Direct evidence for secondary ice formation at around -15°C in mixed-phase clouds

General comments

The manuscript shows results of cold stage tests from samples taken at Jungfraujoch with the aim of illustrating secondary ice formation at an "individual hydrometeor level". These analyses could yield quantitative estimates of ice crystal enhancement, but the data are too few to make a publication-worthy conclusion in my opinion. The authors note that Hoffer and Braham attempted a similar per-particle analysis more than 50 years ago. Their sample size was 300 snow pellets, 150% that presented here, and they note in their abstract that "a firm statement could not be made as the number of observations is limited." The burden is on the authors to explain why it is sufficient to show ground-based data from only 10 days.

Thereafter, the introduction needs to be expanded in my opinion. Right now, there is not a thorough discussion of existing literature. Approximate values and measurement techniques for INPs and IRs in mixed-phase clouds should be mentioned, in particular the abundance of measurements from Jungfraujoch (e.g. with the Ice Selective Inlet (Kupiszewski et al. 2015), the Ice Counterflow Virtual Impactor (Mertes et al. 2007), and the Horizontal Ice Nucleation Chamber (Lacher et al. 2017) as discussed in *Cziczo et al.* Measurements of Ice Nucleating Particles and Ice Residuals).

The analyses also need to be fleshed out. A more complete picture of the meteorology could be given by including the range and variability of air temperatures and wind velocities during the sampling periods. If photos of all the crystals were taken with a high-quality camera, some of these should be shown. Is there a more rigorous means of classifying the crystals than what is "considered to be planar, branched"? If the size of the crystals was measured with ImageJ, could some of these statistics also be presented? In Section 2.3, there is also a mention of rime analyses with a second cold plate, but it was not clear to me how this fit in. The results shown in Figures 2 and 3 are from pristine, unrimed dendrites, right?

With additional data and stronger analysis, more could be gleaned from this study. If the cold plate measurements are subject to any contamination, then 12.6% of the droplets refreezing is actually an overestimation. And a limited crystal geometry has been used to define secondary ice; at -15°C and lower supersaturations, other geometries are possible. So perhaps the multiplication factor of 8 is more of a lower bound. Quantitative estimates of this factor are needed for models, and field measurements at the hydrometeor level, rather than the bulk cloud level, are a new, if labor intensive, technique.

Specific comments

Page 1, Lines 18-20: The conclusion that "secondary ice can be observed at temperatures around -15°C" is not an especially compelling one, given that many previous studies have already shown this. Is there a hypothesized mechanism? Or was observed multiplication factor higher under certain conditions?

Page 1, Line 23 – "*These freezing pathways*" as there can be contact or deposition or immersion freezing.

Page 1, Line 26 – A few additional, more recent observations might be cited. For example, Lasher-Trapp et al. *JAS* [2016], Ladino et al. *GRL* [2017], and Jackson et al. *ACPD* [2018].

Page 2, Line 19 – For completeness, you could mention the correction of such shattering artifacts in more recent data by inter-arrival time algorithms and K-tip probes.

Page 2, Line 22 – I would define rime when you first discuss rime splintering above in Lines 4-5.

Section 2.2 and Figure 2 – The authors have taken a number of concerns about cold-stage measurements into consideration with their setup, which I appreciate. I would cite Tobo 2016 for the use of a semi-sold, hydrophobic substrate, and you might mention the possibility that INP settle out or aggregate within your large-volume droplet [e.g., Emerstic et al. 2015 *ACP*]. I am still concerned, however, that 20% of the control droplets have frozen by -25°C, almost 10°C above the threshold temperature for homogeneous freezing. Could the estimated enhancement factor be adjusted to account for these "false positives"?

Page 4, Line 1 – I would add a sentence that summarizes what this 'global classification scheme' is because it is not so widely used, as far as I know.

Page 4, Line 27 – Is there a reason that the "custom-built cold stage" used for single crystal analysis was not also used for the rime?

Page 4, Lines 28-29 – I am not sure what is meant by "droplets of molten rime". You are melting the aggregation of frozen droplets and then refreezing them upon a cold plate? Or somehow separating the droplets within a single aggregate? Please clarify here.

Page 5, Lines 19-21 – Measurement uncertainty and / or variability for this estimate needs to be included.

Page 5, Line 32-Page 6, Line 1 – The mention of INP from soils does not seem particularly relevant to me, as those will not be the INP source at Jungfraujoch.

Page 6, Lines 5-14 – Blowing snow is a very important consideration here, given several existing studies on this mechanism at Jungfraujoch. You are considering pristine dendrites here, right? Otherwise, there is the potential for riming growth, not just depositional growth.

Table 1 – For periods that last as much as 14 hours, it would be more rigorous to give mean and standard deviation for values like air temperature / wind velocity since a single value will not be characteristic. Are there are any vertical wind measurements?