ANSWER TO REVIEWER 1

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.

We are grateful to the reviewer for his/her comments that have helped us improve our manuscript.

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.

   Thank you for this suggestion. The simulation with no SIP at all was initially included in the Supplementary Information, in the section that the sensitivity to the H-M process was tested. This test was referred as 'CNTRL_NOHM', which corresponded to a simulation with the default M05 but with H-M deactivated. In the revised text we now refer to this simulation as 'NOSIP'. Moreover, we have included the 'NOSIP' results in Figure 2, to help the readers quantify the impact of secondary ice production by comparing the rest of the simulations to 'NOSIP'.

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.

Thank you for pointing out this mistake. Cloud ice is converted to snow when diameter exceeds d_{cs}=125 μm. However since cloud ice (and snow) size spectra are represented with a complete gamma distribution, this means that sizes larger than 125 μm are not excluded in the spectra (and, in fact, by the definition of a complete gamma function the sizes mathematically extend to infinity). However, if the characteristic diameter of the cloud ice category is larger than 250 μm then all cloud ice is converted to snow in M05. Since break-up in FRAG1siz is allowed only if the characteristic diameter of the particle that undergoes break up is larger than 300 μm, collisions with cloud ice do not contribute to multiplication. This statement is now corrected in the revised text (lines 219-221).

Regarding the modeled output plotted in Figures 2-3: when outputs are compared to observations, then only particles larger than 80 μm are accounted for consistency with the cloud phase detection limit in the observations; this holds for model data plotted in Fig. 2 and Fig. 3a,c,d. In Figure 3b, graupel is not compared to observations, so we plot the whole graupel spectrum to show how limited its concentration is. This is better explained in the revised text (lines 282-290) and Figure caption to avoid confusion.

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.

   We have corrected the adapted threshold in this figure; this is now set to 250 μm for consistency with the microphysics scheme assumptions (see answer above). Although it is hard to separate cloud ice/snow in observations, 2DS measurements offer a good indication of the size spectra. Plotting cloud ice and snow categories separately in this figure is important, because it shows that including break-up in the scheme results in more realistic representation of the microphysical properties. Particularly, it is important to show that it is the concentration of the small particles that is substantially underestimated in the control simulation (more than 2.5 orders of magnitude, see Figure 3a); concentration of large particles (>250 μm) is underpredicted by less than a factor of 1.5 (see Figure 3c). Thus, break-up shifts the modeled size spectra towards smaller values in agreement with observations, which indicates that we are likely capturing the correct mechanisms responsible for the ice microphysical characteristics. This is now discussed in lines 302-307. We have also included a fourth panel in this figure, to show the vertical profiles of total ICNCs.

4. The fourth concern is related to parameterization from Phillips et al. (2017) 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.

We apologize for this confusion. In the Phillips parameterization, graupel particles are highly breakable as Ψ is assumed to be larger than 0.5; the fragment generation does not depend on Ψ and is only a function of temperature (see equations at line 622 in Appendix B). In contrast, Ψ is important for cloud ice/snow and that's why fragment generation directly depends on Ψ for these ice categories (equations in line 609). The parameterization assumes that Ψ for these ice types is less than 0.5, lower than for graupel, in agreement with the reviewer's comment.

The different assumptions regarding the rimed fraction of graupel and the rest of the ice categories are now discussed in detail to avoid confusion (lines 229-235 in the revised text). It is also explicitly stated that variations in Ψ only affect the break-up efficiency of cloud ice/ snow.

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?

   This is an excellent point. Only the mass mixing ratios of liquid and ice particles are transferred to the radiation scheme and drive the differences reported in Table 1. No information on effective radius is directly passed to RRTMG scheme. This is now clarified in the text (lines 323-325).

Technical corrections

1. Line 59 "Lachlan-Cope et al., 2016; Wexx et al.,". Wexx –>Wex?
   Although the statement was more general about polar clouds, we have removed the reference 'Wex et al' since this concerns Arctic clouds
2. Line 96 be –> been
   corrected
3. Code and data availability: the link authors provide does not link to the measurement data, please upload the data.
   We apologize that the direct link did not work in the previous manuscript. However, the provided URL address is valid. Hopefully, the link works fine in the revised version (in any case copy-pasting the address in a browser should definitely work)

Comments embedded in the pdf version of the manuscript

The reviewer points out that strong riming is the foundation of the hallet-mossop process. This sounds somewhat contradicting to our findings regarding the fact that only simulations that assume a high rimed fraction for the particles that undergo break-up during ice particle collisions produce realistic ICNCs, while at the same time H-M remains inactive.

While in Phillips parameterization for break-up, rimed fraction is explicitly considered, this is not the case for H-M. To make sure that only sufficiently rimed particles contribute to multiplication, H-M is activated only if the ice particle mixing ratio exceeds 0.1 g kg^{-1}. However, this threshold is ad-hoc and is tuned for mid-latitude clouds, but is hardly exceeded in polar clouds. In the former submission we discarded the liquid thresholds in the parameterization and found no impact on the results. In the revised manuscript, we removed both liquid and ice thresholds and allowed H-M to be active over the whole droplet and snow/graupel spectrum. This enhances the H-M efficiency by a factor of 3 but still cannot reproduce observed mean ICNCs. This set-up overestimates H-M efficiency, as size limitations have been reported in laboratory studies, but the existing thresholds in M05 should be refined for polar clouds. The whole text has been moved from the Supplementary Material to the main manuscript (section 4.3).