Reply to reviewer #1

We thank the reviewer for their careful reading of the manuscript and the valuable comments and suggestions. Our responses are provided below, with reviewer comments in black and our replies in blue. All line references refer to the revised manuscript.

This manuscript examines parts of the annual cycle of cloud temperatures and the relative occurrence of ice during the MOSAiC expedition using bimonthly partitioning. The authors combine remote sensing (PollyXT, KAZR) with surface-based INP measurements to associate ice occurrence with primary ice nucleation and long-range INP transport, while dissecting the data by cloud-coupling state. I think the results showing differences in the ice occurrence fraction between coupled and decoupled cases are mostly robust. However, I have major concerns about parts of the methodology: the writing is mediocre (many sentences are difficult to understand), the literature review is lacking, and some references to the literature are inaccurate, leading to misleading statements and undermining the analysis's credibility.

Specific comments:

- Methodology and Inaccurate references for methodological approaches:
    - IWC detectability threshold: In l. 178-179, it is stated that ice detectability by the lidar is determined "by considering the lowest detectable IWC from a lidar of 10-6 kg m-3 (Buhl et al., 2016)." I do not know where the authors found this threshold. Looking at Buhl et al., 2016, IWC values of 10-8 kg m-3 and lower are clearly seen in Fig. 7. (the case of smaller ice corresponding to smaller reflectivity contours), but perhaps there's something I've missed.

Indeed, in Bühl et al. (2016) no such a threshold was introduced. Actually, it was referred to the wrong study from Bühl here. It should have been the lidar radar comparison study published in 2013 (Bühl et al., 2013).

- Volume depolarization threshold of 0.03 for ice: based on l. 178, looking at Griesche et al. (2021), I do not see any derivation of the 0.03 threshold, other than a very similar quote, which is honestly misleading. It is true that, in theory, very small depolarization values are expected for liquids, but this requires consistent calibration and very accurate corrections and does not account for the occurrence of drizzle (larger non-spherical drops resulting in slightly higher depolarization - see earlier works of Sassen and Platt). A slight deviation in the deadtime correction, for example, could result in high depolarization around the cloud base region and, hence, a false positive for ice. I know the PollyXT is a wonderful instrument, but given the harsh conditions of that remote deployment, I doubt that assumption holds throughout the expedition, though I could be wrong.

To derive a lidar volume depolarization threshold for ice detection, a dependency of the IWC and the volume depolarization was derived. The extinction coefficient α was calculated using the IWC-Z-T and the α-Z-T relationships from Hogan et al. (2006) with Z as cloud radar reflectivity and T as temperature. The extinction coefficient was converted to particle backscatter coefficient by applying a lidar ratio of 30 sr (Ansmann et al., 1992). Finally, from the particle backscatter coefficient, a molecular extinction coefficient derived following Eltermann (1968) and Teillet (1990), a molecular lidar ratio of $8\pi/3$, and an approximated molecular volume depolarization ratio of 0.01 (Biele et al., 2000), the volume depolarization ratio was calculated based on Freundenthaler et al. (2009). This information was added in more detail to the appendix of the manuscript.

Throughout the whole MOSAiC campaign, a lidar expert from TROPOS was aboard Polarstern to ensure high quality lidar data. Additionally, continuous automated absolute calibrations of the depolarization channel are implemented in the measurement cycle of the instrument. Yet, besides all efforts, dead time issues which lead to underestimates of photon counts in high-signal conditions cannot definitely be ruled out. However, as mentioned already in the reviewer statement, such issues would only have an effect at cloud base height. The cloud phase identification was applied only below cloud base, and hence in a height, where no influence of possible dead time effects is to be expected.

- The previous bullet combined with the apparent PollyXT data, which, unless I misinterpreted it, seems off (as demonstrated in Fig. 3):
  - Backscatter units appear closer to sr-1 m-1 than sr-1 mM-1, though if that is the case, I'd expect values closer to 1e-3 around the liquid cloud peak return (e.g., Thorsen and Fu, 2015, https://doi.org/10.1175/JTECH-D-14-00178.1). Given the Raman capabilities of the PollyXT, I would expect to see the derived backscatter cross-section field, which is more informative than the attenuated backscatter.
  - Thank you for this feedback. Indeed, the units are misrepresented in Fig. 3. The correct unit is, as expected by the reviewer, sr-1 m-1. The figure was corrected.
  - When it comes to Raman retrievals, we have to note that more than half of the year, the PollyXT was operated under daylight conditions (as shown in Fig. 2 of the manuscript), which prevent the application of Raman analysis. Also, the presented case study in Fig. 3 of the manuscript was observed when the sun was already 24 h a day above the horizon.
  - Depolarization field: missing depolarization values in-cloud, above cloud base but below the backscatter peak, are shown in Fig. 3b. Those are quite concerning, as I would have expected the cross-polar signal to be strong enough to generate a robust signal. If the NaN (or inf) values were the result of near-zero cross-pol values, then I would not have expected the depolarization to start showing again above a certain depth. Are the elevated values above the NaN region the result of a lack of implementation of multiple-scattering corrections? A very low SNR would likely have resulted in noise rather than a consistent pattern. The problematic depolarization signal around cloud base makes me very uncomfortable with the depolarization-only determination of ice, let alone the extremely small threshold of 0.03. This makes me think that the ice periods are overestimated throughout the analysis period (the radar analysis towards the end, for that matter, might be picking up on some supercooled drizzle, or not - see my penultimate comment of this review).
  - The receiver saturation under these optical thick low-cloud conditions were indeed an issue with the PollyXT measurements. However, as mentioned in the answer to the second specific comment of the reviewer, we are confident, that these issues do not affect the applied cloud phase identification, as this was applied not in the cloud base

height area. Not only the saturation effects can cause problems here, but also multiple scattering effects, which increase the volume depolarization and could have caused a misclassification of the liquid-dominated layer as ice occurrence. The derivation of the depolarization threshold is summarized in the answer to the second specific comment and outlined in detail in the appendix of the manuscript.

- Trajectory analysis: This analysis is prone to errors when considering single trajectories (no ensembles), and results are very inconclusive, giving the feeling of "cherry picking":
  - The authors describe in l. 216-218 the use of 10-day back-trajectories. I doubt that 10-day back trajectories are robust, especially over the polar regions. I believe that a small shift of a few kilometers, or even hundreds of meters, in the starting coordinates could result in major offsets. See the recent literature on the topic, e.g., from ARM.
  - Note: we have changed our wording from travel time to residence time as this is the more commonly used term. Yet, the meaning did not change.
  - The trajectory analysis was recomputed for an ensemble of 27 trajectories for each starting point. Figure 1 (Fig. 10 in the manuscript) shows the mean of all trajectories as the solid line, which agrees well with the single trajectory analysis presented in the initial submission. Additionally, the results based on each single trajectory of the ensemble are shown by the dashed lines in Figure 1. Only little fluctuation from the mean was observed for the single trajectory ensemble members. The temperature regimes, were the main conclusions where drawn from the plot (i.e., above -10 °C for Figure 1a and below -15 °C for Figure 1b) are well covered in terms of observational hours and hence here results from the signal trajectories agree very well with the results averaged over all trajectories.

- Since we generally used the trajectories only up to 4 days (96 h) anyway (except for the overview plot), we have switched to 5-days back-trajectories (see lines 224-229).

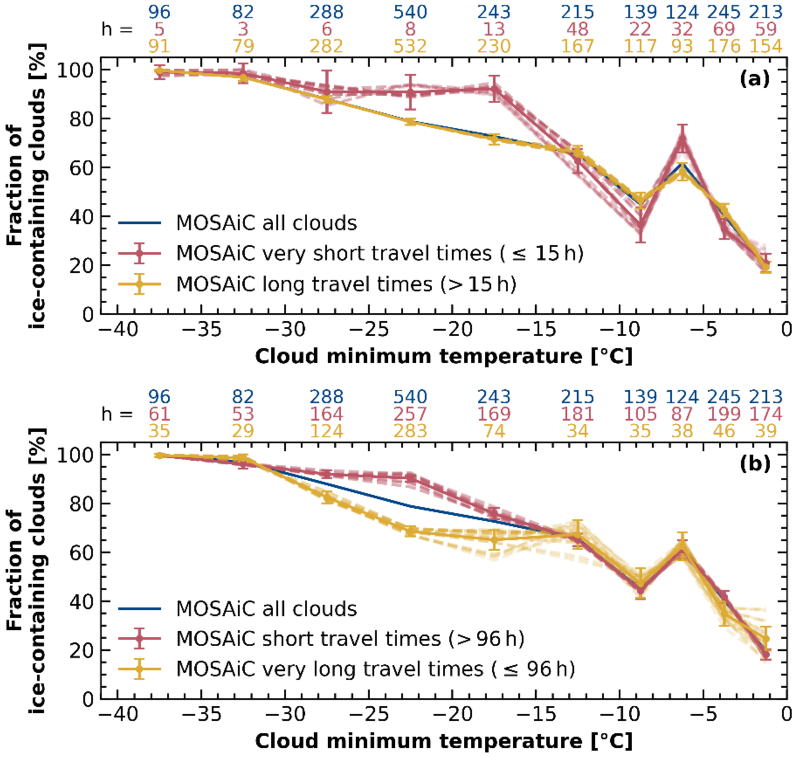

Figure 1: Fraction of ice-containing clouds identified based on the lidar observations and separated by trajectory residence time over sea ice. Upper panel: residence times less than 15 h (red line), residence times more than 15 hours (yellow line) and all clouds (blue line). Lower panel: residence times less than 96 h (red line), residence times more than 96 hours (yellow line) and all clouds (blue line). The dashed lines depict results based on single trajectories, the solid line shows the mean results from all trajectories.

- "weakly coupled" (l. 335-337): I am not familiar with such a "weakly coupled" category. Looking at Silber and Shupe (2022), I do not see any reference to such a "weakly coupled" category. I do see that they used an equivalent ratio value of 5 and not 2, as used here, and their justification was the near-surface uncertainty in ERA5 (model output) over the polar regions, whereas here, real observations over the PolarStern are used, so I do not see a proper justification for this category.
- The idea of this analysis was to derive some evidence for effects the cloud state prior observation could have on the observed cloud state. Yet, the derived conclusions ('weakly coupled' clouds in August + September were more frequently observed as ice-containing than 'strong decoupled' clouds) can be considered as rather weak, due to the mentioned methodological weaknesses and additionally, the low data coverage. We have therefore decided to remove this analysis from the study. Instead, a paragraph in the discussion was added touching weaker mixing processes and the history of the cloud, which cannot be assessed by our observations (see lines 426 - 431).
- The trajectory analysis results (Fig. 9 and related discussion): Fig. 9a appear inconclusive when considering the ice occurrence fluctuations vs. temperature (as opposed to Fig. 5 and mostly Fig. 8, where coupled cases consistently have higher ice fractions), and could be merely the

result of confounders (e.g., time of year and icebreaker position integrated in Fig. 5). Because of this inconsistency, I tend to doubt the results in Fig. 9b, though the explanation in the text does make some sense. That said, note that the number of samples in Fig. 9b are significantly less balanced at T > -15 C, which could strongly influence the apparent agreement albeit the travel time partitioning.

- From Fig. 5 in the manuscript, it is obvious that the conclusion drawn from Fig. 10a in the manuscript can be limited to a period between April and September (no clouds at T > -10°C were observed before April). Figure 2 shows the locations of the trajectories when reaching the ice edge. The corresponding days, as indicated in the legend, are evenly distributed from April to September. Also, the positions of Polarstern as well as the locations of ice-edge crossings are scattered over a wide area. With this shown and the distinct difference of 13 % of fraction of ice-containing clouds at -7.5 °C > T > -5 °C separated at 15 h residence time we believe that this is not merely the effect of confounders. However, likely more effects are playing a role here and we have wakened our conclusive statement (see lines 372 - 378).
- For Fig. 10b (Fig. 9a in the initial submission) of the manuscript, we agree that the data coverage the T > -15 °C is rather imbalanced. We have therefore removed the conclusion that the respective results are an indication for a resupply of INPs from the surface. The data coverage and balance below -15 °C, however, is pretty solid. Also, given that very little effect of the trajectory ensemble analysis can be seen here, we believe that the observed results are an effect of the trajectory residence times.

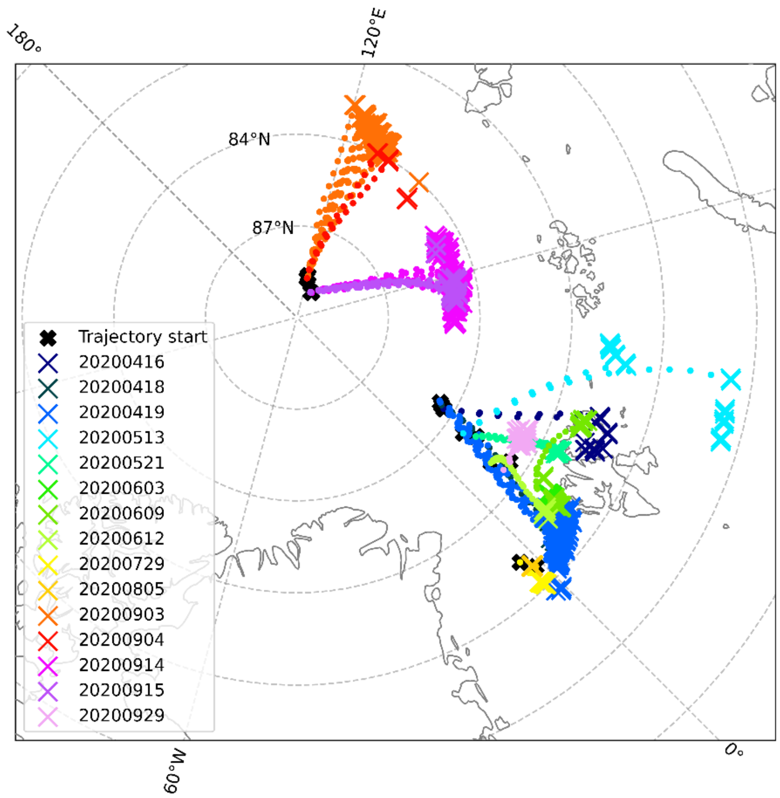

Figure 2: Colored crosses depict the locations of the trajectories when reaching the ice edge. Only trajectories with residence times less than 15 h and which correspond to ice-containing clouds observed at -7.5 °C > T > -5 °C are shown. The black

crosses show the location of the Polarstern at the time of the cloud observation and the dotted lines indicated the trajectory. The location at the ice edge is shown for each trajectory of the ensemble, however the trajectory itself is only shown for one, respectively, due to clarity reasons.

- Also, given the results in Fig. 10, how would this figure (9) look if you apply the trajectory partitioning on the radar-based definition?
- The results based on the cloud radar data are shown in Figure 3.The separation at shorter residence times (15 h) follows a similar pattern as for the lidar-approach shown in Figure 1. At temperatures between -7.5 °C and -5°C more ice-containing clouds were observed when the trajectories reached the ice edge in less than 15 h. In the next colder temperature bin (-10 °C > T > -7.5 °C) the effect was inverted, i.e. more ice-containing clouds for longer residence times.
- For the separation at longer residence times (96 h) there were more ice-containing clouds observed at temperatures between -10 °C and -7.5 °C that needed longer than 96 h to reach the ice edge.
- Note, however, that these effects were observed at fraction of ice-containing clouds larger than 85%, i.e. the relative difference is very small (< 5%).

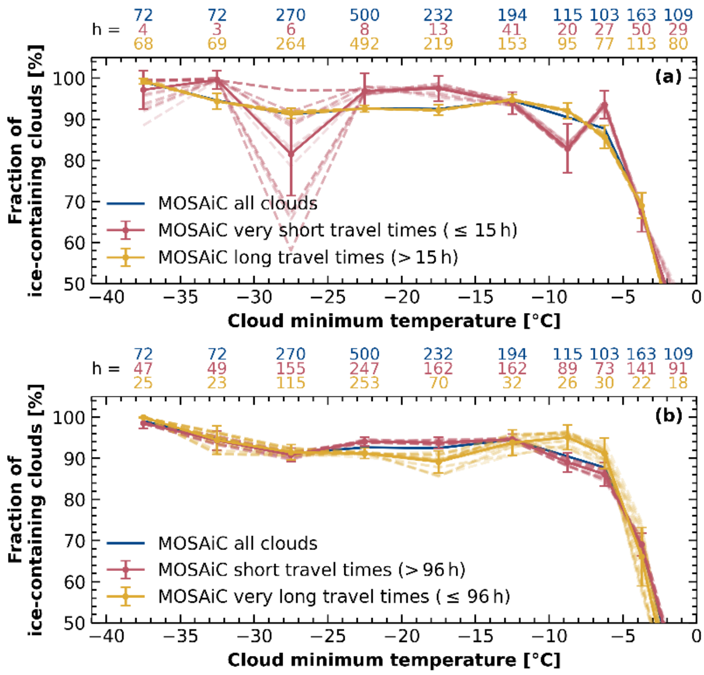

Figure 3: Fraction of ice-containing clouds identified based on the cloud radar observations and separated by trajectory residence times over sea ice. Upper panel: residence times less than 15 h (red line), residence times more than 15 h (yellow line) and all clouds (blue line). Lower panel: residence times less than 96 h (red line), residence times more than 96 h (yellow line) and all clouds (blue line). The dashed lines depict results based on single trajectories, the solid line shows the mean results from all trajectories.

- Overeach statements, for example:
  - INPs vs. trajectory time in l. 244-246 - Eyeballing Fig. 2, this correlation appears coincidental and not more. There are times with an apparent sharp increase in INPC commensurate (or not) with trajectory travel times. More rigorous analysis is required to support this claim and conclusion.

  - We agree that a more sophisticated analysis should be done to support this statement. However, since this is not in the scope of this study, we have removed the respective statement.
  - INP concentration change discussed in l. 247-251: I do not think that more than a 2 orders-of-magnitude increase is "moderate". Moreover, they are at the very least equivalent to the June-July increase when examining the figure in detail.
  - The moderate increase referred to the length of the periods (six months vs two months). In the manuscript it now states ‘increased slowly’ instead of ‘showed only a moderate increase’ (see lines 263 - 264).
  - Conclusions, specifically l. 478-480 (see my final comment below).

- Writing – the text contains many sentences that are difficult to understand (some examples: l. 21-22, l. 290-291), the introduction lacks a clear storyline (the intro often reads like a collection of anecdotes on GW effects, etc.), there’s a frequent change of tense within paragraphs (e.g., l. 31-43), and many typos are found throughout the text (e.g., “during a Arctic”; l. 2, a repeating term “my means" in l. 161 and 175 – what does it mean?). The Introduction’s literature survey is lacking references to older works, giving the impression that the vast majority of findings are from the last 4-6 years. What about the extensive works of Tjernström, J. Curry, and more on ACI, Arctic atmosphere's thermodynamic profile, coupling state, etc.? I understand the focus on MOSAiC findings, but Arctic science existed before (SHEBA, etc.).
  - More results from previous research was added to the introduction. We thank the reviewer for the provided suggestions.
  - Review 2 also suggested changes in the introduction. Based on the comments of reviewer 1 and 2 the introduction was comprehensively revised.

- l. 14 - "along the back-trajectories" - which back trajectories? More information is needed here. Trajectories were not mentioned until now.
  - ➢ Done.

- l. 23. - "initiated by an INP" - you mean "initiated by INPs"?
  - ➢ Certainly, it was corrected

- The continuous reference to INPs as singular throughout the text results in incorrect English when referring to "an INP". At some point in the text this changes and INPs are referred to in the plural form.
  - ➢ The usage of INP and INPs was homogenized in the manuscript

- l. 44 - "INPs of different composition trigger ice formation at different temperatures." - is that accurate? INPs are more likely to activate at different temperatures depending on composition, with the likelihood increasing at lower temperatures, but the text does not reflect that.

- ➢ The respective passage was revised based on the suggested changes. It reads now as: “The probability of INP activation depends on its composition and temperature, increasing with decreasing temperature.” (see line 31)

- l. 71 - "which is capped by a temperature inversion" - while this is typically true, temperature inversions are NOT ALWAYS the case (see earlier work mentioned above), or do the authors have a specific definition for a temperature inversion?
  - ➢ We have relaxed the statement at bit, to include cases where no temperature inversion is present. It now reads as: ‘…which is usually capped by a temperature inversion, most of the time located at cloud top…’ (see line 60).

- l. 83 - "decoupled from the WVT" - you mean decoupled from the sea ice leads?
  - ➢ Not exactly. It was referred to coupling to the water vapor transport from sea ice leads. A few more information on the methodology of the cited study was added (lines 80-84). See Saavedra Garfias et al., 2023 (https://doi.org/10.5194/acp-23-14521-2023) for more details.

- l. 112 - define HYSPLIT and provide reference (likely Stein et al.). This is provided later in the text but should be stated here.
  - ➢ Done

- Fig. 1 - provide lat/lon information (what latitude does the inner circle denote? Some ticks, at the very least, are missing for longitude).
  - ➢ Done

- l. 133 - "another remote-sensing site" - you mean remote sensing facility? Or perhaps a remote sensing platform?
  - ➢ Changed site to platform.

- Table 1: Some of the information appears to be misleading: Can't the volume depolarization ratio exceed 0.5? What about KAZR Ze? can't it exceed +20 dBZ? I doubt that is the case.
  - ➢ These statements were not quite correct (both values can exceed the mentioned range). Since they are not relevant for this study, we removed this information.

- Table 2 - Equation for decoupling height is inconsistent with the text description (l. 189-191).
    - Indeed, the parentheses was not set correctly.

- l. 150. was --> were
    - Corrected.

- l. 170 - 1e-5 sr-1 mM-1? So you use a threshold of 1e-8 sr-1 km-1 for liquid? I doubt it. Perhaps 1e-2 sr-1 km -1?
    - A threshold of 1e-5 sr-1 km-1 was applied. This was corrected in the text.

- l. 271-273 - again, I could be missing something here, but the results in Buhl et al. support the detection of ice throughout the depicted period, considering the reflectivity values.
    - As mentioned above, the wrong Bühl reference was cited here. Bühl et al., (2013) is the correct reference.

- The observations in Fig. 3 seem somewhat off:
    - panel a - backscatter units (see major comment above)
    - panel b - Missing depolarization values in-cloud (see major comment above)
    - panel c - why is the cloud base not shown here as well? It is critical here for case evaluation. Also, see my other comment concerning ice detection.
    - panel d - I agree with your deduction of liquid only between 19-23 UTC. It all appears to indicate drizzle below cloud base, but why does Cloudnet classify it as ice? Is it purely a detectable echo at temperatures below 0 °C?
    - Also, no letters for panels
    - Also, the cloud base curve should be thinner (or smoothed) to enable lidar data evaluation of more pixels

    - The Figure was revised as requested. Regarding the Cloudnet ice classification: Cloudnet classifies ice if two conditions are fulfilled: temperature below freezing point and a falling pixel is identified by the cloud radar. This information was also added to the manuscript, as requested by reviewer #2 (see lines 145 - 149).

- l. 282 - In general, is the fraction of ice-containing clouds an accurate term? I mean, what happens in multi-layer cases? Perhaps you mean the fraction of ice-containing clouds among the lowest liquid clouds, or a similar definition? I think you already refer to this entire analysis as focusing on the lowest liquid clouds, but only towards the very end of the manuscript. This should be stated earlier, much closer to the beginning (and perhaps even in the abstract)

- We have introduced the applied definition of the term fraction of ice-containing clouds in the methods section (Sec. 3.3., lines 205 - 209).

- l. 293 - define how the uncertainty is calculated? Based on Seifert et al., I presume this is simply the standard error, which may or may not represent the _actual_ uncertainty. Given that the definition is rather simple, why not simply provide it "in full"?
  - It is the standard error. This was added in lines 166 – 170.

- l. 318 onward - until now the discussion was about coupled-decoupled but now it is in terms of free-tropospheric clouds. I recommend choosing a fixed terminology and using it throughout the text.
  - The term free-tropospheric cloud was introduced at this point to distinguish the clouds analyzed in this study, with those from Jimenez et al. (2025). This was clarified in the manuscript (see lines 338 - 340).

- l. 350 - without INPs active at T > -15 C - so no INPs whatsoever, or just very small values? Based on Fig. 2d, and as one would expect, there are always INPs, even if at very small concentrations.
  - In cases which were classified as "without INPs active at T > -15 °C" really only filters were use where no INPs active above -15 °C were found. These filters likely have a lower limit and there may have been INPs simply not identified on the filters.

- Also, no clouds were observed at all or do you mean ice-containing clouds were not observed?

  - This was misleading explained. Only at T > -7.5 °C no clouds were observed on days where no INPs active above -15 °C were found on the filter samples. See Figure 4. However, this statement was removed from the manuscript, as the analysis now focuses on the contrast between higher and low INP concentration (see lines 355 – 366).

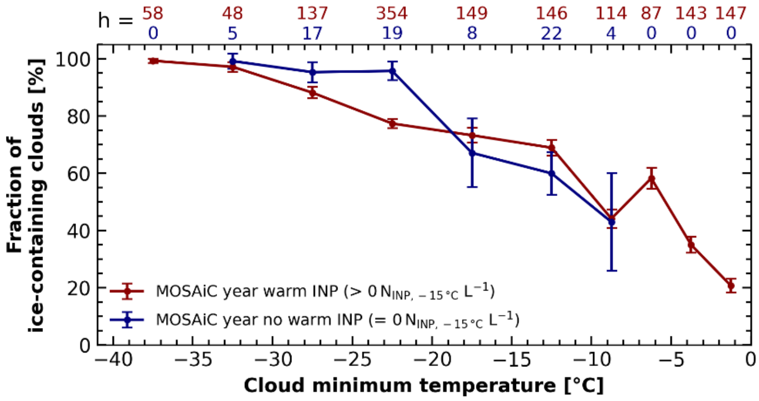

Figure 4: Fraction of ice-containing clouds during MOSAiC separated for INP availability at the surface. The red line shows clouds observed during days, with INPs active at T > -15 °C measured on the filter samples. The blue line shows clouds during days without such INPs found on the filters.

- l. 394-395 and Table 3 - EDR in log10 of what units? Specify.
  - ➢ The EDR are in units of $m^2\ s^{-3}$. This was added to the manuscript (see caption of Table 3).

- l. 405-409 - I agree that it is more likely to detect sporadic ice crystals with a Ka-band radar than with a lidar, but how did you determine the radar echoes are necessarily precipitating ice and not supercooled drizzle for example?
  - ➢ The focus of this study were the low-level clouds. These low-level clouds are, as outlined in the manuscript, difficult to detect by a cloud radar. Hence, the lidar based approach was developed. Developing another cloud radar-based approach was outside of the scope of this study. Therefore, the radar-based analysis was solely based on the Cloudnet target classification. Despite some weaknesses in the algorithm (as for example also indicated by the reviewer when discussing Fig. 3 of the manuscript), Cloudnet is well established and a common tool in the cloud community.

- Conclusions: in l.478-480 - This is indeed an apparent connection, but only an associative one. Recommend stating "associative" since without appropriate modeling accounting for these processes, one can suggest this connection, but not state that it is proven. Tone down.
  - ➢ Done.

Ansmann, A., Riebesell, M., Wandinger, U., Weitkamp, C., Voss, E., Lahmann, W., and Michaelis, W.: Combined raman elastic-backscatter LIDAR for vertical profiling of moisture, aerosol extinction, backscatter, and LIDAR ratio, Applied Physics B, 55, 18–28, https://doi.org/10.1007/BF00348608, 1992.

Biele, J., Beyerle, G., and Baumgarten, G.: Polarization lidar: Corrections of instrumental effects, Opt. Express, 7, 427–435, https://doi.org/10.1364/OE.7.000427, 2000.

Bühl, J., Ansmann, A., Seifert, P., Baars, H., and Engelmann, R.: Toward a quantitative characterization of heterogeneous ice formation with lidar/radar: Comparison of CALIPSO/CloudSat with ground-based observations, Geophys. Res. Lett., 40, 4404–4408, https://doi.org/10.1002/grl.50792, 2013.

Elterman, L.: UV, Visible, and IR Attenuation for Altitudes to 50 Km, 1968, Environmental research papers, Air Force Cambridge Research Laboratories, Office of Aerospace Research, United States Air Force, 1968.

Freudenthaler, V., Esselborn, M., Wiegner, M., Heese, B., Tesche, M., Ansmann, A., MüLLER, D., Althausen, D., Wirth, M., Fix, A., Ehret,G., Knippertz, P., Toledano, C., Gasteiger, J., Garhammer, M., and Seefeldner, M.: Depolarization ratio profiling at several wavelengths in pure Saharan dust during SAMUM 2006, Tellus B: Chemical and Physical Meteorology, 61, 165–179, https://doi.org/10.1111/j.1600-0889.2008.00396.x, 2009.

Hogan, R. J., Mittermaier, M. P., and Illingworth, A. J.: The Retrieval of Ice Water Content from Radar Reflectivity Factor and Temperature and Its Use in Evaluating a Mesoscale Model, Journal of Applied Meteorology and Climatology, 45, 301–317, https://doi.org/10.1175/JAM2340.1, 2006.

Jimenez, C., Ansmann, A., Ohneiser, K., Griesche, H., Engelmann, R., Radenz, M., Hofer, J., Althausen, D., Knopf, D. A., Dahlke, S., Bühl, J., Baars, H., Seifert, P., and Wandinger, U.: MOSAiC studies of long-lasting mixed-phase cloud events and analysis of the liquid-phase properties of Arctic clouds, Atmospheric Chemistry and Physics, https://doi.org/10.5194/acp-25-12955-2025, 2025.

Saavedra Garfias, P., Kalesse-Los, H., von Albedyll, L., Griesche, H., and Spreen, G.: Asymmetries in cloud microphysical properties ascribed to sea ice leads via water vapour transport in the central Arctic, Atmospheric Chemistry and Physics, 23, 14 521–14 546, https://doi.org/10.5194/acp-23-14521-2023, 2023.

Teillet, P. M.: Rayleigh optical depth comparisons from various sources, Appl. Opt., 29, 1897–1900, https://doi.org/10.1364/AO.29.001897, 1990.

Reply to reviewer #2

We thank the reviewer for their careful reading of the manuscript and the valuable comments and suggestions. Our responses are provided below, with reviewer comments in black and our replies in blue. All line references refer to the revised manuscript.

Review of "Annual cycle of surface-coupling effects on Arctic mixed-phase clouds during MOSAiC" by Griesche et al., for publication in Atmospheric Chemistry and Physics

Summary

This manuscript provides a highly detailed analysis of surface coupling on cloud properties during the MOSAiC campaign. The introduction is highly detailed though some spots can be shortened without taking away from the overall message. Another strength of the manuscript is the high number of recent (2020-2025) references – clearly demonstrating that the authors are current on the science surrounding this topic, and after checking each of those references, it is clear the authors have established a novel research idea and approach for the present study. A strength of this manuscript is the quality of the figures and tables. Each figure is very clear and easy to read, while supporting relevant key results or discussion points in the text. The core result of the paper is convincing and robust: It's very clear from the results that observed liquid clouds are very frequently associated with surface coupling, while many ice containing clouds are from decoupled states. INPs have some seasonality with a peak in Summer and likely explain some observed cases where coupled clouds contain more ice in T > -15C cases. The authors also take care to acknowledge limitations of their work such as, for example, realizing that clouds decoupled from the surface may have previously been coupled before, and that partitioning by time and coupling state would have yielded inconclusive results due to the limited number of samples for each bin. While I think the key scientific findings are novel and robust, the writing and communication of the results was cumbersome in some sections of the manuscript. I made many suggestions in the specific comments already, but I think this manuscript could be shortened by at least ~5% in length while still conveying all of the key findings accurately and concisely. The reduction in text may also be helpful for the additional figures I've suggested adding to the text – namely 1-2 to provide additional detail and support for results on the trajectory analysis, and an additional figure partitioning Figure 8 into "lowest vs. highest" INP states for each of the coupled vs. decoupled states to reveal any INP sensitivity (or lack thereof) to the coupling state.

Overall, I think this will make an excellent contribution to Atmospheric Chemistry and Physics given the clear fundamental difference in observed cloud properties as a function of surface coupling, and the novel use of INPs to further explain the occurrence of observed ice in coupled vs. decouple states. However, I believe this manuscript needs a major revision first to expand core details around some of the analysis (methods) techniques, which could be addressed through some additional figure suggestions below, as well as improve the writing of the manuscript for conciseness and clarity (I have made many specific comments below).

General Comments

1. Paragraphs 1 and 2 in the introduction contain a lot of good background information discussing why mixed-phase clouds are persistent, the processes by which mixed-

phase cloud particles exist, and some discussion of the seasonality of Arctic cloud properties. I think these two paragraphs, however, could be reorganized somewhat to discuss surface-atmosphere coupling much earlier, and how resulting processes are tied to surface coupling.

- ➢ The introduction was restructured based on the suggested improvements. Also, the surface-atmosphere coupling is now mentioned already in the first paragraph.

2. Section 2 would benefit from having multiple subsections to organize the descriptions of the various datasets (e.g., (A) OCEANET, (B) INP Data, (C) Radiosonde Data).
   - ➢ The methodology section was subdivided into “ground-based remote sensing”, “INP data”, and “radiosonde profiling”.
3. Section 3.1 of the text was a bit hard to follow. The authors refer to Jimenez et al. (2020) as the source of the method, but it’s not clear how or why thresholds or values are determined (e.g., why “δ should therefore not exceed a value of 0.03”). This section could benefit from additional detail and perhaps could be organized better by adding a list of (say) 3-5 bullet points clearly outlining the lidar-based algorithm.
   - ➢ To derive a lidar volume depolarization threshold for ice detection, a dependency of the IWC and the volume depolarization was derived. The extinction coefficient α was calculated using the IWC-Z-T and the α-Z-T relationships from Hogan et al. (2006) with Z as cloud radar reflectivity and T as temperature. The extinction coefficient was converted to particle backscatter coefficient by applying a lidar ratio of 30 sr (Ansmann et al., 1992). Finally, from the particle backscatter coefficient, a molecular extinction coefficient derived following Eltermann (1968) and Teillet (1990), a molecular lidar ratio of 8π/3, and an approximated molecular volume depolarization ratio of 0.01 (Biele et al., 2000), the volume depolarization ratio was calculated based on Freundenthaler et al. (2009). This information was added in more detail to the appendix of the manuscript.
4. Trajectory analysis is one of the key analysis methods but lacks description in the methods. An example figure with details on, for example, typical altitudes of the liquid base height, how HYSPLIT was initialized, and if an ensemble of points around the MOSAiC site was used. Even for small areas (say, 2x2 km) the origin of parcels can come from a very wide area of the Arctic – this detail is critical for the overall interpretation of the stated results, especially for ensuring that a 1-2 km horizontal distance initiation offset of HYSPLIT doesn’t result in a parcel trajectory that’s 50-100 km or more away from the original parcel’s origin point for the same amount of time. I think adding a figure or 2 into the results showing the HYSPLIT results would be very beneficial.
   - ➢ Note: we have changed our wording from travel time to residence time as this is the more commonly used term.
   - ➢ As requested by reviewer #1, we moved from a single trajectory analysis to an ensemble analysis, which was made clear in the text (see lines 224 - 229). Additionally, a figure illustrating the trajectory analysis for the case study was added (Figure 1, Fig. 4 in the manuscript). This figure shows the back-trajectories for the cloud depicted in Fig. 3 in the manuscript. Note, while an ensemble of 27 trajectories was initialized every hour, only one trajectory of the ensemble and only for every second hour is shown for clarity reasons. The respective location of each trajectory of the ensemble at the ice edge is marked in the same color as the trajectory. If the residence time over sea ice was less than 15 h the location where the trajectory hit the ice edge was marked with a cross, when the residence time was more than 15 h it was marked with a circle.

The location where the single trajectory ensemble members meet the ice edge can vary and hence the derived residence time. For example, some of the trajectories initialized on 18 April 2020 20 UTC (shown in purple) reached the ice edge in less than 15 h (indicated by the purple crosses), while others needed more than 15 h (indicated by the purple circles).

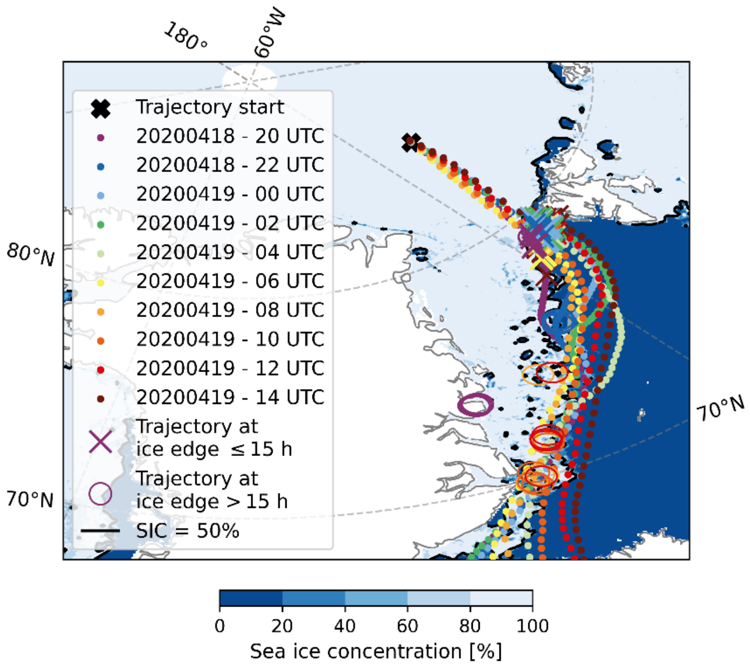

Figure 1: Back-trajectories for the cloud observed between 18 April 2020 19 UTC and 19 April 2020 15 UTC. For each member of the ensemble the location where the trajectory reached the ice edge is marked with a cross or a circle. The background shows the sea ice concentration on 18 April 2020.

5. Lead and melt pond fraction are frequently referenced in the results, however, it's unclear to me how significant this detail is with respect to more obvious analysis points (namely the role of sea-ice concentration on the results). In principle the idea of why they are important make sense (especially in the cited references), but I think the authors need to make a more convincing argument why lead and melt fraction is significant to the conclusions drawn. Can a figure be created partitioning the INP results based on very low lead and/or melt fraction vs. characteristically high lead and/or melt fraction (with statistical significance testing)?
   - The lead + melt pond fraction data coverage is roughly half of the MOSAiC year. Note that some of the melt pond data points shown in Fig. 2c had to be removed. On these days the melt pond fraction was derived from very few pixels within the 100 km radius around Polarstern. A minimum coverage of 10% of valid pixels in the analyzed area was set for the melt pond data. This information was added to the manuscript in lines 241 - 242. The limited lead + melt pond and INP filter data coverage prohibited a combined analysis on the cloud properties. However, Figure 2 correlates the lead + melt pond fraction with the INP concentration (INPC) at different temperatures and gives the respective correlation coefficient. A decent correlation was found between lead + melt pond fraction and INPC, especially for INPs active between -10 and -25 °C (R values between 0.45 and 0.52). This indicates an influence of leads and melt ponds on INPC, as was highlighted already by previous studies (e.g.,

Hartmann et al., 2020, Creamean et al., 2022). Together with the shown impact of INP availability on the fraction of ice-containing clouds this indicates also the importance of lead and melt ponds on cloud properties.

- Note also, SIC data are generally too coarse to capture smaller melt ponds or leads, as these features require a higher spatial resolution. Additionally, the sensors used for SIC retrievals (passive microwave) are designed to detect open water and may misclassify melt ponds. Melt ponds are detected using optical sensors and leads are identified through SAR or infrared sensors.

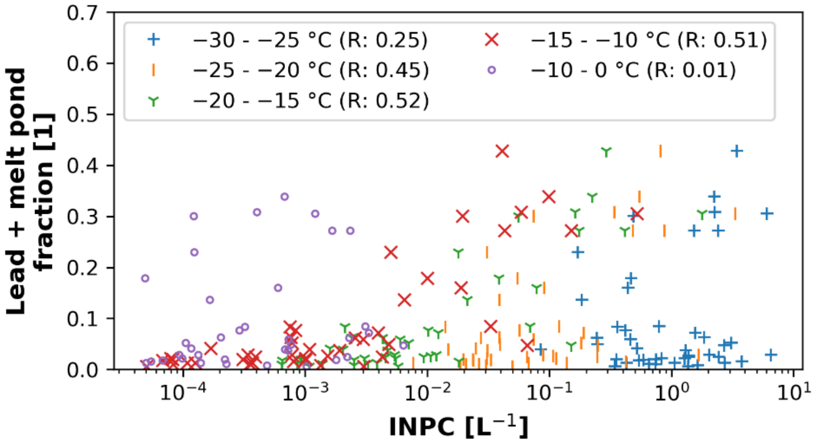

Figure 2: Lead and melt pond fraction against INPC active between -30 °C and 0 °C. The different colors and markers indicate different temperature intervals. The correlation coefficient R between the lead and melt pond fraction and INPC and the respective temperature interval is given in the legend.

Specific Comments

L2: “an Arctic summer cruise”. Also, the sentence starting with “During an Arctic summer cruise…” from L2-4 in the abstract seems to come out of left field, and I’m not sure this motivational detail is needed here.

- We removed the respective part.

L6: comma needed after “March” and “September”

- Added.

L8: comma needed after “July”

- Added.

L44: For this paragraph, I’d include 1-2 sentences tying the importance of INP measurements to surface coupling (for example: to the audience not familiar with INPs, are certain INPs more likely to be sourced from the surface than the free troposphere?).

- The introduction was restructured as suggested in one of the general comments. Now, the different sources of INPs are discussed already in the second paragraph.

L70: Do you mean “deeper” instead of “higher”?

- Indeed.

L110: Comma needed after “radar”.

- Added.

L116-117: INP filter and trajectory discussion is quite central to your analysis, hence, I think calling it “supporting information” undermines its importance. You could just say “Additionally, methods centered around the use of INP measurements, air parcel trajectories, and sea-ice concentration are discussed.”.

- This sentence was revised according to the suggestions. Thank you.

L120: “… aboard the Polarstern…”

- Corrected.

Figure 1: Is it necessary to state that the map was created with PyGMT? Unless it was adapted from another manuscript, this detail may be unnecessary.

- This citation was added to give credit to the PyGMT developers.

L137-139: It would be useful to state somewhere in here what size INPs can be collected by these filters.

- For the INP sampling, 0.2 µm pore polycarbonate filters were used. Based on theoretical collection efficiencies (Spurny and Lodge, 1972), it is assumed that the total suspended particulates were collected. Also, the collection efficiency varies with size of the collected particles and has its minimum at about 0.1 µm (about 80\%). This information is now also given in the manuscript (see lines 153 - 155).

L139: The way this is written, it sounds like the expedition took place at Colorado State University. Unless you meant to say “the filters were analyzed after the expedition at Colorado State University”?

- This is what was meant here. We changed the wording to the proposed suggestion.

L147: This is a fairly important detail. 1-2 more sentences to describe the Cloudnet target classification would be helpful. Or, state here that the Cloudnet algorithm will be described in more detail in the next (Methodology) section.

- A bit more context to the Cloudnet target classification was given.

L153: “introduced in the following and all…” did you mean to say “following paragraphs”? or something else?

- The following paragraphs or rather the following subsections was meant. This was clarified.

L160-162: Suggested rewrite: "The lidar, due to its sensitivity to the number of particles in a sample volume, was primarily used for the identification of liquid-dominated layers. The procedure for detecting liquid-dominated layers follows Jimenez et al. (2020), which relied on normalized attenuated backscatter.

- Done, thank you for the suggestion.

L167: Just say "… profiles were used to avoid misclassification of backscatter signals…"

- Changed as suggested.

L169: What is the significance of the 0.03 value for d?

- As mentioned in the reply to general comment 3, the derivation of the volume depolarization threshold was added to the appendix of the manuscript.

L173-174: This is a pretty important detail that should come near the beginning of the paragraph (screening for liquid near the lidar to see if the profile should be analyzed).

- This information was moved to the beginning of the paragraph.

L175: If you take my suggestion for L160-162, I might suggest moving any info as to why you didn't use the cloud radar to the end of this paragraph.

- Done.

L187: I'd say "…derived from the closest radiosonde profile within 6 hours of the observed cloud profile." And then eliminate the next sentence.

- Changed according to the suggestion.

L199: consider saying "… the clouds were further analyzed based on their coupling state."

- Done.

Section 3.4: I'd reword the title slightly to "INP concentration, parcel trajectory analysis, and surface properties" and further sub-divide this section into an (A), (B) and (C) for INP, trajectory analysis and surface properties subsections respectively.

- A subsection was added in the methodology to introduce the EDR retrieval as suggested by the reviewer in a later comment. Therefore, the subsection "3.4 INP concentration, trajectories, and surface properties" was divided into single subsections covering the INP concentration, the parcel trajectory analysis, and the surface properties.

L216-218: The trajectory analysis description needs much more detail. An example figure would be great to add here as well.

- Added, see answer to general comment 4.

L219: no comma needed after (SIC).

- Removed.

Results section: After reading this, I think the first 4 paragraphs could go under a new Section 4.1 titled “Campaign overview of surface conditions, INP measurements and Sea-ice concentration during MOSAiC”

- The respective subsection was added here.

L229: “An overview of atmospheric and surface properties at the Polarstern site during MOSAiC is shown in Fig. 2.”. Also, you can eliminate the sentence stating “Depicted are different parameters…”.

- Changed according the suggestion.

L239: Is Dada et al. (2022) referring to the 1st, 2nd or 3rd WAI event?

- The 3rd. This was clarified in the text.

L247: It would be helpful to the casual reader to quickly describe (perhaps 1 sentence) what characteristic INP values are and what they represent (e.g., is 5 x 10^{-4} L a large amount? What’s considered high versus low?)

- The maximum INP concentration active at -15 °C measured during MOSAiC was above 1 L^{-1} (during early July, see Fig. 2 c of the manuscript). Barry et al. (2025) put the INP concentrations during MOSAiC into context with the measurements at Zeppelin station on Ny-Ålesund and found an overall agreement between both sites. The largest differences of INPs active at -15 °C were found when the highest concentrations were measured at MOSAiC during late June and early July. During this period the respective INPC at Zeppelin station were about one order of magnitude lower, however Polarstern was about 450 km away from Svalbard. Other recent studies reported similar peak summer INPC active at -15 °C of values between 10^{-2} and 10^{-1} INP L^{-1} (e.g., Creamean et al., 2018, Wex et al., 2019, and Hartmann et al., 2021). Central Arctic mid-winter time INP observations are sparse. Only measurements from land-based stations, as reported in Wex et al. (2019) or late-winter measurements (Hartmann et al., 2020) are published. The reported INPC minima in these studies were around $3x10^{-4}$ INP L^{-1}. This information was added to the manuscript (see lines 267 - 272).

L287-288: This is an oddly worded sentence. What does “detected ice more frequent than periods were observed” mean?

- It was referred to the fact, that based on the lidar approach periods were classified as ice-containing, while the cloud radar reflectivity threshold was not reached (e.g., on 20200419 between 0 and 2 UTC). This was clarified in the manuscript (see lines 295 - 298).

L292: “… for each respective temperature interval…”

- Changed.

L299: what is "The respective signal" referring to?

- The fraction of ice-containing clouds. This was changed in the manuscript.

Figure 8: I certainly understand and agree with why you cannot do a combined temporal vs coupling state analysis as in Figure 5, but could you potentially remake a version of this figure showing, for each coupling state, the coupled vs. decoupled states for the top 30% of INP concentrations vs. bottom 30% of INP concentrations? Doing a figure in this way might reveal the sensitivity (or lack thereof) of INPs on the coupling state, even though you'd be eliminating 40% of the data as I've proposed here.

- Thank you for this idea. Actually, using the lower 30% of the measured INPC at -15 °C as threshold ($6x10^{-4}$ INP L^{-1}) to separate the data set highlights already the sensitivity of the clouds to INP availability at the surface (see Figure 3). An increased fraction of ice-containing clouds with a cloud minimum temperature above -15 °C under coupled situations and when the INP concentration at T > -15 °C was greater than $6x10^{-4}$ INP L^{-1} was derived (red). Lower INPC (blue colors) or decoupled cloud situations (orange) showed similar and rather low fraction of ice-containing clouds at temperatures above -15 °C. The lowest fraction of ice-containing clouds was actually derived for clouds decoupled from the surface and with low INPC (light blue). This indicates the influence of local INPs, likely of marine origin, on cloud ice-formation in coupled low-level clouds. The respective figure in the manuscript was changed to Figure 3 (see Fig 9 in the manuscript).

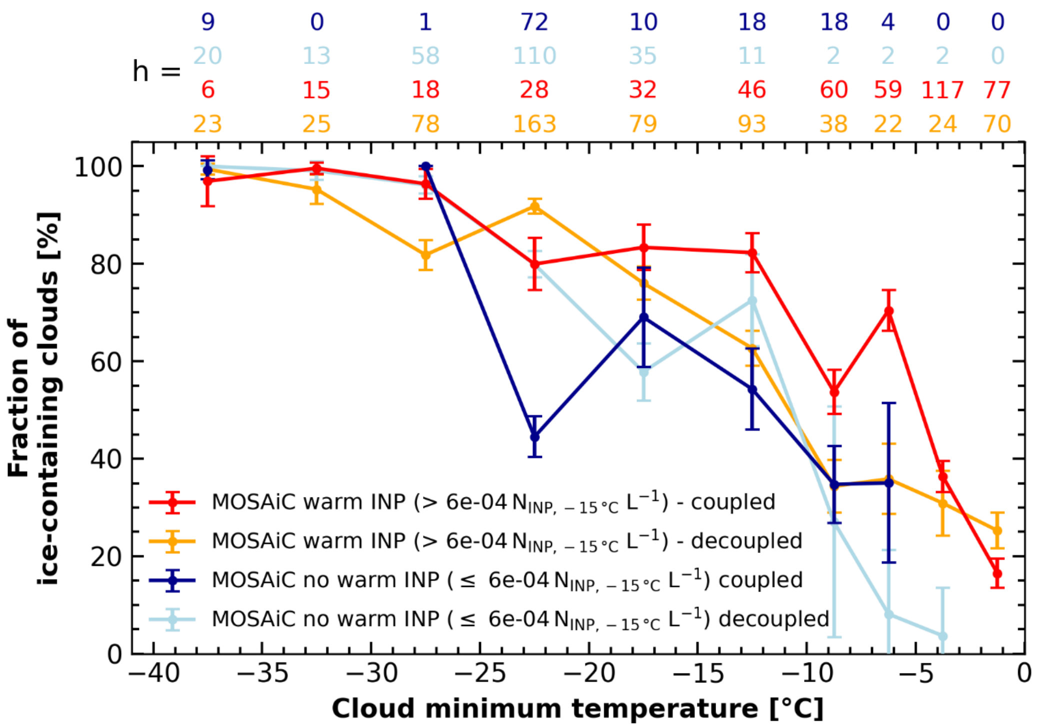

Figure 3: Fraction of ice-containing clouds as a function of cloud minimum temperature separated by INP concentration from surface-based filter samples. As threshold the 30th percentile of the INPC at T = -15 °C was used (= $6x10^{-4}$ INP L^{-1}). The data was further separated into coupled and decoupled clouds. The numbers above the plot highlight the respective analyzed hours of cloud observation.

L354: “too sparse”

- Corrected.

L383: “Another limiting factor was…”

- Corrected.

L389-401: This is a very interesting result, but the Discussion section is not the right place to introduce this point. Move this to the results section, and add a subsection to the Methods section describing the EDR data and how it’s derived.

- A subsection in the methods describing the EDR approach was added (Section 3.7, lines 243 - 249). Also, the paragraph on the EDR results was moved to the results section (Section 4.4, lines 385 - 398).

L448: Change “are also” to “include”

- Changed.

L479: “… could yet be quantified.”

- Changed.

L480: “field campaigns”

- Changed.

L482-483: “… have a different cloud radiative effect.”

- Changed.

Ansmann, A., Riebesell, M., Wandinger, U., Weitkamp, C., Voss, E., Lahmann, W., and Michaelis, W.: Combined raman elastic-backscatter LIDAR for vertical profiling of moisture, aerosol extinction, backscatter, and LIDAR ratio, Applied Physics B, 55, 18–28, https://doi.org/10.1007/BF00348608, 1992.

Barry, K. R., Hill, T. C. J., Kreidenweis, S. M., DeMott, P. J., Tobo, Y., and Creamean, J. M.: Bioaerosols as indicators of central Arctic ice nucleating particle sources, Atmospheric Chemistry and Physics, 25, 11 919–11 933, https://doi.org/10.5194/acp-25-11919-2025, 2025.

Biele, J., Beyerle, G., and Baumgarten, G.: Polarization lidar: Corrections of instrumental effects, Opt. Express, 7, 427–435, https://doi.org/10.1364/OE.7.000427, 2000.

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, Atmospheric Chemistry and Physics, 18, 18 023–18 042, https://doi.org/10.5194/acp-18-18023-2018, 2018.

Creamean, J. M., Barry, K., Hill, T. C. J., Hume, C., DeMott, P. J., Shupe, M. D., Dahlke, S., Willmes, S., Schmale, J., Beck, I., Hoppe, C. J. M., Fong, A., Chamberlain, E., Bowman, J., Scharien, R., and Persson, O.: Annual cycle observations of aerosols capable of ice formation in central Arctic clouds, Nature Communications, 13, https://doi.org/10.1038/s41467-022-31182-x, 2022.

Elterman, L.: UV, Visible, and IR Attenuation for Altitudes to 50 Km, 1968, Environmental research papers, Air Force Cambridge Research Laboratories, Office of Aerospace Research, United States Air Force, 1968.

Freudenthaler, V., Esselborn, M., Wiegner, M., Heese, B., Tesche, M., Ansmann, A., MüLLER, D., Althausen, D., Wirth, M., Fix, A., Ehret,G., Knippertz, P., Toledano, C., Gasteiger, J., Garhammer, M., and Seefeldner, M.: Depolarization ratio profiling at several wavelengths in pure Saharan dust during SAMUM 2006, Tellus B: Chemical and Physical Meteorology, 61, 165–179, https://doi.org/10.1111/j.1600-0889.2008.00396.x, 2009.

Hartmann, M., Adachi, K., Eppers, O., Haas, C., Herber, A., Holzinger, R., Hünerbein, A., Jäkel, E., Jentzsch, C., van Pinxteren, M., Wex, H., Willmes, S., and Stratmann, F.: Wintertime Airborne Measurements of Ice Nucleating Particles in the High Arctic: A Hint to a Marine, Biogenic Source for Ice Nucleating Particles, Geophysical Research Letters, 47, e2020GL087 770, https://doi.org/10.1029/2020GL087770, 2020.

Hartmann, M., Gong, X., Kecorius, S., van Pinxteren, M., Vogl, T., Welti, A., Wex, H., Zeppenfeld, S., Herrmann, H., Wiedensohler, A., and Stratmann, F.: Terrestrial or marine – indications towards the origin of ice-nucleating particles during melt season in the European Arctic up to 83.7°N, Atmospheric Chemistry and Physics, 21, 11 613–11 636, https://doi.org/10.5194/acp-21-11613-2021, 2021.

Hogan, R. J., Mittermaier, M. P., and Illingworth, A. J.: The Retrieval of Ice Water Content from Radar Reflectivity Factor and Temperature and Its Use in Evaluating a Mesoscale Model, Journal of Applied Meteorology and Climatology, 45, 301–317, https://doi.org/10.1175/JAM2340.1, 2006.

Spurny, K. and Lodge, J.: Collection Efficiency Tables for Membrane Filters Used in the Sampling and Analysis of Aerosols and Hydrosols, Tech. rep., https://doi.org/10.5065/D6F769JJ, 1972.

Teillet, P. M.: Rayleigh optical depth comparisons from various sources, Appl. Opt., 29, 1897–1900, https://doi.org/10.1364/AO.29.001897, 1990.

Wex, H., Huang, L., Zhang, W., Hung, H., Traversi, R., Becagli, S., Sheesley, R. J., Moffett, C. E., Barrett, T. E., Bossi, R., Skov, H., Hünerbein, A., Lubitz, J., Löffler, M., Linke, O., Hartmann, M., Herenz, P., and Stratmann, F.: Annual variability of ice-nucleating particle concentrations at different Arctic locations, Atmospheric Chemistry and Physics, 19, 5293–5311, https://doi.org/10.5194/acp-19-5293-2019, 2019.