Response to the reviewers

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August 2024

Comments to the Editor

We address all minor change suggestions. Please find a point to point response to the comments of the reviewers underneath. Additionally, we did some minor changes without changing any content, mostly to have a consistent style for numbers and units, which can be found in the track-changes document. We like to thank the reviewers for their continued improvement suggestions and their help in improving the manuscript. In the following, we address their remaining concerns and comments about our work.

Reviewer 1

"The comments were well addressed by the authors. There are only a few technical corrections remaining (comment numbers from the author's answer):"

Comment 2-3 — "It should be "effect on", not 'effect in'."

Reply — This has been corrected in the manuscript.

Comment 12 — "The author's answer states that they have changed the wording, but the revised manuscript and the track-changes file don't show this. Probably the changes weren't saved."

Comment 13 — "Same as comment 12. The paragraph that should be removed (currently it is appearing double - here and in the beginning of the Results section) is still there."

Reply — The missing changes are included in the manuscript now, thank you for noticing. We can only assume that this is the result of a temporary connection loss to the online text editor.

Comment 22 — In line 346 in the track-changes document, the word "the" appears twice ("comparing the the timesteps"). But actually, I suggest further changes than only removing one "the". I would suggest to change this sentence to the following to make it clearer: "As Fig. 6g shows, the increase in CRE in the VL setting is especially pronounced during timesteps with a smaller CRE." (The words "more" and "comparing" suggest a comparison, but this is not really given in this sentence.)

Reply — We adapted the suggested reformulation.

New comment regarding Fig. 6 — It looks like you put in the wrong plot in Fig. 6g when correcting the figure towards non-italic axis labels (same plot as 5g, PEA instead of SO). Please correct that.

Reply — This is indeed an error, so we corrected Figure 6, as well as Figures 2 and 3, where the colorbar for the Cloud Ice Content plots incorrectly said "Cloud Liquid Water Content".

Reviewer 2

"The authors have done a conscientious job in responding to the reviews and the manuscript has been improved."

Comment 1 — "It seems the observational INP data are not being used as a very strong constraint in this work. The authors note that their focus is not on the spectra themselves (the temperature dependence of the INP) but on differences between overall INP concentrations. This comment, about the temperature dependence of the INP spectra not being relevant, seems a little strange, since the contrasting behaviors between relationships in summer and winter were explained by temperature differences."

Reply — We adapted the discussion to address this point:

- Nevertheless, our findings with respect to the temperature sensitivity of the cloud response to INP concentration changes should still hold, as the steeper increase in the activation temperature profile measured in campaigns such as MARCUS [Vignon et al., 2021] would only cause an even stronger temperature sensitivity than the gradual increase we used here. In addition to that, the temperatures in winter at the station are low enough to explain the reduced sensitivity by homogeneous nucleation. Only the low sensitivity we found over the Southern Ocean in summer would potentially be affected, as the temperature range found for this situation would mostly fall in the region with a higher increase, but even keeping that in mind, the INP settings we tested covered a wide enough range for any increase from realistic values to fall into the tested range.

Comment 2 — "I may be missing something, but in comparing the timeline of INP concentrations (now shown as Figure A3) with the simulation time period shown in Figure 2, the cloud seems to form in Figure 2 starting 2012-02-09 through part of 2012-02-11. Does this not coincide with the very last portion of the Figure A3 timeline, where the INP concentrations drop dramatically? Is this drop due to scavenging by the cloud and is that a significant effect on the cloud evolution? The explanation provided for fluctuations earlier in the run is that concentration changes are due to synoptic-scale weather systems, presumably because scavenging of INPs elsewhere in the domain had occurred prior to arrival at the site."

Reply — We compared cloud water and ice contents with wind direction and INP concentration and can verify that the INP concentration drop shown in Figure A3 is caused by upstream scavenging, as can be seen by the very low

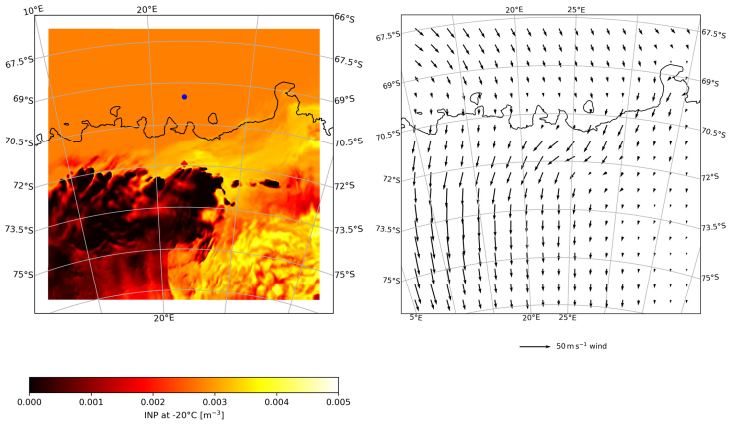

Figure 1: INP concentration (left) at -20°C and wind direction and strength (right) on 10 February 2012, 00:00h, at a model level corresponding to 2250 m height a.g.l. at PEA (red diamond), in the VL simulation. Please note that only INP concentrations of the fifth temperature bin are shown, i.e. not the sum of temperature bins 1 through 5.

INP concentration in areas corresponding to the areas with strong northerly winds in the Figure underneath. We also added a sentence to the corresponding paragraph to clarify this:

- The drop in concentration towards the end of the shown period can be explained by upstream scavenging of available INPs (not shown).

Comment 3 — "This is a study aimed at cloud processes, namely, the competition between liquid and ice particle formation and evolution. I encourage the authors to implement further diagnostics into the modeling framework so cloud processes can be more thoroughly probed. In this way, they can ascertain whether secondary ice processes are playing a role in the simulations."

Reply — In our paper, we looked into the spatial and temporal evolution of cloud ice, cloud water, and INPs. We acknowledge that further research regarding secondary ice processes that play into ice production is valuable. We herefore suggest this in the conclusions now. However, implementing further diagnostic variables is out of scope for this paper and would require changes to the model as well as new simulations. We keep this feedback in mind for follow-up work.

References

E. Vignon, S. P. Alexander, P. J. DeMott, G. Sotiropoulou, F. Gerber, T. C. J. Hill, R. Marchand, A. Nenes, and A. Berne. Challenging and improving the simulation of mid-level mixed-phase clouds over the

high-latitude southern ocean. *Journal of Geophysical Research: Atmospheres*, 126(7):e2020JD033490, 2021. doi: 10.1029/2020JD033490. URL https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020JD033490. e2020JD033490 2020JD033490.