<u>Replies to Editor's Comments</u>

Summary of Author Response

We are grateful for the positive verdict.

We agree that constructive debate with sharing of ideas is always a good idea for progress in science. One could even argue that certainty in science is never possible and that scientific progress involves simply continual shifts in consensus as new observations, models and ideas arise and as fresh generations of scientists enter the arena. Discussions are the essence of science.

We agree that future lab observations will no doubt change and clarify the ideas from the past two lab studies that we have been debating the merits of, perhaps in ways we cannot yet imagine.

We have tried to re-phrase our ideas better to clarify our position and to provide counter-arguments, as required.

Point by Point Responses

Editor: After reviewing your responses to the reviewers and conferring with referee #2, I am generally inclined to accept your commentary for publication. However, there are still a couple of points that you would like considering rephrasing. This may be in part because different communities, here experimentalists and modelers, have different connotations of the applied phrasing.

Response: We agree, there seem to be slightly different philosophical perspectives at play among the various communities. They all have different fortes and unique contributions to make.

Editor: In the revised version of the manuscript, you make clear that the underlying theory of breakup in ice-ice collisions stands alone. I think there is no doubt about this valid contribution to this research area. However, as it is written, I agree with referee #2, that how you use Vardiman 1978 and Takahashi et al. 1995, is best described as calibration.

Response: In the manuscript, our objection is not so much about the use of the term "calibration", but rather is the suggestion from the entirety of the quoted four sentences that somehow a simple curve-fitting was applied. The notion of the particles being unnaturally large in Takahashi's experiment (2 cm) is followed by the suggestion that this will cause any "calibrated" theory to over-estimate the fragmentation. That would be true if no dependence on impact speed were included in the theory and it was simply related to contact area. But that is not the case for our 2017 formulation, which was informed by both lab/field experiments (Takahashi, Vardiman).

We think the term “calibration” induces the reader to view our studies as somehow providing a “speedometer of ice multiplication”. One calibrates an instrument that will not function without the calibration. Our studies are much more than that.

To clarify our position, we added fresh text at lines 323 and 369-376.

Editor: You derive that for c > 1, you have explosive breakups. With no experimental data at hand, which c value would you chose? For example, for a c =1, you may discard the entire effect (if I am not mistaken). c=300 is likely too much, etc. To give you an example what I mean: a speedometer by itself is fundamentally valid, i.e., a linear response to a rotating wheel. However, it is only as good as the calibration is. As the measurement of speed gets better, the accuracy of the speedometer also increases (assuming its theory of operation is correct). Again, this has nothing to do with the foundations of the speedometer/theory. My feeling is that no one doubts fundamentally the theory you developed and applied. However, there are many theoretical studies in our field that need further laboratory data to yield higher accuracy. This is a typical process.

Response: **Yes, if $c < 1$ then the theory becomes superfluous, as we argued here. But that is not likely since the errors in both lab studies of fragmentation are limited, as evinced by Section 4 of the commentary. Takahashi’s own videosonde observations in convective clouds informed the design of his lab experiment.**

Essentially, the process we are describing is fundamentally nonlinear. There is an explosive super-exponential growth of the ice concentration towards the maximum possible for mixed-phase conditions with the positive feedback elucidated by Yano and Phillips (2011) and simulated in detail by Phillips et al. (2017): ice crystals grow to become small ice precipitation (snow or graupel) and then large ice precipitation (graupel), with continual collisions generating ice fragments that grow as crystals, ad infinitum. It is the order of magnitude of c rather than c itself that determines the time-scale of the explosion. So, the analogy of the speedometer would be more applicable if the log of c corresponded to the “speed” that determines the time of the journey.

Yet, as we argue above, a metaphorical “speedometer” is not the aim of our theoretical studies. We are not providing a mere device for measuring the rate of ice multiplication.

We have two independent lab studies, measuring different types of microphysical species in collisions, and they both report appreciable fragmentation. For our detailed simulation of a cold-based convective storm, for which we showed excellent and comprehensive validation (see Figure 6 in the commentary; Phillips et al. 2017a), we estimated that the explosive growth of ice concentration corresponded to $c = 10$ in our model, albeit from breakup in graupel-snow collisions rather than with the graupel-graupel collisions assumed by Yano and Phillips (2011). Phillips et al. (2021, in review) show sensitivity tests with the same simulation and in all the perturbation simulations for varied cloud-base temperature, solute and solid aerosol conditions, updraft speed etc, we find generally the ice-ice breakup boosts the order of magnitude of the ice concentration as the dominant mechanism of SIP among those mechanisms represented in the model (H-M, raindrop-freezing fragmentation, breakup in ice-ice collisions).

In summary, it is the order of magnitude of c rather than c itself that is important for the cloud glaciation, and both lab studies we base our estimates of fragmentation on are not so erroneous as to make c greater than unity when it should be less or *vice versa.*

Note that KL2020 claimed that both lab studies are "conflicting" but they never provided any quantitative evidence to support that claim.

Review: I also feel that the wording in the review article of both studies being conflicting may not be the ideal choice of words. Likely they are not consistent with each other since they were operated a different conditions. However, in my opinion, this is not such an important point. Only more studies will help to achieve convergence and this process usually takes time.

Response: Agreed. The term "conflicting" was unsubstantiated by any quantitative evidence from KL2020. Not only were the conditions of the collisions different (different sizes, different humidities) between both studies but also the microphysical species studied were, mostly, different:

- Vardiman: lightly, moderately and heavily rimed snow crystals, both dendritic and non-dendritic; graupel
- Takahashi: rimed graupel vs graupel in depositional growth

Editor: Regarding the point of failing to include the rotational energy from oblique collisions. I appreciate your discussion on this matter. Your reasoning makes sense. However, and I believe both referees struggled with this, you describe a highly idealized case of spheres. In the atmosphere, we are likely not dealing with spheres but with irregular shaped ice crystals. I can follow your argument in the manuscript, but would it stay around 10% for rotating ellipsoids colliding or other shapes? Of course, you argue for the case of the experiment where spheres were present, but the results of your theory are applied to the "real" world where you likely have not perfect spheres.

Response: Yes, we estimate that for other non-spherical shapes the ratio of final rotational to initial translational kinetic energies would be of the order of 10% or less.

The only material assumption is that the difference in sizes is such that the effective mass of the colliding pair is approximately the same as that of the smaller particle. In other words, the difference in size is at least a factor of 2.

We have added text (lines 273 – 284) to extend our argument for ellipsoids of any shape. The "real world" consists of ice particles whose shapes, albeit irregular, may be approximated by prolate (columnar crystals) or oblate (planar crystals, snow, graupel, hail, freezing drops) spheroids.

Editor: I do not think that this issue needs to be further discussed in detail here but maybe acknowledging caveats, when data is lacking, may be appropriate. In other words, your discussion may imply that this effect is "always" negligible although we have never "observed in situ" two colliding particles followed by breakup. Someone operating in the atmosphere (not lab or model) may misunderstand your points and will just disagree instead of seeing the broader application of your theory. Hence, providing caveats may be beneficial, also to motivate more research.

Response: The caveat in the context of the rotational rebound issue is added as required (lines 295-296).

Editor: Referee #2 and I feel this is valuable addition. I hope these thoughts help to perform the last minor revisions before publication of your manuscript.