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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Secondary ice production in summer clouds over the Antarctic coast: an underappreciated process in atmospheric models" *by* Georgia Sotiropoulou et al.

Anonymous Referee #1

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General comments: The study simulates an Antarctic cloud over the coastal Antarctic and the Weddell Sea in November-December 2015. Secondary ice production from the break-up of collisions between ice particles is added to Weather and Research Forecasting model. The model simulated results are compared to extensive measurements from airborne and ground-based instruments. Their results indicated break-up of collisions between ice particles could account for enhanced ice number in the pristine Antarctic atmosphere, and these results are insensitive to uncertainties in primary ice production. I think the study will be publishable after the following comments are

addressed.

specific comments: 1. My main concern is the experimental design. The control simulation is stated as using the default Morrison scheme (Line 204: "Additionally to the control (CNTRL) simulation, which corresponds to the default set-up of M05"). The Morrison scheme is described in the literature that includes different types of secondary ice production in it, i.e. the rime splintering (H-M) during snow accretion cloud droplets and snow accretion raindrops. Therefore, either I have misinterpreted and some additional description of the control model configuration is needed, or I think we need an additional set of control runs that have no secondary ice production processes included Until there is a clean experiment with no secondary ice production processes it is difficult to interpret the statements about the impact of secondary ice production.

2. My first concern is about "separation size between ice and snow" The authors mentioned in the manuscript Line 215: "Note that since the separation size between ice and snow in the M05 scheme is 125 μm, collisions that include cloud ice do not result in any multiplication in FRAG1siz." The reader may be confused, does Morrison scheme really has a size separation between cloud ice and snow? Does this mean the model does not have cloud ice larger than 125 μm, and no snow particles with a diameter smaller than 125 μm? Morrison scheme is documented in the literature that includes a threshold size (125 μm) for the cloud ice autoconversion process. But this does not mean a size separation between could ice and snow. Based on this, when comparing simulated snow and graupel with observation, does modeled snow and graupel only considering particles larger than 80 μm? Or only modeled cloud ice has this threshold in size.

3. Related to the second concern, the third concern is related to the comparison between observation and model results. The authors mentioned that "consistency with M05, the threshold size separating measured cloud ice from snow is set to 125 μm" Usually, when comparing model ice with observation, we added different types of modeled solid particles together, then using the total mass and total number to compare

with the observed IWC and ice number concentration. Because it is hard to tell cloud ice from snow in observation data. I suggested using a similar method when compare the modeled ice number with observation.

4. The fourth concern is related to parameterization from Phillips et al. (2007) In the sensitivity test, Line 255, “These simulations are referred to as PHIL0.2, PHIL0.3 and PHIL0.4 in the text, where the number indicates the assumed values of Ψ”. In the bulk microphysical scheme, snow is referred to as dry snow, with smaller density (prescribed and fixed in the scheme), graupel is rimed ice with larger density. Setting the same rimed fraction for different collisions is not consistent with the assumptions in the microphysics scheme. I suggested the uses different rimed fraction for snow-snow and graupel-graupel collision. The rimed fraction could be changed in the sensitivity test, but in one simulation, the rimed fraction for the collision between snow-snow should not larger than rimed fraction for the collision between graupel-graupel.

5. The last one is related to radiation Does cloud microphysical properties couple with radiation transform code? It is interesting to see the model has a larger ice number and IWC, after the implementation of the secondary ice process, but the longwave radiation does not change accordingly. How about the effective radius of the ice particle? Does it change after the model has secondary ice production in it?

technical corrections 1. Line 59 “Lachlan-Cope et al., 2016; Wexx et al.,”. Wexx –> Wex? 2. Line 96 be –> been 3. Code and data availability: the link authors provide does not link to the measurement data, please upload the data.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2020-328/acp-2020-328-RC1-supplement.pdf

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