Review of *Secondary ice production during the break-up of freezing water drops on impact with ice particles*
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James, Phillips, and Connolly present experimental results on a secondary ice production (SIP) process involving liquid drop-ice crystal collisions. It is nice to see additional, and especially quantitative, laboratory results on SIP, and I support publication of the article. I feel, however, that several points should be elaborated and that some reorganization of sections would help with clarity.

Major Comments

- Given that a condensed version of the theoretical work in Phillips et al. 2018 was presented, I expected there to be some comparison of the observational results with this model. For example, can you calculate Ns as a function of temperature from the Phillips et al. model and overlay it on Figure 5? Can you say anything about the validity / assumptions of the model on the basis of these experiments?

- I felt that the mechanistic discussion at the start of Section 5 would have been helpful prior to Sections 4.1 and 4.2, so that I had a better sense of what was physically happening in the experiments in Figs 2 and 3.

- I appreciate that the limitation of the glass slide is acknowledged and secondary drop production in other setups is discussed (lines 159-171). But does the presence of the glass slide also mean that the mechanism shown in Figure 6 may need to be modified in the real atmosphere? In particular, given that the secondary drops and frozen fraction have only been quantified in the retraction phase, should an equivalent retraction phase exist between two curved surfaces in relative motion?

- Section 5.1 read like introductory material, as it did not include discussion of any of the study's findings. It is valuable information, but I would integrate it into the introduction as general motivation to study SIP.

- In place of Section 5.1, I think a new (small) "Atmospheric Implications" section or additional sentences throughout the Discussion / Conclusion would be helpful to discuss the atmospheric conditions under which the experiments are representative, i.e. In what regions / synoptic conditions / cloud systems, would ice particles of diameter 6 mm and raindrop of diameter 5 mm coexist? For what range of ice particle and raindrop sizes, does 5.2 m s-1 represent a realistic relative terminal velocity? Would it be possible to use the We and Re characterizing these experiments to identify regimes in in-situ data for which this mode 2 fragmentation could occur? etc.

Minor Comments

Line 15 – "where *subzero* temperatures"

Lines 17-18 – "typically *fall between* 1 x 10-5 L-1 and 1 L-1 at temperatures T~-10 deg C" (looking at Fig. 1-10 from Kanji et al. 2017)

Line 24 – "NWP models underestimate the concentrations of ice particles" – It would be nice to include an order-of-magnitude range for these underestimates.

Line 28 – "supercooled water drop diameters are < 13 um *and* > 24 um" In Hallett and Mossop 1974, both droplet sizes should coexist.

Line 31 – Along with the temperature range for frozen droplet shattering, it would be worthwhile to include a droplet size range as well, since droplet size will be discussed later as a parameter of the current experiments (e.g. 280-350 um in Keinert et al. 2020)

Line 34 – I would suggest to rephrase as “the attention *of laboratory studies has overwhelmingly* focused on the rime-splintering…”, since a growing body of recent work has look at breakup parameterizations (e.g. Hoarau et al. 2018, Sotiropoulou et al. 2020, Sotiropoulou et al. 2021, Dedekind et al. 2021, etc.)

Line 52 – If you note that ‘Mode 2’ of frozen droplet fragmentation is studied, it would be helpful to know what ‘Mode 1’ is also.

Line 64 - “DE_{crit}” (not D_{crit}). Also you have not yet defined the freezing stages when you mention “stage 1 of freezing” here and again in Line 68.

Line 70 – “Finally, Phillips et al. 2018 hypothesised that $\Phi(T) = \min[4f(t),1]$ such that $\Phi = 0.5$ at -10 C” Stated like this, it sounds rather ad hoc. Perhaps an additional sentence can clarify where this form comes from or how it is constrained.

Line 94 – I do not know how important it is, but it was not clear to me what the “x-y translator (modified 3D printer)” was in the setup.

Line 99 – Surface tension of the liquid water is presented as γ in line 61 and σ here; viscosity is presented as λ here and μ in the equation for the Reynolds number.

Lines 113-114 – It was not clear to me why the filament-like structures do not form when the colliding droplet spreads on the glass slide at room temperature. Is there a physical explanation for why this only occurs at colder temperatures?

Section 4.1 – Somewhere in this section or perhaps in the preceding Section 3, it would be helpful to have already referred to Table A1, so that it is clear from how many experiments the results come, e.g. only two glass slide collisions total were performed at 23 and -5 C?

Lines 133-134 – Was anything learned from the partial versus direct collisions? Does one or the other produce more secondary drops or higher frozen fraction? I guess there may be no robust difference, given the difficulty of performing these direct collisions. Also was Figure 5 produced from all data (both partial and direct) in Table A1? This should be specified in the caption.

Lines 136-138 – “The smaller secondary drops observed at impact … were not observed.” This seems like it may be an important limitation. Is there the possibility to improve RPicam resolution in future work? This should be mentioned in the conclusions / future work if so.

Line 148 – “Surface tension and viscosity forces were considered negligible during the spreading phase of the drop” I am confused by this statement. Where / in which calculations are these forces being considered negligible?

Lines 150-151 – I have not seen the prompt-type / corona-type splash terminology before; I would define these terms more completely from the citations in these lines.

Figure 7 – Are the top versus bottom panels also with and without the polarising filter?

Line 176 – "T less than equal to *-11 deg C*"

Lines 180-187 – I find the arguments here difficult to follow. The takeaway is that temperature dependence of frozen fraction is caused by a liquid-ice interaction time scale? Could the authors reword somehow for clarity?

Line 189 – "We believe that the freezing fraction of the secondary drops is independent of the number of drops formed." Is there a reason for this belief? I would expect temperature dependence to dominate also, but I could also imagine that when a fixed fraction of the colliding droplet mass produces secondary drops, and more such secondary drops form, they are smaller and freeze faster..?

Citations

Dedekind et al. 2021Sensitivity of precipitation formation to secondary ice production in winter orographic mixed-phase clouds. Atmos. Chem. Phys. Disc.

Hoarau et al. 2018 A representation of the collisional ice break-up process in the two-moment microphysics LIMA v1.0 scheme of Meso-NH. Geosci. Model Dev. **11** 4269-4289

Kanji et al. 2017 – Cited in manuscript

Keinert et al. 2020 Secondary ice production upon freezing of freely falling drizzle droplets. J. Atm. Sci. **77**(8) 2959-2967

Korolev and Leisner 2020 – Cited in manuscript

Sotiropoulou et al. 2020 The impact of secondary ice production on Arctic stratocumulus. Atmos. Chem. Phys. **20** 1301-1316

Sotiropoulou et al. 2021 Ice multiplication from ice-ice collisions in the high Arctic: sensitivity to ice habit, rimed fraction, ice type, and uncertainties in the numerical description of the process. Atmos. Chem. Phys. **21** 9741-9760