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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A new look at the environmental conditions favorable to secondary ice production" *by* Alexei Korolev et al.

Anonymous Referee #3

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Summary: This manuscript addresses possible mechanisms for generating secondary ice in two different case studies: a tropical, maritime MCS collected during the High Ice Water Content (HIWC) field campaign, and a midlatitude, continental frontal cloud system collected during the Buffalo Area Icing and Radar Study 2/Weather Radar Validation Experiment (BAIRS 2/WERVEX). Both cases used observations collected from the National Research Council (NRC) Convar580, with nearly the same instrumentation, but the main focus is upon the CPI image analysis. A theoretical section in the paper describes the approach, to derive a maximum ice particle size that would be indicative of those ice particles occupying similar thermodynamic characteristics as where they originated. Using that maximum size, the authors examine the characteristics surrounding large number concentrations of particles less than or equal to that

maximum size in order to identify possible secondary ice production mechanisms responsible for those large numbers. The authors conclude that at times rime-splintering may be active, at others freezing-raindrop shattering may be active, and explain why other mechanisms can be ruled out. They also hypothesize that in some instances raindrop recycling from below the melting level upward might be important to the shattering mechanism.

I found the manuscript difficult to follow, for several reasons. First, it suffers from an "identity crisis", in that the introductory section leaves the reader confused about what the main goal of the paper is. (I expand on this point below.) Then, the repetitive nature of explaining each SIP mechanism, and then addressing them separately for each case, adds up to a lot of text, and back-and-forth jumping by the reader to understand why a conclusion is different in one section versus another. And finally, the conclusions section is rather lack-luster, essentially making points that we have known for quite a while, and can be found in numerous other manuscripts.

I believe that this manuscript presents a unique approach that is not adequately emphasized in the text, in the novel use of the CPI data to try and address multiple SIP mechanisms. No other study to my knowledge has attempted to do this with the theoretical basis constructed here. I do have some concerns about this approach that I'll discuss below, but I believe for this paper to have the impact that it could, the manuscript should be rewritten **to emphasize its unique contribution** (including the title), rather than **to try and make hard conclusions about which mechanism was likely most important** (I agree with the concluding section that the observations are too limited in this regard). As written, the manuscript attempts to draw hard conclusions (in places), but then undermines its own message by discussing how limited the observations are in determining the active mechanisms (lines 838-845, and 897-903).

Major comments: 1. Section 1 is very long. Because of the recent AMS Monograph review article by Field et al. (2017), this much explanation does not seem necessary regarding the different mechanisms. I'm not opposed to having this section in there, but it reads like a review article, leaving the reader to wonder what this study is actually addressing. 2. Unclear focus. Following comment #1, at the end of the Introduction, a question is posed: “The question that arises is, could these observations reflect an actual occurrence of different types of SIP?” Is this then the focus of the paper? The 2nd section states the objectives as “Based on the results obtained the authors attempt to revisit the role of different SIP mechanisms and identify conditions favorable for SIP.” Later in the Conclusions section however, it is stated that “The obtained results are expected to contribute in our understanding of SIP, and they may be used by cloud modeling studies for evaluation of secondary ice production in the numerical simulations of clouds.” So was this the real objective, to essentially create some cases for future numerical modeling? The focus is extremely important here, because it would allow the authors to address one of these completely. In the current state, the manuscript falls short of meeting any of these objectives, in that the results are not novel, nor is the data set sufficiently described to provide a good target case for future numerical modeling. 3. Without more information, the observations as presented are insufficient to make a good case to test with numerical modeling. It is clearly stated that both of these cases were seeded with ice from overlaying cloud, but we are not told what temperatures the overlying clouds are, nor are the values of measured INP active at these temperatures given. How can a model get this case correct if such “initial conditions” are not provided? 4. Abstract. The final lines 27-31 are inconsistent with the discussion in the conclusions section. Please revise. 5. Line 345: In specifying the tau values, a uz of 1- 4 m/s was specified. But earlier in the manuscript, strong updrafts were noted, up to 15-20 m/s in the MCS (Line 254), and Fig. 8 shows the analysis was applied in 10-15 m/s updrafts. So how can these values of tau be applicable in that case? My concern is that in stronger updrafts, the small ice particles would be leaving their place of origin much more quickly than assumed in the paper. That would make the Lmax derived in the next section MUCH smaller. (Again, having some radar pictures of the vertical velocity structures would be helpful here and might have helped me understand why you used these values in deriving the taus.) 6. The arguments presented in sections

3.3 and 3.4 appear logical, but because the entire study hinges on this derived Lmax, a more thorough discussion of uncertainty in its estimate, and the implications of its uncertainty, is required. 7. I have two large concerns with the CPI analysis. First, I was happy to find the analysis of shattering on the CPI in the AppendixâĂŤI've always wondered about that. But that information does not seem to be discussed much in the main manuscript. I think it should be, or at least should be discussed a little more in the main text, how those fragments were eliminated compared to the small secondary ice. Second, I am not confident in the CPI-derived estimates of ice particle number concentration based on a scaling with water droplets from the 2DSâĂŤthey would not have the same shattering effect as the ice crystals, and it is not clear that the CPI cameras would necessarily trigger at the same rate for water droplets versus ice. Why not use some times when the cloud is completely glaciated, and compare the CPI image rate with the CIP numbers at those times? While I don't feel that the authors have completely misidentified SIP episodes, their magnitude is highly questionable with the current approach. 8. Sections 7 and 8 could be condensed into more focused text, which would greatly help the reader comprehend the main points. 9. Lines 769-770: here is where a discussion of shattering on the CPI, and the removal of those particles and its uncertainty, is extremely important (and in the following sections as well). 10. Melting Layer Hypothesis: Especially in the maritime MCS, why can't the large supercooled drops just be growing in updraftsâĂŤwhy would they have to be recycled melted particles to be important? And if they were heavy enough to fall and melt, what mechanism would bring them back into an updraft? This section seems to really “reach” past what one can identify with your observations. Why not just let the data speak for themselves? Dual-polarization data would really be needed to pin this down.

Minor Comments: 1. Line 133: I think you mean “overestimated” rather than “underestimated”. 2. Lines 170-175: This assumes that air at the cloud edge containing sublimating crystals in incapable of being reintroduced into the core of the cloud; numerous studies have shown that entrainment in cumuliform clouds can bring outside air into the interior of the cloud (but no studies have looked at the effects on ice, to my knowledge.) 3. Lines 237-243: “One of the important finding of this study is that melting layers in many cases work as a source of large liquid drops, which then ascended to a supercooled environment via convective or turbulent updrafts. After impaction freezing by preexisting ice, the drops may shatter and initiate a chain reaction of secondary ice particle production.” Why is this stated here in the “objectives and data sets” section? It’s written like an abstract that lists the results. It’s very confusing here- please move this to the abstract or the conclusions section. 4. Lines 252-254: some evidence of this glaciation would be good. Can’t this be shown with observations and radar data? This might help the reader understand the nature of the “seeding” of these clouds, as referenced in Line 237. 5. Line 271: I’m pretty sure you didn’t have an X-band radar mounted on the Convair. Please fix text. 6. Equation 6 needs a reference, or some explanation. 7. Lines 455-461: I am not convinced that one can tell from the CPI images if droplets spread out sufficiently to prevent rime-splintering from occurring. Comparing it to the roughness of the rods used in the laboratory (in the next paragraph) seems unreasonable.

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