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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of Secondary Ice Production on Arctic Stratocumulus" *by* Georgia Sotiropoulou et al.

Anonymous Referee #2

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The manuscript studies the sensitivity of ice particle number concentration on different assumptions about secondary ice production in Arctic mixed phase clouds. The topic itself is interesting and merits publication, but selection of methods and the observational case requires more discussion. There is some discussion of the weaknesses of selected modelling approach in Discussion section, but the reason for the employment of certain modelling tools should be made clear already in the model description. Overall, even though the comparison to measured data and selected methods have issues, the manuscript provides some evidence for the role of secondary ice production and gives directions for further studies. Thus I recommend publication after corrections and additional discussion.

1) What is the role of measurement conditions? It is said that the wind is from the West

and measurements are performed both over the open water and ice. Droplet concentration seems similar in both, but there is no discussion how ice particle concentration differs and where the presented values are measured. Is there any potential for the surface to be the source if ice hydrometeors?

2) LPM: I have some problems in understanding what the LPM model employed is actually simulating. Does it solve the hydrometeor condensational growth assuming three size classes both for liquid and ice, or is it somehow parameterized how particles grow from some size range to another based on characteristic time parcel is spending in a single updraft. I'm actually surprised that there is no spectral size resolving model employed. Such models should be available and numerically efficient enough to be used in the presented application. In line 139 it is stated "The LPM allows a detailed description of the formation, growth and evolution of cloud droplets and ice particles as they interact with each other". I disagree with this, I would not call three bins detailed what comes to representation of cloud and ice particle size distributions. Thus also coagulation rate and secondary ice production are only approximate, although probably accurate enough to provide first estimates and to be used in this paper.

3) MIMICA: Line 145 or later in section 3.1: Maybe you should state explicitly the reason why SIP processes are not directly implemented into MIMICA.

4) How does the SIP enhancement work in a case when the ice particle concentration at cloud base in MIMICA is higher than predescriped IN concentration? Does it still enhance the concentration? I assume such conditions to occur frequently in modelled boundary layer cloud.

5) Line 283: "The mean observed INP concentration is 0.006 L-1 and never exceeds 0.05 L-1". From where does these numbers come from? The conditions are really warm for heterogeneous ice nucleation, with modelled values at minimum -6.5 degrees and measured even warmer. What aerosol particles are active in such a warm temperature.

6) Within MIMICA it would be possible to track temperature dependent IN concentration. How would this more realistic approach change the simulations? In comparison to observations it would have been interesting to see if the spread in modelled data is as wide as in observations. When I look at modelled data, I am really surprised how small standard deviation there is in the output. Enhancement should depend quite strongly on the updraft at the cloud base based on Figures 4, 6 and 8.

7) Line 463: “A main challenge in parameterizing BR is that a correct spectral representation of the ice crystals is required, which is more feasible in bin microphysics schemes”. This is true, and the same limitations holds for all cases when temperature dependent ice nucleation or secondary ice production is included. If the number concentration is tuned to be correct, the size distribution and total mass is probably wrong due to given shape for size distribution.

8) Jones et al., 2018 is not accepted for publication, so it should not be cited.

9) Schwarzenboeck et al., 2009 title is “Indications for stellar-crystal fragmentation in Arctic clouds”

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