Followup review of “Secondary ice production in summer clouds over the Antarctic coast: an underappreciated process in atmospheric models” by Sotiropoulou et al.

General Comments

The authors have done a very nice job of pulling material into the main manuscript or otherwise clarifying statements, measurements and modeling parameters in responding to my original comments. I have just a few suggestions in hopes to make crystal clear what advances are made in this paper versus what pieces of the puzzle remain for investigation. In particular, the paper still somewhat dances around the point that primary ice nucleation based on current information appears not capable of providing the requisite starting ice concentrations for the BR process to then provide the critical secondary mechanism needed to explain ultimate ice concentrations achieved in pockets of clouds over the region. A qualitative word, “meaningful” is used to describe ice concentrations of relevance, and yet, what has been measured so far in the region of interest is then not “meaningful.” Just a few word changes will clarify this point.

Specific Comments

1) Abstract, line 36: I suggest “necessary and sufficient”. This seems to me to be true. Sufficient alone implies to one that this is not difficult to explain. I understand that some possible explanations have been provided, but there remain significant efforts to prove any such points. I do not mean to detract from the significant results of the paper, only to emphasize mildly that this is a missing piece.

2) Section 4.4, lines 399-404: I believe that it is not an issue of calibration or even potentially missing INP sources, as this presently suggests. I can suggest the emphasized words in “None of the utilized primary ice nucleation parameterizations were **developed or are likely representative for**…”. The following sentence appears irrelevant and should be removed. The pristine or limited nature of the INP populations in the region is the important point, and no new source is likely to explain any missing populations. If trying to say that new parameterizations will be needed to accurately represent sources, that would be fine, but it is not articulated that way in this revised wording.

3) Paragraph starting on line 440: This is excellent. One minor note on line 437. Terrestrial INPs are “assumed to be” or “expected to be” higher? The noted model study reference discusses contrasts between land and oceanic INP concentrations, but does not demonstrate that Antarctica is a specific elevated source of INPs.

4) Conclusions, lines 455-457: Again the same point as above. Suggest (new words emphasized in bold and others crossed out) “…such conditions could ~~likely~~ be achieved through ice seeding (as likely happens in the examined case) or through INP transport from the Antarctic continent, where INP concentrations are ~~generally~~ **predicted to be** higher (Vergara-Temprado et al., 2018). **These points remain for future confirmation.**”

5) Appendix A, line 500: Suggest to replace “have been calibrated” with “were not developed to be representative of”. Also, add “parameterizations” after Cooper and DeMott.