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Reply on CC2

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Author 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-AC3, 2021

We thank the authors of the review paper for responding to our commentary.

I am glad to see that we all agree that the two laboratory studies by Vardiman and Takahashi et al. were important for initiating research into this mechanism of breakup in ice-ice collisions.

However, regarding the first point from Thomas Leisner, our commentary was about what the authors actually published in their review paper last year (Korolev and Leisner 2020 [KL2020]) and its intellectual basis. Whether the authors now continue to criticise both laboratory studies by Vardiman and Takahashi et al. is not the salient issue.

Perhaps we can re-visit the text of the corrected review paper by KL2020:

"A detailed analysis of the Takahashi et al. (1995) laboratory setup indicated that the riming of ice spheres occurred in still air, which resulted in more lumpy and fragile rime compared to that formed in free-falling graupel.The collisional kinetic energy and the surface area of collision of the 2 cm diameter ice spheres also significantly exceed the kinetic energy and collision area of graupel whose typical size is a few millimeters. Altogether, it may result in overestimation of the rate of SIP, compared to graupel formed in natural clouds. …No parameterizations of SIP due to ice–ice collisional fragmentation can be developed at that stage based on two laboratory observations [both studies by Takahashi et al. and Vardiman], whose results are conflicting with each other" (sentence B).

This text was clearly a negative criticism of the Takahashi and Vardiman lab studies, because both studies aimed to provide a basis for predicting observed concentrations of ice seen in natural clouds. Indeed, Takahashi et al. (1995) wrote in the abstract: "This ice particle production mechanism ... may help to explain the high concentrations of ice crystals observed m mantlme winter cumuli". They go onto make a quantitative prediction of ice concentrations in-cloud resulting from the ice multiplication based on their observations and claim agreement with aircraft observations. Similarly, Vardiman (1978) used his lab/field data of fragmentation as the basis for predicting in-cloud concentrations of ice too, creating a theory with a 0D analytical model.

The final sentence B of the quote from KL2020 effectively says that no such quantitative predictions can be made from their laboratory observations. These quantitative predictions made in both lab studies constitute "parameterizations" by the experimentalists themselves.

Regarding the third point, that same sentence B is such a strong statement about the lack of usefulness of both lab studies for representations of natural clouds that it was only reasonable to contact the authors of the review for clarification. When we did so, they elaborated their criticisms with more detail, as related in our commentary.

Of course, the personal communication citation can be withdrawn, if the KL2020 authors do not agree to our interpretation. But any commentary such as ours need not be restricted only to material of the KL2020 review.

When such a contentious statement is made, denying the accuracy and usefulness of the only two lab studies for breakup in ice-ice collisions, it is valid to ask what other possible criticisms could be made to justify it, beyond what is merely in the review.