Reply to Anonymous Referee #3

The paper addresses an issue of interest, and I believe it deserves to reach out the scientific community working on modelling of ice formation in clouds.
Overall, I share the same doubts and reserves as the other reviewer as long as the readability of the current paper is concerned. The authors need to make the paper self consistent and easier to read. Please explain all the assumptions taken and describe the parameterizations with some level of details. Just referencing to existing works is not enough! I'm not familiar with the work of Miltenberger et al., don't expect that other readers will be.

Reply: We have added more detail on the set-up of the simulations (Appendix A, l. 74-75, and l. 80-84 of the new manuscript) and also the comparison to observations as reported in our earlier work on this case (l. 120-148).

Please consider adding a table showing the main features of each parameterization, guiding the reader through your methodology. The commonalities and differences of each scheme is functional to discuss the spread of the ensemble. Without providing info about the diversity/commonality of the underlying assumptions of each scheme, how is possible to interpret if the spread of the ensemble reflects true physical uncertainty? Perhaps all schemes descend from the same physical assumptions, in that case I would expect an overconfident ensemble spread. As the paper stands at this stage, it cannot be deduced.

Reply: We have added a more detailed description of the different parameterisations and the resulting difference in the temperature-dependence of INP number concentration, which is the main impact of the different parameterisations (l. 99-116 of the new manuscript).

Another obscure point to me is the use (or not use) of 'observational data'. At the beginning of section 2 the COPE campaign is mentioned. What about using the data collected there to shed some light on the bias/error of the modelling results? if this is part of the baseline simulation it needs to be clarified. Maybe I'm missing something, but I believe that the use of measurements could enormously add value to the current findings (at least, if possible, for one variable; I believe it'd be very important if you did).

Reply: A paragraph summarising the comparison with observational data for the baseline case in Miltenberger et al (2018a) has been included (l. 120-148 of the new manuscript). Here we also detail how the comparison to radiosonde and radar data differ for the new sensitivity experiments. We agree that a more detailed comparison to for example in-situ observations of ice-crystal number density or 3D radar reflectivity structure would be interesting. However, the model data is either not available (radar reflectivity) or is not available at a sufficiently high time resolution to allow for a meaningful comparison (in-situ data is mainly sampling rising cloud tops). We briefly comment on this in the abstract.

On a less general note:

- *consider adding a description of Hallet-Mossop process (and maybe acronym it to H-M);*

  Reply: We have added a short description of the Hallet-Mossop processes in l. 93-96 of the new manuscript.

- *consider give percentage of the values in table 1, absolute magnitude alone doesn't say much about variability;*

  Reply: We have altered the table to the relative spread in the variables, i.e. the ratio between the difference between maximum and minimum value to the value in the baseline simulation (Tab. 1 of the new manuscript).

- *line 8: perhaps you meant 'changes' rather than 'change'?*

  Reply: Thanks for spotting this, this is corrected in the new version.