The Chance of Freezing - A conceptional study to parameterize temperature-dependent freezing by including randomness of INP concentrations

Hannah C. Frostenberg[1], André Welti[2], Mikael Luhr[3], Julien Savre[4], Erik S. Thomson[5], and Luisa Ickes[1]

[1]Department of Space, Earth and Environment, Chalmers University, Gothenburg 41296, Sweden
[2]Finnish Meteorological Institute, Helsinki 00101, Finland
[3]former Department of Meteorology, Stockholm University, Stockholm 10691, Sweden
[4]Meteorological Institute, Faculty of Physics, Ludwig-Maximilians-Universität, Munich 80333, Germany
[5]Department of Chemistry and Molecular Biology, University of Gothenburg, Gothenburg 41296, Sweden

We thank both reviewers for the constructive feedback in this second round of reviews. Below we copied the reviewers' comments in black and added our responses in blue.

1 Response to referee 1 (report #2)

General comment

I thank the authors for the care they took in answering my earlier comments as R1 in the previous review. Requests for clarification and technical comments in particular were very well adressed.

I think the manuscript is now stronger and more clearly presented as a first exploration of a novel concept in parameterizing INPs in models. However I think this last point could still be improved: the authors stress in their answer that "this paper represents a first attempt to use a random approach [...] and should be seen as a novel concept and its analysis rather than a finished parameterization scheme to be used in other models". I think such an exploratory study is very relevant for ACP, but that this point still needs to be stressed as explicitly as in this citation in the abstract. Also in the response: "The main goal of our study was to test the concept of random drawing from an INPC distribution instead of using fixed values only depending on, e.g., temperature, and one major result is that it is the rare, large INPCs that control the freezing in a cloud and not the median values". This could be in my opinion also present as-is at the end of the introduction when presenting the objectives of the study. It would be better in order to indicate more clearly the limitations of this approach and how this work should be extended in future research.

Thank you for this feedback. To state limitations and exploratory character of the study more clearly, we added similar statements to the abstract and at the end of the introduction as suggested by the reviewer (see revised manuscript).

1.1 Detailed comments

1) "It is not possible to increase the drawing frequency to higher values than the model time step". It would be possible to decrease the model time step and use this as a new baseline, but I agree that the additional computing cost makes this unreasonable to ask for here. Maybe you should note here that for this reason, changing the model time step could have an effect on the parameterization.

We added this limitation and implication of the time step to the manuscript in section 3.4.4 (see revised manuscript).

2) "However, we agree that ideally, the values should converge for higher drawing frequencies". I also think this should be said explicitly in the text, and that this is something that future work should investigate.

We added this now in section 3.4.4 and in the conclusions (see revised manuscript).

3) Using the Fletcher parameterization leads to even lower ice formation than the new parameterization. But is this the only difference between the schemes? What would be the difference between this scheme and a Fletcher parameterization tuned up to produce more ice? The discussion could mention that future work could investigate how the parameterization's behaviour differs from a time-independent one, other than just the amount of ice which could always be adjusted up or down in models. The added value of the new scheme could be more clearly revealed from a detailed comparison with a simpler adjusted scheme.

Unfortunately, we did not investigate the difference between this scheme and a tuned Fletcher parameterization (we agree that this would be an interesting test for further studies). One question here is how much the Fletcher scheme would need to be tuned and if that is still a realistic representation. We can try to make some assumptions based on some of the simulations that we did. In Fig. 5 one can see the difference in the new scheme due to a shift in the median of the RFD. To achieve a more realistic IWP one would need to shift the median by at least a factor of three. Since the Fletcher scheme is a lot lower compared to the standard of the new parameterization (F23), it would need to be tuned a lot more than that, probably at least by one order of magnitude.
Following the reviewers suggestion we added this point as outlook on further research at the end of Section 3.3 (see revised manuscript).

4) It's still not clear why a lognormal distribution of INPC would require additional normalization for sampling. Drawing from a lognormal distribution, while not completely straightforward in FORTRAN, does not require some special renormalization (see for example https://numpy.org/doc/stable/reference/random/generated/numpy.random.lognormal.html for an implementation of a lognormal draw in python, and for a normal distribution in FORTRAN an implemen-

tation of the Box-Muller algorithm here https://masuday.github.io/fortran_tutorial/random.html). You should consider attaching the implementation and especially the sampling code to the article to make this work more easily reproducible.

The distribution is normalized to give a cumulative probability of 1 at each temperature, independent of the discretization of the INPC field (size of the bins). The normalization factor is a constant, depending on the definition of the INPC field. We added a clarification on this in Section 2.1 (see revised manuscript).

5) “Note that since large INPC and the chance to draw this large INPC determine the ice in the cloud, this introduces an indirect time-dependency of the scheme. The resulting ice in the cloud depends indirectly on the time until a large INPC value is drawn at a specific grid point. Increasing the drawing frequency leads to increases in the ice variables that do not appear to converge” You could note here that it might be possible to correct for this time scale dependence by introducing an additional correction parameter based on the drawing frequency in future implementations.

Good point. We added this in Section 3.3.4 (see revised manuscript).

2 Response to referee 2 (report #1)

I thank the reviewers for their item-by-item responses. Regarding the first and most major comments, although they have been answered, I do not see where they were addressed in the manuscript itself along with relevant references.

For clarity we restate the first and most major comment to which Referee 2 refers:

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.

What may not have been clear in our previous response is that the discussion which is initiated by the referee’s comment, namely the connection between ground-level measurements and cloud-level INP etc., introduces significant open questions in the field. However, they are largely beyond the scope of this study, which was to investigate how we might incorporate variability and a distribution of INP occurrence into a cloud-resolving model. For this exercise, the width of the distribution and the temperature-dependent change in INPC are much more relevant than the absolute INPC that the reviewer is concerned about. We simply chose a representative INP distribution, and also showed in the previous response that it is robust for both Arctic measurements and the equatorial measurements from which it was extracted. It is for these reasons the referee does not find much altered text directly addressing the comment. We hope our re-emphasis of the "conceptual character" of our study serves to help clarify this point.