**Comment on "Review of Experimental Studies of Secondary Ice Production" by Korolev and Leisner (2020), Phillips et al., acp-123**

In this reviewers' opinion, the responses to the reviewers' comments are inadequate. How do you move the field forward-not by writing that there's little need to do more laboratory experiments-especially on fragmentation, . The study makes considerable use of the Takahashi (1995), experiments, where 2 cm ice balls were collided to generate fragmentations. How could that be considered the final answer? I have my own experience, working with a key player in recent identification of processes that lead to secondary ice production, and based on that have a theory that has not been examined in-depth.

The responses by Phillips et al. are highlighted below in bold text, my thoughts on the responses are not highlighted.

**Phillips et al. (2017ab) provides details about the role of breakup in clouds. Eventually the vast inaccuracy from omitting breakup in ice-ice collisions in current models will be widely recognised.**

Reasonable response, although I don't think they are correct in their interpretation.

**As we see it there are three points we wish to convey and we have updated the text to modify this. First, theoretical and modelling studies of SIP by breakup in ice-ice collisions are possible even without any further laboratory experiments**

I disagree with this point. It's an overconfident view

**Second, our previous theoretical and modeling studies of this type of breakup are valid EVEN IF all the previous laboratory experiments turn out to be totally erroneous. Lastly, previous laboratory experiments about breakup in ice-ice collisions are, in fact, not as erroneous as KL2020 tries to suggest. Any issues of representativeness or bias (e.g. sublimational weakening with Vardiman) in both lab/field studies are possible to correct for and are not prohibitively serious.**

This is not a good response in my opinion.

**In natural clouds, such hail particles (2 cm) grow by alternating episodes of dry and wet growth, hence the layered structure of hailstone sections. There is the re-freezing of the wet surface just like the wet surface of the frozen drop in the lab experiment.**

You haven't addressed the question in my opinion.

**There is the clear suggestion here from KL2020 that a grave error is introduced by applying the lab**

**results to estimate the breakup of graupel in natural clouds, as Takahashi et al. (1995, their Section 4) were attempting to do. Such an estimate was the stated goal of their paper in 1995.**

I agree with the reviewer and not the author

**As simulated by Phillips et al. (2015), it is perfectly possible for 1-2 cm hail particles to form inside a cloud, and when they do so they will collide. The fact that in the lab one of the particles was fixed is equivalent to changing the CKE by a factor of 2 relative to both being free (as shown elsewhere here; Eq (3)) which is equivalent to a tiny error in the fragmentation rate (as shown in Section 4 of the commentary; Fig. 4). There is no problem provided one stratifies the data in terms of energy.**

The vast majority of SIP production is in clouds where rimed particles and sometimes graupel are involved in the SIP process. The Takahashi 1995 study is not valid for the vast majority of situations.

**Such energy conservation is an absolute constraint that all collisions, whether natural or artificial,**
**must follow.**

This is an inadequate statement that is not proven with laboratory data. This is why more laboratory data are needed.