Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-696-RC2, 2022

Comment on acp-2022-696

Anonymous Referee #2

Referee comment on "The chance of freezing – a conceptional study to parameterize temperature-dependent freezing by including randomness of ice-nucleating particle concentrations" by Hannah C. Frostenberg et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-696-RC2, 2022

Review for “The chance of freezing --- parameterizing temperature dependent freezing including randomness of INP concentrations” by Frostenberg et al.

This manuscript develops a new parameterization of immersion freezing to take into account the fact that ice-nucleating particle (INP) concentrations span many orders of magnitude at any particular temperature within the mixed-phase cloud temperature range. The authors’ approach was to derive a lognormal distribution of INP concentration (INPC) based on ship-based data of INPs collected on filters for subsequent drop freezing experiments for relatively warmer temperatures and diffusion chamber experiments for relatively colder temperatures and is only dependent on temperature (and aerosol-independent). From the INPC lognormal function, a temperature-dependent relative frequency distribution (RFD) was derived, from which an INPC value is drawn from when a freezing event occurs. In this way, different values of INPC can occur for the same value of temperature while remaining relatively computationally efficient (twice the cost of assuming a constant ice number concentration). The authors tested their parameterization using a large-eddy simulation (LES) model, MIMICA by its ability to represent a mixed-phase stratocumulus cloud observed during the Arctic Summer Cloud Ocean Study (ASCOS) Arctic field campaign. They conducted various sensitivity tests including how the mean, standard deviation and bin size of the INPC function and resolution and time step of MIMICA impact the simulation of mixed-phase cloud properties. The authors conclude that their new scheme is highly sensitive to the standard deviation of their INPC function and that it can simulate cloud ice mass reasonably well.

Overall, I find the idea presented by the authors interesting and potentially useful for improving the realism of immersion freezing by taking into account the variability of INPC for a given temperature. The manuscript is quite well-written, particularly the introduction and methods, however, I had a harder time following the explanations of the results of the sensitivity tests which I also found speculative. I am not quite convinced of how effective the new scheme is, however, because (i) the potential lack of realism of the model (missing graupel, secondary ice production, no comment on other ice nucleation processes), (ii) the fact that the scheme is based on observations at low latitudes whereas it was tested in the Artic, and (iii) the qualitative comparisons with the observations. I also find some of the explanations and design of the experiments unclear which makes it difficult to fully comprehend and assess the scheme. However, with some additional clarifications, the novel idea may have merit and in my opinion could be worthy of consideration for publication after major revisions. Detailed comments follow.

Major comments:

- My most major concern about the dataset used is that it only uses surface-based INP concentrations which may not be representative of the cloud-layer (see e.g. Creamean et al. 2018, Griesche et al. 2020). For a decoupled cloud system such as the one that is actually the focus of this study, the surface-level INPs may not impact the Arctic low-level clouds of interest. Furthermore, these observations were made over Cape Verde, while the parameterization is being tested in the Arctic, which has very different INP abundance and composition. Assuming this parameterization might also be applicable to large-scale climate models, it has been shown that the vertical structure of INPC can play an important role in simulating the Arctic and how it changes in the future is especially sensitive to the vertical structure of INPC in a large-scale model (Tan et al. 2022). Please discuss the limitations of the assumptions of the framework in the context of the surface observations that were used and how they may influence the simulation of cloud properties and how they may also potentially influence future climate projections if the scheme can be applied to climate models.
- It is curious that the ASCOS observations are not directly plotted in the figures. Could the authors add the observations for a more direct quantitative comparison with their results instead of qualitatively referring to previous papers?
- I'm unclear about why the sensitivity test for the sampling frequency is designed the way it is. In particular, starting on lines 264-267: "Freezing events still occur at every time step, but within the respective time period (e. g. five seconds or 60 minutes), the INPC is constant at one grid point for the time period. If the temperature at the grid point changes before the completion of the time period, a new INPC is drawn earlier." Because a new INPC will be drawn earlier if the temperature of the grid point changes before the completion of the time period, then that should mean that each of the sensitivity tests will draw a new INPC before the specified frequency. It therefore seems to me that specifying the frequency in the sensitivity test is not useful since a new INPC can be drawn before the specified frequency. So in the end, are the simulations really comparing the effect of sampling frequency? Could the authors clarify why the sensitivity test was designed this way?
- If the various sensitivity tests are not identically initialized, then if an INPC is drawn

before the time period specified for the sensitivity test ends, then the comparison of the different sampling frequencies are not directly comparable. However, if all simulations are initialized

- It's not clear to me why the authors deem 300 random draws to be the threshold for a reproducible prediction of the RFD. From Fig. 2, it appears that the mean root-mean square error (RMSE) seems to plateau closer to 1000 draws, when the mean RMSE is < 5. This is substantially larger than 300 draws and more than the 9216 grid points they quoted with the same temperature. Could the authors explain how they arrived at this conclusion in Section 2.1.2?

Minor comments:

- The acronym ASCOS should be spelled out when it first appears on line 66.
- Line 97: With regard to Solomon et al. 2015, I think you mean INP "recycling" (and not "cycling").
- How exactly are the INPC drawn from the distribution? Does the subroutine use a random number generator?
- Line 233: "on" should be replaced with "at".
- In Fig. 3, I can't find anywhere in the manuscript that explains how the different simulations in the ensemble members differ. Please explain.
- Lines 233-236: This discussion about secondary ice production (SIP) is however not relevant to the discussion of Fig. 8 since the appendix states that MIMICA does not represent SIP (line 479).
- Some of the explanations are speculative although plausible (e.g. pertaining to discussion starting on line 177). Can the tendencies of the associated processes be displayed somewhere to substantiate this discussion?
- How are other modes of ice nucleation represented (if at all)?

References:

- Creamean, J. M., Kirpes, R. M., Pratt, K. A., Spada, N. J., Maahn, M., de Boer, G., Schnell, R. C., and China, S.: Marine and terrestrial influences on ice nucleating particles during continuous springtime measurements in an Arctic oilfield location, Atmos. Chem. Phys., 18, 18023–18042, https://doi.org/10.5194/acp-18-18023-2018,

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- Griesche, H. J., Ohneiser, K., Seifert, P., Radenz, M., Engelmann, R., and Ansmann, A.: Contrasting ice formation in Arctic clouds: surface-coupled vs. surface-decoupled clouds, Atmos. Chem. Phys., 21, 10357–10374, https://doi.org/10.5194/acp-21-10357-2021, 2021.
- Tan, Ivy, Donifan Barahona, and Quentin Coopman. "Potential Link Between Ice Nucleation and Climate Model Spread in Arctic Amplification." *Geophysical Research Letters*4 (2022): e2021GL097373.