Replies to the Reviewer 1 comments
Second Review of MS acp-2019-611, revised version, Nov. 22, 2019

This is an important paper, taking advantage of advances in imaging cloud ice particles from airplanes to deduce whatever may be deducible about secondary ice production, an important and long-standing problem in cloud physics. It is acceptable for publication, I think, in its present form, but I will make several suggestions for minor revisions in the presentation.
Reply: Authors appreciate Reviewer's careful reading, valuable comments and time spent to evaluate this manuscript.

Line 121 extent instead of extend;
Reply: Corrected as per referee comment

Lines 131-132 -- the last two sentences of this paragraph do not communicate much to a reader at this point, without a physical explanation of how the HM process is expected to operate and what the alternatives are.
Reply: Unfortunately, in the cloud physics community there is no consensus regarding the physical process responsible for the HM process. As mentioned in the introduction (lines 67-75) there were several works aiming to understand the physical mechanism of the HM process (Macklin, 1960; Choularton et al., 1978, 1980; Emersic and Connolly, 2017). However, the explanation of this phenomenon is still under debate. In order to address the Reviewer's comment the last two sentences were modified as: **"As can be seen, the identification of SIP gravitates towards the HM-process, whereas mechanisms such as activation of INP in transient supersaturation around freezing drops, ice fragmentation due to thermal shock or sublimation were not even considered. In this regard the question that arises is, could these observations reflect an actual occurrence of different types of SIP?"**

Section 3.1 I don't agree with the use of the word "assumptions" here. The two on lines 191-193 are more properly called approximations. I think the argument that most of the small, hexagonal ice xtals derive from "secondary ice" is very strong, but we don't need to "assume" it. And number 2 likewise is not an "assumption" in any sense of the word -- it's an approach used in the analysis.
Reply: The term "assumption" was replaced by "approximation" as per Reviewer's comment.

Then on p. 6, the use of the word "characteristic" is, to me, quite inappropriate. I would favor "typical" or "approximate" sizes or residence times. I do understand what the author means here, but upon encountering the word characteristic so many times, at first I couldn't follow the meaning.
Reply: The term "characteristic" was replaced by "typical" as per Reviewer's comment.

Line 291 -- using counting rate instead of concentration takes the meaning out of the measurement. It is explained that the sample volume is quite uncertain, but the concentration is of course what is important, so it should be identified here in the text and it should be noted that the concentration scale is on the figure. Personally, I would have put the concentration scale on the left and the counting scale on the right in the figures. The explanation of the sample volume problem is good.

Reply: The concentration and counting rate scales in Figs. 5, 8 and 13 were reversed as suggested by the Reviewer.

Lines 534-538 The argument about small particles having shorter residence times than larger ones doesn't make any sense at all to me, nor is it particularly important to the argument here, I think. "Residence time" must depend critically upon updraft, downdraft, turbulence, fall speed, and the various possible "sinks." Small ice in mixed or supercooled cloud has limited "residence time" mainly because it grows past being small, obviously.
Reply: I believe there is some misinterpretation of the text. The original statement in Lines 534-538 said that **small** SIP particles will stay longer in the environment of their origin, whereas **large** SIP particles have shorter residence time in clouds. This is consistent with the Reviewer's comment. The statements about the residence times are important for cloud simulations.

Line 543 "characteristic" again. Not a good word, for me. But the small hexagonal plates are a wonderful observation. It's too bad that there aren't any comparable small needles around -5C!, but maybe they would be too thin for the instrument to detect.
Reply: The term "characteristic" was replaced by "typical" following the Reviewer's comment.

The thin plates and their interpretation are for me a rather wonderful observation, and surely they grow around -1 or -2C, but the original secondary ice "must" have descended from above??
Reply: Thank your for the comment. Yes, small SIP fragments descending from above (e.g. -3C to -4C) due to turbulent diffusion and re-growing into plates at -1C to -2C is a possible scenario.

Line 798 --Now it's 10? I thought it was more like 20, before.
Reply: In the text in Section 5 the thickness of plates was assessed as 10μm to 20μm. Since the initial size of the fragment should be smaller than the thickness of the plate, the *smallest* size of the SIP fragments is estimated as 10μm or less. In order to mitigate ambiguity in the interpretation of the text in Line 789, it was modified as: "**The smallest size of the splinters generated during SIP were estimated at 10□m or less.**"

I think that temperatures should be included in the figure captions for all of the multi-image figures.
Reply: Following the Reviewer's comment the temperature was added to multi-image figures where it was missed (Figs.18,20,21,24)

This paper generates suggestions in me, for field and laboratory approaches testing some of the rather speculative (but not unreasonable) interpretations of the ice data.
Reply: The authors absolutely agree with this comment.

The many other suggested secondary ice mechanisms are mentioned in the introduction and then each is again discussed at some length near the end. This is a very long paper, and for my taste, I would have left out the re-cap of every secondary ice thought, at the end. Perhaps in favor of more details about what the difference might be between the recycling-water-drops hypothesis and the HM process ("rime shattering," though the actual mechanism behind the lab results seems to me not demonstrated).

Reply: The authors considers that the section at the end, which reiterates the considerations of different SIP mechanisms, is important here. It shows limitations of the in-situ observations in identifying different SIP mechanisms from in-situ observation and prepare a background for the statement about importance of lab SIP studies. At the same time, we cannot expand the discussion of the rime-shattering mechanism, without making it too speculative. More information is required to make the next step.

I recommend acceptance. This is an important work.
Reply: Authors appreciate the Reviewer's evaluation of this work.

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07 December 2019

RE: 2nd revision of ACP-2019-611 paper

Dear Martina,

Attached please find the 2nd revised version of the manuscript titled: "A new look at the environmental conditions favorable to secondary ice production" by A. Korolev, I. Heckman, M. Wolde, A.S. Ackerman, A.M. Fridlind, L. Ladino, P. Lawson, J. Milbrandt, E. Williams submitted for evaluation for publication in the Atmospheric Chemistry and Physics along with the point-by-point replies to the Reviewers' comments.

We are looking forward seeing your decision.

Sincerely,

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