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Comment on acp-2021-123

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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-CC2>, 2021

Here a few important points from the perspective of the authors of the review article:

- We do not criticize, and certainly not „negatively criticize“ the work of Takahashi and Vardiman. These pioneering studies are up to now all laboratory experiments we have to rely on regarding ice-ice fragmentation
- We criticize, or rather feel sorry, that this work was not taken up and extended to a broader range of atmospherically relevant situations. This follows the general tone of our review article which calls for a new surge in laboratory experiments on secondary ice formation.
- We indeed have some objections on how the original experiments were carried out and where they do not reflect what is the predominant situation in clouds where ice-ice collisional fragmentation occurs. This was not the topic of our original article and Phillips et al. cite it here as "personal communication", without giving any detail. They then refute arguments that are (at least partially, especially when rotations an angular momentum is mentioned) misinterpretations of our communication to them. We strongly feel that the discussion here should stay focused on the content of our review article. Depending on the final content of the comment we will prepare a reply to clarify this.
- What we wanted to touch in the review, and perhaps not stated clearly enough is that we feel that the wide distribution of atmospheric ice with respect to shape, crystallinity, degree of riming etc. has to be assessed in much more detail. We do not believe, that one single parameter can account for all these factors, but this is beyond the scope of our review and could be worked out by us further if necessary.