Summary of Author Responses to Reviewers

It should be emphasized that neither of the reviewers point out anything to invalidate the merits of our commentary article. Thus, we believe that it must be accepted for publication, while also taking into account some of the comments of the reviewers.

Reviewer 1 only comments on secondary issues, and whatever way we adjust the comment text, these changes will not affect our main messages. The Reviewer 1 just needs to be slightly more concrete about the comments so that we can take them into account.

It appears to us that the Reviewer 2 rather misunderstands the basics of the scientific logic applied here. By our own interpretation, the comments by Reviewer 2 are based on those misunderstandings, and we find no basic validity. However, if we have misunderstood the comments, we sincerely request clear elaborations by this Reviewer.

In summary, we believe that the present comment must be accepted as it is, since neither Reviewer can provide comments that are mostly constructive.

<u>Reply to Reviewer 1</u>

<u>Author Response</u>

The following comments in the review, unfortunately, take the form of a collection of expressions of personal judgments, but without providing any supporting evidence for these judgments. More than often, stated judgments are hardly elaborated. Thus it is just impossible for us to improve anything in response.

It is unfair that the review makes no attempt to provide any reasons when stating brief opinions. We highlight this where it occurs below.

Any academic journal is a forum for reasonable debate. The review process normally includes such debate. For any debate to be reasonable, reasons should be provided for opinions given.

<u>Point-by-Point Comments</u>

Reviewer: 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? Reviewer: 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.

Response: As this leading paragraph suggests, the present Reviewer is inherently skeptical about any theoretical studies unless there is overwhelming support from laboratory experiments and field observations. We do not wish to change the Reviewer's personal perspective on this matter. We just need to point out that stand-alone theoretical studies are possible for the SIP as demonstrated by Yano and Phillips (2011), Yano *et al. (*2016) and Phillips et al. (2017a). Those studies demonstrate a potential possibility of an explosive SIP, that is of course, to be verified by observations.

In contrast with what the Reviewer suggests above, we have never stated at any place in our commentary article that "*there is little need to do more laboratory experiments*". Our own position is totally the opposite: any healthy progress of science is possible only when both theoretical and modelling studies on the one hand, and the laboratory experiments and field observations on the other hand, are working together as equal partners. As KL2020 suggests in their conclusion, if we begin to argue that one side depends on the other totally, we are going to lose any healthy progress.

Naturally, we do not say that no progress is possible on one side, at all, without any progress of the other, which would mean that one side of the progress is only possible with the help of the other side. In fact, it is possible to launch a field campaign to look for a new mechanism of SIP, when there is only a very vague theoretical speculation to justify this. Likewise, it would also be possible to imagine a situation of performing some theoretical studies where, for whatever reason, there were to be only suggestive support from the laboratory experiments, or even just a very vague observational suggestion. Such stand-alone theoretical studies would have a role in the wider scientific context.

Finally, regarding what we actually wrote about sublimational breakup, there was quite a dramatic debate in the online interactive exchange. We clearly won the argument: KL2020 had claimed a short duration of sublimation is needed for fragments to survive, while we proved analytically that a quasi-equilibrium ice concentration arises that persists during lengthy convective descent. Regarding what we wrote about breakup in ice-ice collisions, in the last round we included many validation plots for aircraft data for storm simulations (Phillips et al. 2017b), deploying our formulation of this breakup in a cloud model (Fig. 6 of commentary). Only with the formulation included were the aircraft observations reproduced.

It is unsurprising that the review makes no mention of this: these are both debates that we won.

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.

Response: The Reviewer unfortunately chooses to disagree with us here. However, without any elaborations, we cannot further comment on this.

Reviewer: …. 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

Response: This is just a fundamental point: even without any laboratory experiments, certain theoretical studies are always possible. This does not mean to deny an importance of laboratory studies. We are just saying that it is wrong to suggest that the theoretical and modelling studies inherently depend on laboratory experiments: theories and modelling on the one hand, the laboratory experiments and field studies, on the other hand, are definitely mutually dependent, and it is rather unhealthy to suggest one side totally depends on the other.

That was not an expression of overconfidence, but rather was a simple statement about the basic nature of theoretical and modelling studies.

Reviewer: 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.

Response: Although the Reviewer thinks that our previous response was not good, we cannot comment back on this without any reasons given.

Reviewer: 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.

Response: Here, again, the Reviewer chooses to disagree. However, we cannot comment back on this without any reason given.

Reviewer: 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.

Response: Again, it is difficult to respond to such tangential isolated comments when no coherent argument is expressed in the review.

Reviewer: 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.

Response: We agree. The reviewer proves the point we are making: the most prolific type of breakup in ice-ice collisions involves graupel, because graupel is dense and has the greatest CKE.   Phillips et al. (2017b) showed that graupel-snow collisions create the most ice fragments out of all types of collisions.

Hail (defined as > 5 mm) is exactly the same general type of particle as graupel (defined as < 5 mm), with the only difference being that it is larger and hence denser. Both types of particle grow predominantly by riming, hence the general densification as they become larger.   Density of accreted rime increases with fall-speed as the particle grows from being graupel to hail.

In none of our theoretical studies do we actually apply to all sizes of graupel the exact fragmentation number measured for hail-sized ice spheres.

Reviewer: The Takahashi 1995 study is not valid for the vast majority of situations.

Response: We disagree.   Yes, most clouds displaying SIP do not have 1-2 cm hail. No, that is not a problem because such clouds do usually involve graupel and graupel is the same general type of particle as hail, as noted above. And our theoretical formulation (Phillips et al. 2017b) is universally applicable to all sizes of particle, so that fitting its parameters to the Takahashi results allows it to be then applied to collisions among graupel/hail of all sizes.

Reviewer: 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.

Response: This is an extraordinary claim by the review.

The context for our quoted statement in the last round of responses was: "*Perfection is not needed for the effect from breakup to be simulated realistically, because there is nothing controversial about supposing that the initial kinetic energy in the frame of reference of the centre of mass of the two-particle collision is the source of energy for the fragments irrespective of whether both particles are free".*

We never wrote that energy conservation is the only law of conservation to apply to a collision nor that it is the only constraint, nor did we write that the actual magnitude of the initial kinetic energy is somehow independent of whether both particles are free (we wrote that it is not). We were writing about the general principle of conservation of energy, which is part of the First Law of Thermodynamics.

The law of conservation of energy is ineluctable as a basic principle of science. It states that the total energy of an isolated system is constant. This needs no experimental verification. It is the basis for the entire field of classical mechanics in Physics for the last few hundred years.

Here, the present Reviewer asserts that the adoption of the energy conservation principle here is "inadequate", and insists that it must be proved by laboratory data. Of course, such an insistence is just unreasonable: throughout the history of science, it is common knowledge that no physical principle was ever "proved" by laboratory data, which is always imperfect. Only after enough "supports" (these hardly constitute "proofs") by laboratory data do scientists decide to accept these principles.

Replies to Reviewer 2

Author response

We are grateful to the reviewer for their effort in scrutinizing the manuscript.

Point-by-point Responses

Reviewer: The authors claim that 2 statements in the review article by Korolev and Leisner (hereafter KL2020) are misleading and distort the validity of their contributions.

Statement 1: The theoretical framework of collisional fragmentation developed in Yano and Phillips (2011), Yano et al. (2016), and Phillips et al. (2017[a]) was calibrated against experimental results of Vardiman (1978) and Takahashi et al. (1995).

Here what the authors said in their articles:

P.2 of Yano2011 states, "The goal of the present article is to demonstrate the important efficacy of this mechanical breakup (or fragmentation) process by a theoretical investigation. For this purpose, we take the parameters estimated by more recent laboratory data (Takahashi et al. 1995)."

P.2 of Yano2016 states, "Yano and Phillips (2011) and Yano et al. (2016, here- after YP11 for the former and YP collectively) show, under a deterministic approach, by taking the experimentally estimated parameters by Takahashi et al. (1995), that the ice breakup process can indeed lead to explosive ice multiplication under certain regimes."

P12 of Phillips2017a states, "Theoretically unknown parameters are estimated from the observations, both from outdoors and laboratory experiments, by Vardiman (1974, 1978) and Takahashi et al. (1995)."

Although the word "calibrated" is not used in the text, it does characterize pretty-well what the authors state in their publications.

In this manuscript, one of the main points is to lessen the stated connection between their modeling results and the Vardiman1978 and Takahashi1995 experimental results. The authors claim their modeling study results stand despite the possibility that the experimental results could be 'totally erroneous'.

I think this is correct and perhaps a better characterization of the connection is that the modeling results are consistent with these previous laboratory experiments. This idea is stated well in the manuscript. But this idea is more of a correction and corrigendum to the author's previous statements than it is a comment on Korolev2020.

Response**:**

We never wrote that the experimental results from Vardiman and Takahashi could really be totally erroneous, nor do we believe this to be even a real possibility. We were merely mentioning a purely imaginary scenario, for the sake of argument ("*Secondly, hypothetically our previous theoretical and modeling studies would still be valid even if all the previous laboratory/field experiments about fragmentation in ice-ice collisions were to be shown to be totally erroneous*").

In criticizing our comment concerning the Statement 1, we are afraid to say that the review fails to convey any understanding about the difference between "calibrations" to adjust the model and simple "estimations" of values. A calibration usually suggests that a given model does not properly function without this procedure. That is hardly the case here for any of the papers cited (Yano and Phillips 2011; Yano et al. 2016; Phillips et al. 2017a).

Both Yano and Phillips (2011) and Yano *et al*. (2016) provide theoretical analyses that do not require calibrations from any laboratory experiments. The behavior of both versions of the model is defined solely in terms of a single nondimensional parameter, $\tilde{c}$. Both analyses were performed over a full possible range of $\tilde{c}$ without referring to any laboratory experiments. That is exactly what we mean by both theoretical studies are 'stand-alone'. Although this point was not explicitly stated in the original articles, it must have been obvious for all the readers who already know how to interpret the theoretical studies. We are afraid to say that both the authors of KL2020 as well as the present Reviewer do not understand the basic nature of theoretical studies.

As these three quotations above show, we do refer to those laboratory experiments for the purpose of estimating the value of this nondimensional parameter, $\tilde{c}$. However, the theory itself does not need this specific number. The sole purpose of getting the number is to infer where a typical atmospheric cloud is situated along the coordinate of this nondimensional parameter so that a link between the theory and the real world can be established in a solid manner. However, even if (that is a purely hypothetical situation) new laboratory experiments in future, somehow, were to provide completely different estimates, then there would be no need for us to repeat those theoretical studies again: the theory part would stand by itself, because our existing studies are not based on mere "calibrations" as KL2020 wrongly assert.

Similarly, Phillips et al. (2017a) created a theoretical formulation of the numbers of fragments per collision by deriving an expression from the law of conservation of energy. An energistic coefficient of restitution was applied. A statistical distribution of the strength of asperities was derived from other observations to create the expression. The main thrust of the study was provision of a versatile framework into which future lab observations could be assimilated. For an application to real simulations, some of parameters were inferred from observations by Vardiman (1978) and Takahashi et al. (1995).

Thus, the above sentence coupled with the very next three sentences in KL2020 together give a false impression that somehow the formulation by Phillips et al. (2017a) was obtained by simple curve-fitting to lab data and would have never functioned properly without both lab/field studies. Moreover, when the formulation is applied in a cloud simulation, as argued in our commentary there is a lack of sensitivity of the eventual ice concentrations to errors in the formulation since the simulated cloud system is in the explosive unstable regime anyway.

Reviewer: *Statement 2: No parameterizations of SIP due to ice–ice collisional fragmentation can be developed at that stage based on two laboratory observations, whose results are conflicting with each other.*

It is not clear how this statement applies to the author's work since they claim their model formulation and result is not based or dependent on the experimental work. They claim their work is valid even if the experimental work is 'totally erroneous'. The authors explain this well and this warrants publication as it emphasizes a point not well stated in their previous publications. But it would be nice of the authors also were to add some new results.

After describing the Vardiman1978 and the Takahashi1995 results, KL2020 claim it is hard to judge the consistency of the results given the differences in experimental setups and conditions. I agree but they then go on to state these results are conflicting. I agree with the authors that the word "conflicting" is likely not a proper characterization of the 2 laboratory studies. One has c within the range 1 -100 and the other has c = 50 (which may be corrected due to contact area corrections as the authors suggest, to 5-10). The authors make several good points about their model results being consistent with the experimental results. But they also concede that the laboratory experiments could be totally erroneous. So, while it is fair to disagree with the KL2020 characterization, all this hand waving will not be settled without more laboratory work.

The KL2020 review was not focused on modeling efforts. Phillips2017b cloud model-field data comparison shows good agreement with a fragmentation parameter consistent with the laboratory experimental results. This work certainly stands on its own. I suggest the authors reconsider the title of the manuscript. Much of the manuscript stands on its own as a clarification of what was previously stated in the author's publications.

Response: The statement 2 here is simply logically wrong: it is not true that neither theoretical nor modelling progress is possible without any further laboratory experiments. Even in the absence of those two laboratory/field experiments (Vardiman, Takahashi et al.), our theoretical studies (Yano and Phillips 2011, Yano et al., 2016) would have been possible as already emphasized above. Our modelling development has also been possible, thanks to already available knowledge form the studies in statistical physics, although with uncertainties in specifying some model parameters.

We are not sure how to respond to the actual comments by the Reviewer, who half agrees with us, but also half disagrees with us. One wonders if there is any fundamental disagreement here.

Reviewer: Smaller points:

Line11-16: Please break this into 2 sentences. There is also a typo here. Also be careful with the word 'valid' - usually this means validated by experiment. But I'm having trouble following the logic: How can the theory or model be validated by experiment when, at the same time, the authors concede the experimental results could be totally erroneous? You can't have it both ways can you?

Response: There was a misunderstanding of what we wrote here. We have never validated our any application of our theoretical formulation with the lab data used to constrain any of its parameters (Vardiman, Takahashi *et al*.).

No, 'valid' does not necessarily mean validated by comparison with observations. Rather the technical term is "validation" or "to validate", as in "model validation".

Anyway the sentence has been re-phrased and condensed (line 15).

Reviewer: Line 107-108: 30 minutes is not the case for all \tilde{c}. Time is much longer for the range \tilde{c} \sim 1 to 10 as shown in Fig 3. Perhaps mention what is the lowest value of \tilde{c} consistent with the time-scale observed for the clouds in the field observations?

Response, lines 127-131: Done.

Reviewer: Line 179: The word 'consistent' rather than validated?

Response: No, the term "*validated*" is fairly applied everywhere. We will not remove the term.

"Model validation" refers to the technical procedure of comparing a model prediction with coincident independent observations, after using observations of the case to initialize the model. Figure 6 shows many "validation plots".

Phillips et al. (2017b) use the term "validation" for this rigorous comparison with aircraft observations.

Reviewer: Line 265: Better to state the limit rather than 'is minimal'.

Response, line 272: Done.

Reviewer: Line 268: The 1% is only true for a certain range of values given the curve

Response: Yes, it is the value for the slowest impact speed observed by Takahashi et al. The other impact speeds they observed have an even weaker percentage change than this (< 1%) because they observed the fragmentation to approach an upper limit as the speed increased.

Text is now clarified (line 267).