

Reply on CC3

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Community comment on "Comment on "Review of experimental studies of secondary ice production" by Korolev and Leisner (2020)" by Vaughan T. J. Phillips et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-123-CC4>, 2021

I thank Korolev for this comment.

We disagree that our remarks about sublimation breakup were redundant in the commentary (Phillips et al. 2021, Sec. 3.2).

Originally, the original review (Korolev and Leisner, 2020 ["KL2020"]) concluded that:

"Since small ice fragments have lower terminal fall velocity, their residence time in the undersaturated environment may be long enough to result in their complete evaporation before they can re-enter a supersaturated environment. This appears to be a significant limitation of the SIP mechanism due to sublimation breakup. This mechanism is also unlikely to explain explosive concentrations of small ice crystals frequently observed in convective and stratiform frontal clouds (e.g., Lawson et al., 2017; Korolev et al., 2020) ... Activation of SIP due to the fragmentation of sublimating ice requires spatial proximity of undersaturated and supersaturated cloud regions. In this case, secondary ice particles formed in the undersaturated cloud regions can be rapidly transported into the supersaturated regions prior their sublimation. Such conditions may occur in cloud regions affected by entrainment and mixing with out-of-cloud dry air."

Firstly, the mechanism stated here by KL2020 is about entrainment mixing between environment and cloud edges. There is no mention of convective downdrafts. In fact, KL2020 does not even mention descent anywhere as a cause of the sublimation breakup. By contrast, our focus was chiefly on convective downdrafts (Phillips et al. 2021, Sec. 3.2) and we provided the scale analysis to estimate the ice enhancement in such descent. Of course, we mentioned entrainment mixing as a possible cause of breakup but our emphasis was on deep convective descent.

Secondly, the concept depicted by KL2020 involves fragments being emitted at a particular instant of time and disappearing at some later time by total sublimation with the possibility of transfer into an adjacent supersaturated region. Whether that mixing happens too early or too late determines if the fragments are actually transferred.

Again, our picture is different. In Sec. 3.2 of our commentary (Phillips et al. 2021), we write that there is continual emission of fragments during deep descent in a convective downdraft (e.g. 2 m/s) that is perhaps 1 or 2 km deep. Irrespective of the rate of mixing from the downdraft to any adjacent ascent, the ice concentration reaches a quasi-steady

state due to the balance between generation and depletion of fragments by total sublimation. This is what we mean by quasi-equilibrium.

According to our conceptual picture, there is no need for spatial proximity between subsaturated and supersaturated regions because the same quasi-equilibrium ice concentration will be transferred regardless of the rate of the mixing or timing of recirculation from downdraft to updraft. Similarly enhanced ice concentrations from sublimation breakup will be in the recirculated air whether entering the updraft sooner or later. In the commentary we concluded:

"The pivotal point here is that such a quasi-equilibrium concentration is maintained throughout the entirety of the subsequent [convective] descent after being reached. Thus, any recirculation of downdraft air into the surrounding convective ascent would transfer air enriched in fragments for their subsequent vapour growth and survival."

We hope that in the rest of this debate here the uniqueness of our ideas can be at least acknowledged.

References:

Korolev, A., and Leisner, T.: Review of experimental studies of secondary ice production. *Atmos. Chem. Phys.*, 20, 11767–11797, <https://doi.org/10.5194/acp-20-11767-2020>, 2020.

Phillips, T.J., Yano, J.-I., Deshmukh, A., and Waman, D.: Comment on "Review of Experimental Studies of Secondary Ice Production" by Korolev and Leisner (2020), *Atmos. Chem. Phys. Discussion*, <https://doi.org/10.5194/acp-2021-123>, 2021