

   Authors — We modified the text accordingly.

8. ***Reviewer*** — Expression for the saturation mixing ratio: The correct (but still approximate) formula for the saturation mixing ratio is $qsat = \epsilon e_{sat}(T)/P$ with ϵ the ratio of molar masses of water and air, respectively.

   Authors — We actually are working with the volume mixing ratio rather than the mass mixing ratio. This is now specified in the text.

9. ***Reviewer*** — 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.

   Authors — We agree with the reviewer, and now mention the statistics in the text.

10. ***Reviewer*** — 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?

    Authors — This has been added.

11. ***Reviewer*** — 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 μm $\leq$ r $\leq$ 100 μm, 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.

    Authors — In Sect. 2.2.1 the full equations are presented and they are the ones used for the numerical analysis in Sect. 3.1. The main microphysical approximation present at that point is that of spherical particles (necessary for tractability). Following the reviewer's comment, we now discuss the validity of this approximation relative to observations in Sect. 2.2.1, and quantify the associated uncertainty in Sect. 4.2.

    If it is true that further approximations (including the linearisation of the saturation vapor pressure) are made in Sect. 2.2.2., they are only introduced in order to derive the simplified system (toy model) for the theoretical analysis. They are only used in the context of this simplified system, and relaxed starting Sect. 3. Besides, for the toy model, we also approximate the growth factor to its expression without kinetic or ventilation corrections. Since approximations are introduced for the different microphysical factors at the same point, in our opinion, there is no real discrepancy between the treatment of the different processes.

References

Spichtinger, P. and Krämer, M.: Tropical tropopause ice clouds: a dynamic approach to the mystery of low crystal numbers, Atmos. Chem. Phys., 13, 9801–9818, doi:10.5194/acp-13-9801-2013, 2013.