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Interactive comment on "Contrasting ice formation in Arctic clouds: surface coupled vs decoupled clouds" *by* Hannes J. Griesche et al.

Anonymous Referee #1

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This study examines the impact of the surface coupling state of polar clouds observed during the PS106 voyage on the probability of ice occurrence. The authors use a comprehensive set of ground-based measurements to detect cloud boundaries, detect the presence of ice, and analyze the thermodynamic state of the atmosphere. The authors suggest that cloud coupled to the surface contain ice more frequently than decoupled clouds because of enhanced INP concentrations transported from below.

The manuscript provides a comprehensive introduction with a reasonable literature survey. A common change of tense in the introduction and other parts of the manuscript (e.g., p. 4 l. 3-30, p. l. 7-10) could impact to flow of the text to some readers, and hence, I recommend the authors to revisit this issue.

While I agree with the general conclusions made by the authors, I find multiple weak-

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nesses in the methodology, which could significantly impact the analysis quantitative results, even if the conclusions will be similar. Therefore, I recommend major revisions of this manuscript before it should be considered for publication in ACP.

Major comments:

- The authors have the 35 GHz ground-based radar data at their disposal, one of the best remote-sensing instruments for the detection of precipitating hydrometeors (and ice particles in particular), even in very small concentrations. Yet, they only use the lidar data to detect precipitation. Commonly occurring cases of weak Arctic precipitation can be missed by lidars in such cases, as a result of the potentially minor contribution of very small ice concentrations to an air volume's total cross-sectional area of scatterers (e.g., when including nearly spherically-shaped aerosols). This is also evident in Fig. 2, where there is a clear indication of (weak) precipitating fall streaks in the radar data between 18-21 UTC (also suggested by the depolarization plot), even though this period is classified as an ice-free cloudy period. I think that the authors should incorporate the radar data in their analysis, because at the moment, weakly precipitating clouds could significantly change their analysis results (see for example Fig. 3 in Buhl et al., 2013; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50792) in the proceeding figures.

- In continuation of the previous comment regarding ice detection using lidar depolarization ratio data, the analysis could have been influenced by specular reflection from plate ice crystals within the -20 – (-8) °C temperature range. In these cases where plates precipitate from the cloud base, the determined cloud base might be lower than it actually is (depending on the depolarization threshold), and a cloud can be classified as ice-free since the change in depolarization or the depolarization threshold for ice detection (not clear from the text) is not strong enough. I know that the common tilting of lidars by a few to several degrees off zenith (e.g., 5 degrees as in the PS106 voyage) is commonly believed to address this specular reflection issue, but that is a common misconception, as it does not consider commonly observed higher canting angles of ice particles (see for example Appendix A in Silber et al., 2018; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2017JD027840, Noel et al., 2002; https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2002GL014828).

- I find the theta criterion for the determination of surface coupling state problematic because it doesn't consider the cloud height above the surface, which could result in more lower-level clouds being classified as coupled. As an example, a cloud base at 200 m with theta difference just below 0.5 K (e.g., theta of 260 K at the surface rising linearly to roughly 261 K at cloud base) would be considered coupled even though dtheta/dz = 5 K/km, which is strongly stratified. In their analysis, the authors should take into consideration the height dimension as well as the measurement uncertainty of the RS-41 radiosondes (0.3 C in T, 4% in RH). The current potential for a low-level coupled cloud bias could contribute to the stark coupled vs. decoupled ice occurrence fraction differences at higher temperatures discussed in Fig. 4, given the summertime dataset manifested in the greater occurrence of lower, warmer clouds (see for example the results from Svalbard in Nomokonova et al., 2019; https://acp.copernicus.org/articles/19/4105/2019/).

- Estimation of the INP number concentration: the method in Mamouri and Ansmann (2016) relied on European and Mediterranean data of aerosol mixtures, the values of which can be significantly different from Arctic regions (see for example Kanji et al., 2017, https://journals.ametsoc.org/mono/article/doi/10.1175/AMSMONOGRAPHS-D-16-0006.1/28236). Moreover, in the Arctic alone it has been shown that there is high INP variability and that INP concentrations are not correlated with multiple types of aerosols (e.g., Wex et al., 2019, https://acp.copernicus.org/articles/19/5293/2019/). Given the fact that based on our current knowledge INP occupy only a small fraction of the total aerosol number concentrations (and likely their projected area), there are just too many degrees of freedom in the INPNC retrieval and I do not see how can the authors estimate the INPNC even with the scaling factor they decided to use, and do not see how their conclusions could be dismissed even without the rather short INPNC analysis discussion. I find it very hard to believe that the uncertainties in INPNC (as shown in Figure 6) are smaller than an order of magnitude.

Minore comments:

- There is no information on the route of the PS106 voyage. I recommend adding a map for reference or at the very least specify the latitude/longitude ranges of that voyage.

- p. 1 l. 2 - suggest defining that OCEANET is a platform in the first instance.

- p. 1 l. 11-12 “This provides further evidence . . .” – this sentence is not supported by the analysis and is not explicitly discussed in the text. I recommend removing it or revising the analysis and text accordingly.

- p. 1 l. 13 – “acting as seeds for ice multiplication” – again, this impact of seeding from below is not discussed and supported by the text (might be suggested only implicitly in the discussion about blowing snow).

- p.1 l. 16-18 - suggest reordering these two sentences.

- p. 1 l. 23 - "above the one" - suggest rewording

- p. 3 l. 34 - "as it is the case for the 35-GHz ..." - Even though the ARM KAZR is a valid example, I recommend either removing this part of the sentence or providing a different example, because the KAZR nor the Barrow site are not discussed in this paper.

- p.3 l.21 - "First studies ..." - I do not understand this sentence - suggest rewording

- p. 3 l.27-29 - is this *the main feature* of Arctic clouds, or simply one of their common features? Also, do the clouds necessarily form in inversions, or do they form the inversions? I think that both options are plausible (see for example Morrison et al., 2012; https://www.nature.com/articles/ngeo1332, Silber et al., 2020; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL087099, Sedlar, 2014; https://journals.ametsoc.org/view/journals/apme/53/12/jamc-d-14-

0065.1.xml?tab_body=fulltext-display)

- p. 6 l.3 - please clarify whether the linear or circular depolarization ratios are used (I suspect the former).

- p. 9 l. 31 - it could be the vast majority of clouds (> 80%) but this is certainly not every cloud.

- What are the depolarization thresholds for the determination of liquid and ice? Are there backscatter thresholds as well? These values should be explicitly specified for reproducibility by potential readers.

- p. 7 l. 1 - what is the slope or the metric with which cloud top is defined? This should also be specified.

- p. 7 l. 5 - "coldest temperature" - temperatures can lower but not colder - suggest rewording here and in other locations in the text.

- p. 7 l. 6-9 - This method of using the inversion base temperature as cloud top temperature may explain some of this study's results, as the assumption becomes less valid in cases where clouds protrude into temperature inversions, which often occur concurrently with stronger mixing, not necessarily down to the surface. I think that in the context of this paper the authors might be able to make their point by defining their current "cloud top temperature" as "minimum cloud temperature", which would also be valid for cloud protruding into an inversion, and would retain the essence of INP activation temperature widely discussed in the text. Also, note that note all polar liquid-bearing clouds are capped by a temperature inversion. See for example Sedlar and Tjernström, 2009; https://link.springer.com/article/10.1007/s10546-009-9407-1, Sedlar et al., 2012; https://journals.ametsoc.org/view/journals/clim/25/7/jcli-d-11-00186.1.xml?tab_body=fulltext-display, Sotiropoulou et al., 2014; https://acp.copernicus.org/articles/14/12573/2014/, Silber et al., 2020; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL087099

- p. 7 l. 12-14 - based on the fact that the authors have used the radar for this seeding cloud proximity criterion, I think that they refer here to overlying hydrometeors rather than overlying clouds. If that is correct I recommend revising the text accordingly.

- p. 9 l. 31 - it could be the vast majority of clouds (> 80%) contain ice but this is certainly not every cloud as currently stated in the text.

- Fig. 3 caption confusion - deltaT should be 5 C below -10 C and vice versa.

- p. 11 l. 13-14 - "The reasons for the increase in ice forming efficiency for low and coupled clouds in the Arctic must be caused..." - while this is likely the case, I think that the authors should tone down this sentence.

- p. 12 l.10-11 - decoupling does not necessarily mean that there is an underlying inversion, but only that the underlying layer is stable. I suggest revising the text accordingly.

- p. 12 l. 11-13 - clouds largely act to destabilize the polar atmosphere and not the opposite. Another more likely possibility is that once the marine aerosols are mixed aloft, the atmosphere becomes decoupled as a result of radiative cooling of the surrounding ice surfaces.

- p. 12 l. 16 - add "as" before "such"

- p. 12 l. 23 - define beta

- p. 12 l.13 - a reduction in beta is generally seen throughout the atmospheric profile regardless of the decoupling height (and sometimes increases above the decoupling height such as in the green and blue curves), so I find this argument by the authors to be rather subjective.

- Fig. 6 - what do the normalized 0 and 2 values represent?

- p. 13 l.1 - Temperature units are missing.

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