Impact of gravity waves on the motion and distribution of atmospheric ice particles: reply to reviewer 1

April 9, 2018

We would like to thank the reviewer for his/her constructive comments on our manuscript, especially for the suggestion of missing relevant references. Please find below our point-by-point reply.

1. ***Reviewer*** — This investigation points to an effect in cirrus clouds that has so far been largely over- looked, called wave-driven localization. It means that by the combined action of waves, crystal sedimentation and crystal growth/sublimation it can happen that crystals collect in a region where the relative humidity wrt ice is about 100quence of this is that the lifetime of those crystals is longer than without the waves since the crystals cannot fall away from that "elliptic fixed point"; this in turn might reduce dehydration and increase the occurrence of thin cirrus in the TTL. This is an interesting paper, with a high quality of its mathematical derivations and numerical applications. It is worth publication in ACP.

   That said, I must admit that I am not convinced of the relevance of the localization effect for the atmosphere. This remains to be demonstrated. There are two major reasons for my scepticism:

   1) There are a number of simplifications, necessarily in the analytical model, and in the numerical model. For many of these there may be good reasons or they are harmless (spherical crystals). But there are two simplifications that may be critical. One is the assumption that crystals are already there at the initialisation of the model. On page 18 the authors state " What happens ... is that only the ice crystals INITIALLY located near $RH_i \simeq 100\%$ remain ...". As ice nucleation usually needs high supersatu- ration, I wonder whether there are ever ice crystals initially at 100%.

   Authors — Although the wave-driven localization at 100% relative humidity depends on ice crystals being already present there, our use of "initially" in the context of the article does not necessarily refer to the nucleation time of ice particles. We agree with the reviewer that supersaturation is needed to nucleate ice crystals in the TTL, but as the sedimentation starts, ice crystals will likely encounter $RH_i \simeq 100\%$. At this time, the crystal size, the wave characteristics, and the background relative humidity will be critical to determine whether these ice crystals will be sensitive to the wave-localization effect. One could imagine several mechanisms, such as small-scale gravity waves locally increasing the

RH_i, to explain the initial formation of ice crystals that subsequently sediment (see also response to reviewer 2). However, including this in our set-up would require a number of additional assumptions that are better left for future investigations. We now emphasize explicitly in the text the "ad hoc" initialisation.

2. ***Reviewer*** — The other critical assumption is that of a negligible feedback of crystal growth/sublimation on RHi. As the authors say, high crystal concentrations are common in TTL cirrus, so that assumption might be unrealistic. To my view, it is an assumption that might be necessary to develop the theory and the arguments, but later it could be relaxed. It should be relatively easy to run the numerical model with water-ice feedback. The question then is whether the localization effect is still present when the feedback is switched on. However, actually there are cirrus clouds in the TTL that have extremely small crystal number concentration. These are the "Ultrathin Tropical Tropopause Clouds (UTTCs)" (Peter et al., 2003). Luo et al.(2003) have proposed a mechanism that leads to a stabilisation of such clouds. I suggest that the authors mention the UTTCs and the corresponding mechanism, although it works without waves. Also Spichtinger and Kraemer (2013) proposed a mechanism that would produce clouds with low crystal numer densities; their mechanisms works with short waves where the wave "down phase" essentially terminates the ongoing nucleation process. I think this work should also be mentioned and the difference between the proposed mechanisms briefly discussed.

   Authors — It is true that our study is more relevant to low ice-crystal number clouds since we have on purpose omitted the feedback of ice crystals on water vapor. This idealized set-up notably enables us to highlight the role of the wave-driven localization effect, which is able to maintain clouds at $RH_i \simeq 100\%$ on its own. When referring to very thin, low ice-crystal number cirrus such as those observed by Jensen et al. (2013, 2017), we were actually already considering UTCCs without using the name. We now explicitly mention the name "UTCCs" and reference Peter et al., 2003 in the revised version paper. We had actually missed the very relevant Luo et al.(2003) reference, which is now discussed (in Sect. 2.2.3). However, the work of Spichtinger and Kraemer (2013) deals with the influence of gravity waves on ice nucleation, a very different problem from that addressed in our work. We now mention their study in the introduction.

3. ***Reviewer*** — 2) On page 20 (last lines) the authors make the point that the localization is an important effect and that its disregard in global models with their coarse vertical resolution and in weather models leads to "significant uncertainties". To my view, this is too cheap a statement. The statement may be ok if it had been written in conditional tense and without the "significant". Otherwise, it must be shown what the bad consequences of its negligence are on dehydration, radiation, water vapor transport into the tropical stratosphere, etc.

   Authors — We changed the statement following the reviewer's suggestion.

4. ***Reviewer*** — 1) Page 2, Line 21/22: As the wave phase is a purely mathematical object, I suppose that it can only affect ice crystals indirectly. An influence can only be exerted by material (physical) properties of the crystals environment, as T or RH. Does your statement imply that such properties are uniquely related to the wave phase?

Authors — Yes, with our assumptions temperature and relative humidity anomalies are uniquely related to the wave phase.

5. ***Reviewer*** — 2) P. 3, L. 2: Is it possible at all that RH=const in a wavy environment? Perhaps in this special case you better speak of "solid particles that fall but that don't grow or sublimate" instead of ice.

   Authors — It might be possible within very thick, high ice crystal number clouds which would damp the relative humidity. But we agree with the reviewer's suggestion that it is better to talk of solid particles and changed the text accordingly.

6. ***Reviewer*** — 1) Page 1, Line 22/23: Isn't the wind identical to the movement of air parcels?

   Authors — Yes, but not to the motion of falling particles.

7. ***Reviewer*** — P. 2, L. 2: 190 K is not a range.

   Authors — Corrected

8. ***Reviewer*** — P. 2, L. 3: insert "of the" before "atmosphere".

   Authors — Corrected

9. ***Reviewer*** — 4) P. 2, L. 15: write "to and fro" instead of "to and from".

   Authors — Corrected

10. ***Reviewer*** — 5) P. 2, L. 20/21: "the falling particles fall in the same direction as the wave phase" implies that the phase falls. Better write "the falling particles fall in the direction of wave propagation".

    Authors — Corrected

11. ***Reviewer*** — 6) P. 2, L. 32: which system?

    Authors — The wave-ice crystal system. This has been precised.

12. ***Reviewer*** — 7) P. 5, L. 3,4: "green" should be "red".

    Authors — Corrected.

13. ***Reviewer*** — 8) P. 5, L. 26: Although the notion "perfect gas" exists (a further simplification of an ideal gas), the gas constant should be termed "gas constant" or "specific gas constant for water vapor". There is nothing in the calculation presented that needs the assumptions of a "perfect gas".

    Authors — Changed

14. ***Reviewer*** — 9) P. 7, L. 5: please write "crystal number concentrations".

    Authors — Changed

15. ***Reviewer*** — P. 7, L. 27: The "next section" is 2.2.3, not 2.2.1.

    Authors — Corrected

16. ***Reviewer*** — 11) Eq. 18: I am puzzled by the terms RH i c (Z). Before RH i c was introduced as a constant. Why is it now a function of Z? Please mention also the meaning of the terms in the brackets (probably Clausius-Clapeyron and pressure change?).

    Authors — We were keeping the Z because that formula is valid for any profile of relative humidity. We now specify the different terms.

17. ***Reviewer*** — 12) P. 8, L. 17: As RH i c was never specified, is the fix point possible for the whole range of possible values? Is it tacitly to be understood that RH i c is close to or above ice saturation since there are ice crystals?

    Authors — The necessary condition for the fixed point is that there exists regions where $RH_i = 100\%$ in the wave field. This has been clarified in the text (Eq. (22)).

18. ***Reviewer*** — P. 9, L. 3: Check sentence!

    Authors — Rephrased

19. ***Reviewer*** — 14) P. 9, L. 13: Which of the amplitudes?

    Authors — Temperature amplitude, this is now specified.

20. ***Reviewer*** — 15) Fig. 2: Is it possible to indicate the direction of the motion in phase space?

    Authors — We have added arrows to indicate the direction.

21. ***Reviewer*** — 16) P. 11, L. 11-12: On first reading, it was not clear to me what exactly is the difference between the "cold" phase in the eastern Pacific and the "cooling" phase in the western Pacific. Only the later reference to figure 2 clarifies that. I suggest to refer earlier to the figure to illustrate the distinction.

    Authors — Done.

22. ***Reviewer*** — 17) Figure 4 does not show green points, contrary to what the caption says.

    Authors — Corrected

23. ***Reviewer*** — 18) P. 14, L. 17: blue is a color as well!

    Authors — Corrected

24. ***Reviewer*** — 19) P. 16, last par: change "equality" to "equation".

    Authors — Changed

25. ***Reviewer*** — 20) Figure 6: blue and black are hard to distinguish.

    Authors — We dashed the blue line to avoid the confusion.

26. ***Reviewer*** — 21) P. 18, L. 15: The sentence is a bit strange. In clear sky there are no cirrus clouds. How can then their dehydration efficiency be constrained?

    Authors — We meant all sky dehydration efficiency, we have rephrased that sentence.

27. ***Reviewer*** — 22) P. 19, L. 6: "order" should be "power".

    Authors — We meant order moment. Corrected.

28. ***Reviewer*** — 23) P. 19, L 30.: "disagreement" between what?

    Authors — Models and observations. Now specified

References

Jensen, E. J., Diskin, G., Lawson, R. P., Lance, S., Bui, T. P., Hlavka, D., McGill, M., Pfister, L., Toon, O. B., and Gao, R.: Ice nucleation and dehydration in the Tropical Tropopause Layer, Proc. Nat. Acad. Sci., 110, 2041–2046, doi:10.1073/pnas.1217104110, 2013.

Jensen, E. J., Pfister, L., Jordan, D. E., Bui, T. V., Ueyama, R., Singh, H. B., Thornberry, T. D., Rollins, A. W., Gao, R.-S., Fahey, D. W., Rosenlof, K. H., Elkins, J. W., Diskin, G. S., DiGangi, J. P., Lawson, R. P., Woods, S., Atlas, E. L., Rodriguez, M. A. N., Wofsy, S. C., Pittman, J., Bardeen, C. G., Toon, O. B., Kindel, B. C., Newman, P. A., McGill, M. J., Hlavka, D. L., Lait, L. R., Schoeberl, M. R., Bergman, J. W., Selkirk, H. B., Alexander, M. J., Kim, J.-E., Lim, B. H., Stutz, J., and Pfeilsticker, K.: The NASA Airborne Tropical Tropopause Experiment: High-Altitude Aircraft Measurements in the Tropical Western Pacific, Bulletin of the American Meteorological Society, 98, 129–143, doi:10.1175/BAMS-D-14-00263.1, URL `http://dx.doi.org/10.1175/BAMS-D-14-00263.1`, 2017.