Response to Review 2 of "Improved Formulation of Fragmentation of Snow during Collision with Graupel/Hail based on Observations at Jungfraujoch: Cold NonDendritic Regime of Temperature"

Reviewer: This paper investigates the fragmentation of naturally falling "non-dendritic" ice particles at the Jungfraujoch station using the portable instrument developed in Gautam et al. (2022, 2024). The results of the experiments are fitted to the Phillips *et al.* (2017) formulation and some numerical tests are performed. The overall concept of the study is interesting, especially as only a limited number of studies have explored this SIP mechanism. Examining the fragmentation of naturally falling ice particles is particularly relevant, as it enables the observation of numerous collisions without the need for artificially generated ice particles.

However, I have significant concerns about the major limitations of the experiments, which are not addressed in the paper. A key issue is the use of a GoPro camera, which is not a research instrument and is therefore not designed for measuring ice particles (It is primarily used for filming sports). Consequently, a substantial proportion of fragments is likely undetected, yet this limitation is not discussed in the study. Additionally, the properties of the ice particles such as fall speed, mass, rime fraction, and collision kinetic energy (CKE) are subject to large uncertainties due to the methodology employed. Important elements regarding the numerical modelling setup are not provided. The presentation of the figures could be improved to enhance clarity and readability.

Overall, while the study's concept and topic are relevant, the paper exhibits several methodological weaknesses that undermine its rigour. Without substantial improvements, I cannot recommend publication in ACP. If the authors thoroughly address these issues, the study could become suitable for publication. I therefore recommend a major revision.

Response: We are grateful for the critical appraisal of the manuscript, which will help us to improve its quality. We agree that a better camera would be an improvement and that the detection threshold was a weakness. In fact, we have altered our estimate of this threshold to 200 microns (300 previously, from more thorough inspection of the images of collisions.

We see no evidence for the claim that a substantial proportion of fragments is likely undetected. Nevertheless, we heed the review's warning and will try to improve the apparatus in future.

That claim is difficult to reconcile with the ineluctable evidence from the fragment size distributions shown in the paper. They show a clear disappearance towards zero of the fragment number with decreasing size well above the limit of the resolution and only one peak overall.

Are we to believe that there is some vast hidden second peak of fragmentation at small unresolved sizes (50 microns) that we missed ? It seems unlikely. **It is not true that Grzegorczyk et al. (2023) ever saw such a small-size mode for natural snow: they did not study natural snow at all. Their peak at 50 microns of fragments, which they suggest was an artificial artefact of their experiment, was far less numerous than the major broad peak at about 400 microns.**

All the fragment size distributions ever plotted in studies of natural snow, which are by Vardiman (1974, thesis), and Gautam *et al*. (2024), show only one peak, as expected theoretically. As noted in replies to reviewer 1, Lo (1983) observed the fragmentation of artificial snow including tiny sizes and our inspection of their published fragment size distribution suggests that it conforms to a lognormal form, as expected theoretically from the multiplicative nature of microscopic breakages on impact.

As required, throughout the paper we have made improvements to the description of the model setup, the presentation of figures, and the assessment of uncertainty. Two new sections about implementation of the scheme and about discussion have been added (Sections 7 and 8).

Reviewer: The resolution of the GoPro must be explicitly stated in the paper. Is it the same as reported in Gautam et al. (2024) (150 µm)?

The detection limit is given as 300 µm (line 238). However, if the resolution is 150 µm, this corresponds to only two pixels, which is insufficient for reliable detection. In situ aircraft probes typically require at least three pixels to identify hydrometeors, suggesting that the detection limit should be no less than 450 µm.

Response:

We now think the detection limit was more similar to that found by Gautam *et al.* (2024): 200 microns. We have altered Figure 4 accordingly.

We do not know how many pixels this corresponds to, but it is probably about 4 pixels in view of a theoretical formula for the pixel size of the goPro camera.

We now state the resolution of the camera at various places (lines 20, 298, 306, 472, 524, 538).

Reviewer:

Strong evidence indicates that SIP generates fragments smaller than 300 or 450 µm. Numerical simulations by Lawson et al. (2015) and Huang et al. (2021), where SIP was turned off, revealed a lack of ice crystals around 100 µm compared to in situ aircraft observations. This suggests that small fragments are likely missed during your experiment.

Response:

We disagree.

The review mentions SIP as if it is some single process. It is not. It is a bunch of disparate mechanisms with contrasting physics.

Lawson *et al.* (2015) were observing raindrop-freezing fragmentation, not breakup in ice-ice collisions. This is because their aircraft sampled young convective turrets ascending through the mixed-phase region with high concentrations of supercooled raindrops and drizzle. We simulated their observed ice concentrations and LWC in our 2018 paper with a parcel model of bin microphysics (Phillips *et al*. 2018, JAS). Sure, raindrop-freezing fragmentation may generate both big and tiny (e.g., < 0.1 mm) fragments and tends to prevail if there is a strong warm rain process and the cloud is sampled while young, as with their study near the Caribbean (Lawson et al. 2015). Breakup in graupel-snow collisions, which tends to yield larger fragments (e.g. about 10% of the size of the parent snowflake), takes much longer to become prolific and cannot happen appreciably yet in a young ascending turret (Phillips *et al.* 2018; Waman *et al*. 2022). So numerical simulations of such young turrets by Lawson et al. are not relevant to the present paper, which is about only breakup in ice-ice collisions.

Huang *et al*. (2021) showed only simulations apparently without any SIP and they never managed to obtain agreement with the observed ice concentrations. The difference between the numerical simulation PSD and that observed cannot be reliably attributed to SIP in their plots, as there may have been innumerable deficiencies of the models they used. A simulation that has been invalidated rather than validated, against observations, is no simulation at all. Even if the difference could be attributed to SIP, most of the ice particles of surplus predicted by Huang *et al*. seem bigger than 400 microns.

In summary, there is a variety of SIP mechanisms and they can generate fragments in contrasting size ranges. Aircraft data have never measured the sizes of fragments specifically from our target mechanism of SIP, namely breakup in ice-ice collisions. So we cannot say what their sizes should be. Fresh text has been added to clarify in the new discussion section (lines 470-496).

Reviewer: This limitation should be clearly addressed in the abstract, results, and conclusions.

Previous studies demonstrate the challenge of detecting small ice particles produced by SIP. Lauber *et al.* (2018) and Keinert *et al.* (2020) used a 20 000 fps camera with around 1 µm pixel resolution. Even with such advanced imaging, Kleinheins *et al*. (2021) found that during some pressure release event likely producing secondary ice splinters, fragments were not detected. The recent experiment of Seidel et al. (2024) employed a sophisticated ice counter for the Hallet–Mossop process, using the impaction of splinters on a supercooled sucrose solution.

Response: We are grateful for the reviewer's suggestions about ways to improve the apparatus.

However, again these cited studies are for an SIP mechanism that is not relevant to the present paper. All of the SIP observations noted here in the review are for mechanisms involving breakup during drop freezing. This includes the HM process, with freezing of cloud-droplets (about 25-30 microns diameter). The studies by Lauber *et al.* (2018) and Keinert *et al.* (2020) are only for raindrop-freezing fragmentation. These fragmentation processes all involve emission of tiny fragments of ice of about 10 microns in diameter (Phillips et al. 2018). In such fragmentation, the energy is from the mechanical stress of an outer ice shell from expansion of trapped inner liquid freezing.

By contrast, the fragmentation in ice-ice collisions of the present study involves completely different physics of the solid phase only, and the energy for fragmentation is from the collision kinetic energy. That is why fragments tend to be larger generally, being about an order of magnitude smaller than the parent particle mostly.

Regarding the frame speed (fps), in our study the incident snow particles are only falling at about 1 m/s, which gives about 1 cm of fall per frame. The field of view is quite wide and the fragments will be arrested by their drag force soon after emission and are practically suspended in the air. So there is no rush to observe them before they leave the field of view.

In summary, the studies cited in the review (Lauber, Kleinert, Kleinheins) have little relevance to the present study. Yes, the fragments observed in those studies were tiny, requiring a spatial resolution of about 2 microns, which is much finer than ours (0.2 mm). But that was for a completely different type of SIP involving drop-freezing that is known to produce tiny 10-micron fragments. Our mechanism of breakup in ice-ice collisions does not involve any freezing. Fresh text has been added to clarify (lines 470-496).

Reviewer: Grzegorczyk et al. (2023) employed both holography (10 μm resolution) and microscopy (3 μm resolution) to study ice fragmentation. Compared to these approaches, the current setup (only 120 fps and 150 μm resolution) is limited, and improvements should be considered.

I strongly recommend improving this aspect for any future observations.

Response:

We agree that the spatial resolution (0.2 mm) of our camera setup could be improved, and we will heed the reviewer's warning in future experimental work. GoPro cameras are intended for wide views of common scenes and are not intended to be focused on close objects.

Perhaps in future work we will try to get a camera much closer to the impact, perhaps with a special lens as in the studies. However, I doubt that any hidden mode of fragmentation will be discovered with an improved camera.

Reviewer: The inability to capture small ice particles produced by fragmentation in your experiments is an important limitation. Grzegorczyk et al. (2023) demonstrated that fragmentation generates fragments smaller than 300/450 μm, reinforcing the need for discussion on this limitation.

The use of a single camera is insufficient, as out-of-focus ice crystals may be missed. Grzegorczyk et al. (2023) showed that a high-speed camera detected only ~10 ice crystals, whereas microscope and holography measurements captured significantly more (~100) in the same experiment. This again suggests that the current study likely underestimates the number of detected fragments.

Response: Yes and no.

By reading their paper (Grzegorczyk *et al*. 2023), I cannot see a factor of 10 discrepancy mentioned anywhere (search their paper PDF for "camera" or "holograph"). When I contacted the author, they claimed that the issue was the sample volume and the optics limiting the sample volume and depth of field, rather than the spatial resolution at optimum distance. If only a small volume was in sharp focus, and the colliding particles were in free-fall, then that would explain why many fragments were missed with the camera of Grzegorczyk *et al*.

In our study, the spatial resolution was slightly better than for the camera in the study by Grzegorczyk *et al*. 2023. Although our GoPro camera was not good for focusing on such close objects, it does have a wide and deep field of view. So their problem of fragments falling out of the field of view does not apply to us.

Grzegorczyk et al. (2023) made lab observations only of artificial snowflakes created manually, not natural snow, which would likely have created unnatural fragmentation. Indeed, their paper admits this:

> "*It is very likely that the manual production of snowflakes generated diverse ice structures for the different snowflakes that could lead to distinct fragment sizes and therefore to the presence of a second mode around 50 µm for some collisions. Since only 16 FSDs are presented here, more experiments have to be done to clarify the observation mentioned before*."

They say this because they themselves made their own snowflakes for their own experiment. The small minor peak (50 microns) coincides with the sizes of the crystals (about 50 microns) they generated and manually assembled to construct the artificial snowflake (1 cm). The minor peak is likely to be simply those small component crystals, which seem rather small.

Regarding their published plots, when we look at the published fragment size distribution from Grzegorczyk *et al.* (2023), (**Fig. B**), we see that most of the fragments are at sizes > 0.3 mm, which is above our detection threshold (0.2 mm). There are two modes, a broad one around 400 microns and the other noted above at 50 microns. The one at 50 microns has relatively few splinters, if one integrates the area under the curve. So even if it were somehow present in our data with natural snow (it is likely an artefact as noted above) and missed because of our spatial resolution, its omission would not introduce much error.

Anyway text has been added to discuss these limitations of the camera in a new discussion section (Sec. 7; lines 470-496). We do not see evidence of any serious problem of undercounting here.

Detection
threshold of
present paper
(0.2 mm)

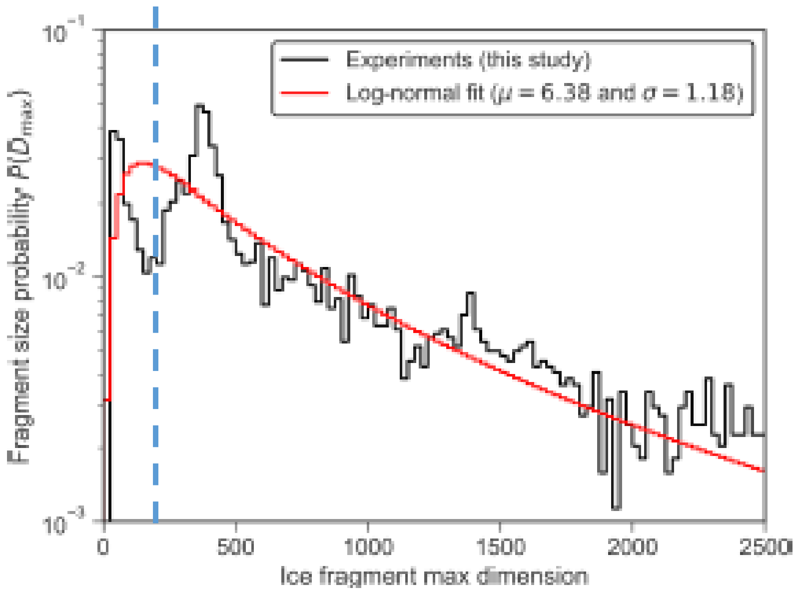

Figure B: *fragment size distribution for graupel-snow collisions measured by Grzegorczyk et al. (their Figure 15). The detection threshold from the present study is superimposed (dashed blue line).*

Reviewer: Given these limitations and the evidence of the presence of smaller ice fragments, the paper should explicitly state the number of fragments is a 'minimum number of ice fragments' larger than the threshold size in the abstract, results, and conclusions. Caution is needed when interpreting the findings, and I strongly recommend adding a discussion section addressing these limits based on the relevant references mentioned previously.

Response: We have introduced a caveat about this detection threshold in the Abstract (line 20) and concluding section (line 538).

Reviewer: The original formulation of Phillips et al. (2017) suggests that the mass of fragments of 0.001 times that of the parent particle (except for hail). Based on the findings of this study, did you modify the mass of the fragment? If so, does it modify the particle size distribution of ice crystals obtained with the AC model?

Response: This is an excellent point. No, we did not modify the prescribed fraction (0.001), but perhaps we should.

In the present study, we see that the fragments are mostly at a size of 20% of that of the parent snow particle on average. That implies a ratio of the mass of 0.008. This is rather different from 0.001 in the original paper. Our present result is for non-dendritic ice from cold cloud-tops and compares with a ratio of about 0.001-0.027 for non-dendritic ice with warmer cloud-tops from Gautam *et al*. in Sweden.

We have not performed a simulation yet to check the effect on the cloud simulation from these larger fragments relative to our 2017 paper. In future studies, we may do so to evaluate the impact on particle size distributions. Thank you for the excellent idea.

We have added text for a new implementation section, including formulae from Gautam *et al.* (2024) (with corrections of typos) for the prescribed size of fragments (lines 438-444) with a new Eq (4).

Review: Line 51-73: There are repetitions; I suggest reorganising this paragraph. Additionally, more studies that have implemented the Phillips et al. (2017) parameterisation and demonstrated improved ice crystal number concentration compared to observations should be cited (e.g. Huang et al., 2022; Karalis et al., 2022; Han et al., 2024; Grzegorczyk et al., 2025).

Response: Well spotted.

This reorganisation and inclusion of these references has been done with fresh text (lines 70-94).

Review: Line 60-61: Mention the Grzegorczyk et al. (2023) study, which observed a large number of fragments during graupel-snow collisions.

Response: Grzegorczyk *et al*. (2023) is now mentioned as required in the introduction (lines 62-65).

Review: Line 97: Was the camera able to capture all ice spheres where ice breakup occurred? Given that the distance between spheres was 2 cm, was any rebound of snowflakes between the spheres observed when they were around 1 cm in diameter? What was the reason for using a 2 cm ice sphere as a proxy for graupel/hail instead of a smaller target size?

Response: Yes, all the ice spheres where breakup occurred were in the field of view of the camera. We used the ImageJ software to zoom in on the small region where each impact happened.

No, the spacing between spheres was 2 cm. The actual size of the ice spheres was much less than that, only about 0.5 cm or less.

We have clarified the size of the ice spheres (lines 117-118).

Review: Line 110: It is written that the cloud top was between -25 and -32 °C while the ground was at -5°C and RH=27%. Regarding the distance between the cloud base and dry conditions at the ground, I think that sublimation might affect the shape of the particle and therefore the fragility of the ice particles.

Response: We agree this is an issue.

We sought to avoid this problem by prioritizing snow above cloud, which is partly why we sampled at the Jungfraujoch mountain-top. But we were below cloud-base.

The air was subsaturated, with a supersaturation with respect to ice of about -70%.

The snow particles would be expected to have lost some fraction of their mass by sublimation. The extent to which this is so depends on the local cloud-base.

Here is the vertical profile of liquid water content of cloud from ERA5 (resolution 31 km), (**Fig. C**). Over the station, the local cloud-top and cloud-base were higher. However, the sampling (at -4 degC) was above the large-scale cloud-base, which was near the freezing level.

The local cloud-base, relative to the station, was never measured. If the local cloud-base were say 5 deg K colder than the large-scale cloud-base (0 degC), then it would be about 100 metres above the sampling level (-4 degC). At the local cloud-base, the supersaturation (with respect to ice) assuming exact water saturation would be 5%. Thus, descent at 1 m/s would take about 100 sec and would involve an average supersaturation (ice) of perhaps about -30 %. That would cause shrinking at a rate of about 5 microns per second of the falling snow particles (see Deshmukh *et al*. 2022, their Figure 11, red dashed-dotted curve). If at initial sizes of about 5 mm, the average snow particle would shrink by about 10% in size. This would affect the fragility of the snow, since 30% of the mass would be lost in this scenario. But the effect would likely not be huge.

Fresh text has been added in the discussion section about this potential bias (lines 508-512).

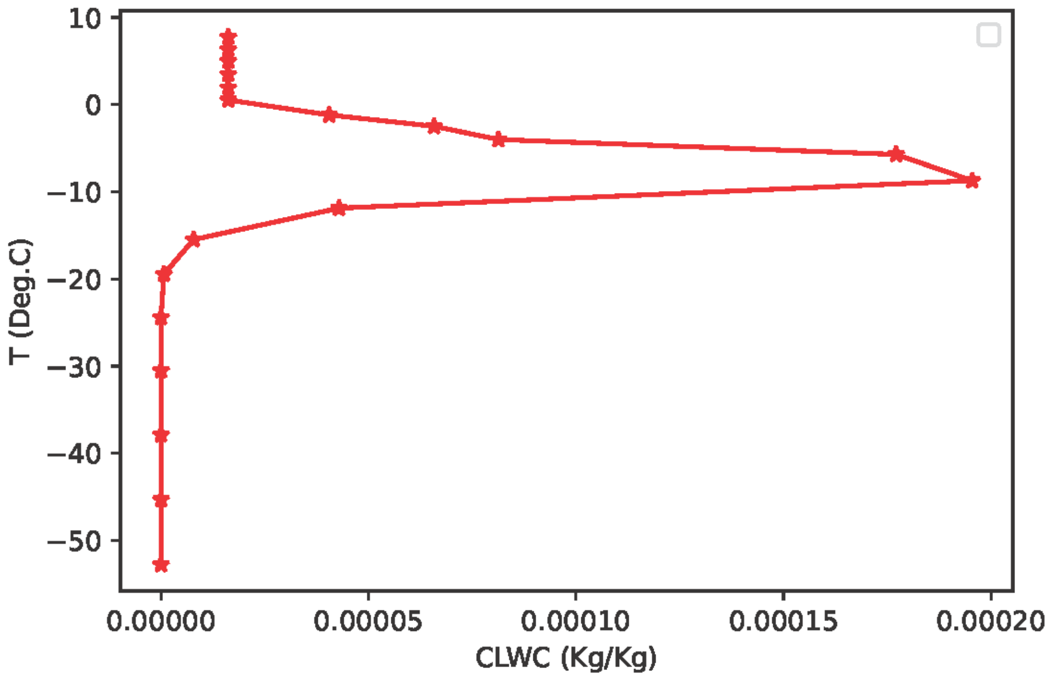

Figure C: *vertical profile of cloud-liquid water content from ERA5 re-analysis.*

Review: Line 114: On what observations is this statement based?

Response: There were no direct observations of snow habit since we could not resolve the snow crystal habit with our camera. Instead, the satellite data showed a local cloud-top of about -25 to -32 degC. The crystals growing at that temperature would be expected to grow non-dendritically above the dendritic region mostly.

Extra text has been added to clarify (lines 137-139).

Review: Figure 2: The ice crystal images are of poor quality, please provide better quality images and, if possible, include video of breakup events or at least some images.

Response: We will provide some videos of collisions when the paper is accepted for publication.

Unfortunately, the camera is at the limit of its field of focus so there is little that can be done to improve the image quality. The issue is the camera, not the image processing afterwards.

We have included an extra panel in Figure 2 with an image from the video camera of a fragmentation event. The fragments are visible.

Review: Line 160: The mass of the particles is estimated based on the integrated mass, which is used to derive the m-D relationship. How were these ice particles observed? Was a GoPro camera used? More details are needed. Given the significant variability in ice particle properties, the uncertainty in mass as well as parameters in Table 1 should be assessed.

Response:

- a GoPro camera was used to measure the sizes of all ice particles falling into each capsule while it was open. Their individual masses were not measured.
- Only the total mass of all sampled crystals in the capsule was measured.

Least squares fitting provided the two parameters of the mass-size relation, from these measurements.

At line 317, we state that the error in estimating the size was 13%, causing an error in inferring the mass per particle of 20%.

The parameters in Table 1 for the mass-size relationship (a and b) are not an appreciable source of error since so many crystals were sampled in both capsules.

We have evaluated errors of both parameters in Table 1. The errors are tiny since the sample size is so large. Thus, the only error in estimating the mass of any snow particle (20%) is from estimating its size (13%). Extra text has been added as required explaining the derivation of a and b (lines 217-220).

Review: Line 166-167: Was the size and number of fragments inspected manually for 100 collisions? Please provide more details about the method and some images.

Response: Yes they were inspected manually by the first author who used ImageJ.

Extra text is now added at lines 204-208. Extra images are provided of fragmentation events in Figure 2.

Review: Section 2.6: The simulation performed to obtain the rime fraction was initially design for California which is a completely different from the situation at Jungfraujoch in Switzerland, why using it? Please clarify that, give more details about the simulation setup, results and how the mean rime fraction was calculated.

Response:

It is a different location but the cloud-type is not so different. It is a mixed-phase stratiform cloud that was observed in a field campaign and our simulation of it was validated in published papers that we cite.

To find the average rime fraction, we choose the cloudy part of the large mesoscale domain (300 km long) for averaging that corresponds to the cloud temperatures actually observed at Jungfraujoch.

Extra text, especially about the model setup for the California case and derivation of rime fraction, has been added to clarify (lines 230-252). There is also new discussion added about uncertainty of inferring the rime fraction (lines 499-506).

Review: The number of asperities which is a key parameter relies on the rime fraction while this parameter is only estimated from numerical simulation. This is very dependent on the ability of the microphysics scheme to reproduce the real cloud situation. The uncertainty about rime fraction and its importance must be more discussed.

Response:

More discussion of the rime fraction uncertainty is included in the new discussion section (Sec. 8; lines 499-506). The uncertainty in rime fraction we now quantify with an error of 20%.

Review: Griggs et al. (1986) show that riming tends to inhibit fragmentation while it is the opposite in Phillips et al. (2017) formulation, did you have some information about that from the apparent shape of the particle observed during your experiments?

Response: It was difficult to discern from the video the degree of riming.

The type of riming studied by Griggs et al. is quite different from the riming being considered in the present paper. They studied graupel particles that they created artificially in the lab.

The riming in the present paper is simply that of the snow particle. Most of its mass is not rime, otherwise it would be a graupel particle.

There is no contradiction between the present paper and the study of Griggs et al. since the riming being referred to is of contrasting types of ice particle (snow vs graupel).

Review: Line 227-228: Larger ice particles may fall faster, have higher mass and CKE, and contain more asperities that could break. How do you explain, then, that larger ice particles produce fewer fragments? One possible reason is that higher CKE may generate fragments that are too small to be detected.

Response:

We have revised Figure 3 and those findings no longer apply.

We now find that there is a general increase of fragmentation with increasing size in the observations through the entire size range. However, there is a slight 'plateau' at sizes similar to, and larger than, the size of the ice spheres, where the contact area no longer increases with size.

Text has been updated (line 286).

Review: Line 229: Change "number" to "minimum number is about 5."

Response:

We disagree.

As argued above, we see no evidence for any systematic under-counting. We plotted the fragment size distribution in order to check the goodness of the counting and Figure 4 shows the single peak that is expected is fully resolved.

Review: Figure 3: The figure appears to be average, while line 222 indicates that approximately 100 collisions were observed. Could you show all the raw measurements?

Response: Done with the new Figure 3.

Review: Line 237-238: This cannot be stated, see my previous comments.

Response: We do not agree.

This statement has been modified but not removed. It is now at lines 297-299: "T*he single peak observed is well above the threshold size of detection (0.2 mm), which is consistent with almost all secondary ice fragments from each collision being visible and detected.*". We think this is a fair statement.

There is no evidence for a hidden peak of fragmentation existing at unresolved sizes of fragments. The lab experiments for drop-freezing fragmentation mentioned in the review are irrelevant here and the fragment peak at 50 microns observed by Grzegorczyk *et al*. (2023) was disavowed by Grzegorczyk *et al*. in their published paper as a likely artefact due to their snow being artificial and not natural, as noted above. They made their snowflake by manually assembling crystals of about that size.

Review: Figure 4: Why are there so few data points? A line density plot or a bar plot with a smaller bin width might improve clarity. Additionally, the unit on the y axis is missing.

Response: There are only about a dozen points on the fragment size distribution because these are averages over a subset of the collisions and we opted for a few bins that are wide, so as to avoid noise from the limited number of collisions in each size range.

Figure 4 is now modified to include error-bars.

Review: Line 250-251: How was the 13% fall speed error calculated? 120 fps resolution suggests it may be higher.

Response: The camera inside the chamber is used to measure the error. As the snow particle falls over a vertical distance of 10 cm, its time is measured by counting the number of frames. But only about 12 frames happen in that time. The error in time measurement is 1/120 sec or 13%, where the time taken to fall over 10 cm is about 0.07 sec assuming an average fallspeed of 0.7 m/s.

Review: Line 252: Gautam et al. (2024) report a 30% CKE error, while this study reports 46%. How was this estimated?

Response: 0.2 mm (minimum size detectable) divided by 2.5 mm (average size of incident snowflake) gives a 8% error in the size. But accounting for random orientation, we it seems the error goes up to 13%. A 13% error in size implies a 20% error in the mass (the exponent of b = 1.5 implies that the size error is multiplied by this to get the mass error).

Review: Figure 5: Why so few measurement points? Improve this figure by showing raw measurements of N vs. CKE. Also, why do some collisions have identical CKE values, given the variability in fall speed and particle mass?

Response:

There was a problem with the plot and it is now replaced with all 100 points displayed.

Review: Line 265: You cannot state that the rime fraction is estimated from the "snow sampled" if it is derived from simulations.

Response: We have re-phrased this with extra explanation (lines 332-333). We meant that the estimation was done by modeling, not by observations.

Review: Line 283: Was the rime fraction constant in the simulation? Please provide relevant results to clarify this.

Response: You mean in the simulation of ACAPEX? No, in that simulation the rime fraction was explicitly predicted.

That line is about implementation of the scheme in an atmospheric model by others in the community.

This has been clarified and a new implementation section has been added (Sec. 7), to which that statement has been transferred (line 455).

Review: Line 296-297: Better in prediction compared to what? Compared to the measurements used initially for the fit?

Response: It performs better compared to the observations used to construct it.

Review: Section 6: What type of simulation is conducted using the AC model? What is the cloud numerical setup employed? Please provide further details.

Response: More details about the numerical setup have been added (lines 235-245).

Review: Line 346: It is unfortunate that the effect of rime fraction on the number of fragments cannot be studied based on your measurements. There are some instruments like the MASC probe that could provide information about the rime fraction of ice particles.

Response: That is interesting that the MASC probe can reveal rime fraction. Perhaps future experiments can use this.

Review: Line 371-373: Please exercise caution when referring to the detection of fragments and revise this sentence accordingly.

Response: Yes, it has been revised in a more cautious way (line 538).

Review: Figure 6, 7 and 8: Could you clarify how these plots are obtained from the simulations?

Response: First, the standard case is one collision of a snowflake of idealized size (5 mm), rime fraction (0.2) and CKE (1E-7 J). These are the three input quantities. The output is the fragment number per collision (N) predicted by the new version of the scheme from the present paper (Table 1). Then we do a sensitivity test by varying each input quantity in isolation over a prescribed wide range above and below the standard case value. Thus CKE is varied from 1E-8 to 1E-5 J for Figure 6 (red line) with size and rime fraction constant. And similarly for variation of size with CKE and rime fraction held constant for Figure 7 (red lines). On each plot (Figs 6, 7 and 8), at N = 12 is one point on the red line for the standard case, while there is a sensitivity test.

These three plots are simply sensitivity tests with respect to the three input quantities for the idealized simulation of a collision of one snowflake with a graupel particle.

More lucid explanation has been added to the text (lines 378-384).

Review: Conclusion: Update this section based on the previous comments.

Response: Done. We now mention the threshold for detection (0.2 mm) at lines 524 and 538.

Review: Minor comments:

- Only a few studies about fragmentation in ice-ice collision have been made, please cite all of them (add Griggs et al., 1986, Takahashi et al., 1994, Grzegorczyk et al., 2023) in the introduction.

Response: This has been done with a new paragraph in the introduction (lines 52-68).

- The quality of all figures needs to be improved (increase the dpi please).

Response: We have tried to improve these.

- **Line 69-71:** Cite references that support that.

Response: Re-written with references (lines 83-86).

- **Line 74:** Mention that Takahashi et al. (1995) experiment is also used by Phillips et al. (2017) formulation.

Response: Done (line 94).

- **Line 180-183:** Please precise which parameter needs the rime fraction.

Response: Done (line 228).

- **Figure 3:** The legend in the upper right corner is empty. The resolution of the figure might be improved.

Response: Done.

- **Suggestion:** plotting x and y axes on a log scale instead of using Log N for clarity.

Response: Done for all figures.