New type of evidence for secondary ice formation at around -15°C in mixed-phase clouds

General comments

The authors are presenting a method to assess the likelihood of secondary ice production on a per-hydrometeor basis. They have been thorough in setting up the new experimental apparatus and have used it over a month at Jungfraujoch. The authors note that the setup is "field deployable", so that it could be used also in field campaigns. My specific comments have mostly been addressed, but given that the novelty is in the methodology, I wonder if Aerosol Measurement Techniques would not be the better fit for this manuscript. The scientific conclusions still seem limited to me. For example, it is a bit extreme to state that "no conclusion regarding the process of secondary ice formation can be drawn from our observation." Could not the meteorological data be used at least to speculate on more and less likely secondary mechanisms? Is the enhancement factor higher if the cloud base temperatures or horizontal winds are stronger? Or if the winds come from one direction or another?

I also want to say that I still have reservations about the ability of this method to estimate ice enhancement factors for mixed-phase clouds in general. Were all (or almost all) dendritic ice crystals retained from the flow across the black aluminum plate during sampling periods? If so, it is impressive that there were only 229 such crystals over 10 days. If not, representativeness is still a concern. The authors state that "if we had the crystals from a small fraction of a cloud volume and would extrapolate our findings to a much larger volume in which primary and secondary crystals are very heterogeneously distributed, we would face a problem." But as I understand it, this is what is being done. It is stated very generally in their responses that they "can draw a conclusion regarding secondary ice formation within mixed-phase clouds".

Let us set aside this concern because it is still interesting to look at individual ice crystals. Some caution needs to be taken in any discussion of ice crystal habit and ice formation: ice crystal habit encodes information about *growth temperature* not *formation temperature*. Ice crystallization is a kinetic process and dependent on the crystal's temperature-supersaturation history. It is possible to nucleate at a cold temperature and then enter a warmer temperature zone – by sedimentation, advection, etc. – and do most of the growth there. It seems unlikely to me that homogeneously nucleated ice crystals move into a zone of -15°C before significant growth has occurred, so that the method should generally not have false positives in this way. But I do think that this kinetic nature of ice crystallization warrants mention within the manuscript.

I appreciate that photographs of ice crystals have been added. Those in the supplemental material, and in fact all of the text and imagery in the supplement, could be added to the main manuscript in my opinion. This is again given the emphasis on a new technique. Finally, given that "closer inspection of the enlarged photographs" indicated that some were not planar or branched, it would be nice to have a more rigorous means of classification for future studies. Would there be a way to use the ImageJ software used for sizing to also do some kind of "shape processing"? If the authors have ideas for rigorous classification algorithms, they could mention these within the conclusion section. I have only a few other specific comments.

Specific comments

Page 2, Line 13-14 – This point is slightly confusing (because secondary ice is associated with warmer temperatures and here you are mentioning colder temperatures). I would rewrite as *Because they all (n = 301) re-froze only at temperatures substantially lower than the measured cloud top temperature, the authors presumed them to be of secondary origin.*

Page 3, Line 19-24 – In my opinion, it makes more sense to list the motivations to focus on -15°C in a different order. This is a detail, but the first motivation is really the distinctive ice habit at this temperature. Thereafter, the crystals have lower density and terminal velocity, and the observations of Westbrook and Illingworth (2031) and the higher ice-ice collisional efficiency seem reasonable.

Page 3, Line 30 – "nucleated" not "catalysed"

Section 2.2 – My former concern about INP coagulation and sedimentation within the larger volume droplet was not addressed. It is favorable that "*the procedure takes ~15 minutes*", but there is still sufficient time for a non-negligible drop in particle surface area (see Emersic et al. 2015 ACP Figure 8). This caveat needs to be mentioned.

Page 4, Line 18-19 – How are you able to "*exclude hoar frost particles"*?

Page 4, Line 25 – How exactly were the "*images … later analysed more exactly"*? Visually?

Table 1 – Thank you adding the standard deviations.