Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-511-RC2, 2021

Comment on acp-2021-511

Anonymous Referee #2

Referee comment on "Aerosol–cloud interactions: the representation of heterogeneous ice activation in cloud models" by Bernd Kärcher and Claudia Marcolli, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-511-RC2, 2021

Review for Kaercher and Claudia (2021, ACP)

This study discussed the representation of ice activation in cloud models and identified a problem with the application of cumulative activation fractions when considering the INP/ice particle budget. The authors formulated differential activation fractions that are consistent with the reduction of INP number after activation and demonstrated that the new representation can prevent the INP overestimation. They applied the new formulation with lab-based soot INP measurements and showed using the differential activation fractions indeed prevents the INP overestimation.

The manuscript is concise but very clearly written. The derivation of the formulation is inspiring. This work will improve the INP representation in cloud parameterizations, especially for considering the competition between homogeneous and heterogeneous ice nucleation processes. I recommend publication after some clarifications. Below please find my specific comments.

Title: In my opinion, the title is a little bit too general. A more specific title would be better, e.g., something like "Improving the heterogeneous ice nucleation parameterization using differential activation fractions"?

Line 31-32: Then for immersion freezing measurements that are reported only as a function of temperature, does the INP overestimation problem also exists?

Figure 1 caption (3rd line): "'no budget' approach, arrows labeled with φ". There are three arrows labeled with φ, but it seems only two of them indicate no budget approach?

Page 5, Line 103-105, formula (7): Could you please elaborate how you came up the idea of using such a mathematical form? In other words, why other forms can not conveniently fit cumulative AFs? Does it work for other activation fraction forms other than the one (ns function) reported Ullrich et al. (2017)? Also, maybe consider showing the measured and fitted curves in a figure as appendix? Just to have an idea about how "reasonable" it is.

Page 5, Line 108-110: It seems that the choices of s* and &s (a factor of 3 changes) are a bit arbitrary (or did I miss something important?). If s*=0.352 and &s=0.0175 are used for plotting figure 2, how will the results look like? (I assume &s here is not the grid spacing delta_s).

Page 8, Figure 4: It looks a bit surprising to me that the activation fraction for soot with 400nm size is similar to that for the desert dust particles as shown in Figure 2. Also, do you still need to fit cumulative AFs for this application? If so, how did you choose s* and &s?