

Interactive comment on “The Prevalence of Precipitation from Polar Supercooled Clouds” by Israel Silber et al.

Anonymous Referee #2

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The manuscript “The Prevalence of Precipitation from Polar Supercooled Clouds” by I. Silbert and colleagues documented an observational analysis of long-term measurements at two sites, one in Alaska and the other in Antarctica. They found that the supercooled clouds produce frequent precipitation at cloud base and most of them reach the ground. The attributed the discrepancy between their findings and the previously reported spaceborne estimates to the detection limitation of the spaceborne measurements. Finally, the authors suggest that the supercooled cloud formation is an important gateway for ice formation, and that the cloud base precipitation statistics can be used for evaluating large-scale models. Overall I find this study very interesting and the manuscript very well-prepared. The presentation is straightforward and easy to follow. The text is concise. The methods are described clearly, and the science findings

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are supported by evidence and are potentially significant. Therefore, I recommend this manuscript to be published.

I only have a few comments for the authors to consider:

1. The authors did not clearly state the phase of the supercooled clouds and their precipitation. Because different mechanisms drive different clouds, it will be very helpful to explicitly discuss the phase of clouds (mixed phase, supercooled liquid, or pure ice) as well as the phase of the precipitation (liquid, ice, or both) in each analysis throughout the manuscript.
2. This study relies on ~ 7 years of measurements at Alaska and ~ 1 year of measurements in Antarctica. Are these results representative enough? Are there any other previous, ongoing, or future measurements that can provide further validation? The ambient environmental and meteorological conditions are not discussed in the manuscript. Do they play a role? It will be helpful if the authors provide a paragraph to discuss these aspects.
3. The sampling and the procedure for data processing are described very clearly, but it will be helpful to discuss the caveat, limitation, or consequences of the sampling or data processing procedure, especially if any of the steps (or assumptions) might affect the science conclusion. It will also be helpful for the readers to know even if the procedure will not affect any of the conclusions.
4. The discussion on the INP is a hypothesis used for explaining part of the precipitation statistics, but unfortunately the paper also indicates that there is no INL observations at the two sites and therefore the paper did not provide any evidence or analysis on INP. I am wondering if field campaign measurements can be used to bridge the gap and provide solid evidence to back your hypothesis.
5. While I understand that the authors suggested the use of precipitation statistics at cloud base for model evaluation because it is independent of atmospheric thermody-

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namics and can be retrieved with certain degree of confidence, it is unclear to me how it matters in climate modeling and what processes it tries to constrain. It will be much more helpful (and potentially increase the community's interest in using this new metric for evaluating models which eventually increases the scientific significance of this paper) if the authors elaborate why this metric is useful and which process(es) in models can be constrained by it.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-993>, 2020.