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 $8\pi/3$, 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.
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).

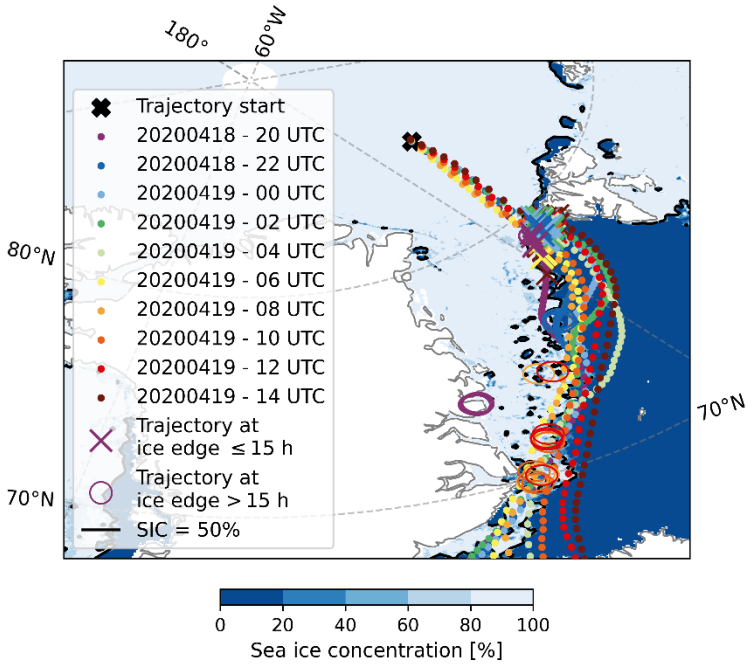

Figure 1: Back-trajectories for the cloud observed between 18 April 2020 19 UTC and 19 April 2020 15 UTC. For each member of the ensemble the location where the trajectory reached the ice edge is marked with a cross or a circle. The background shows the sea ice concentration on 18 April 2020.

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.

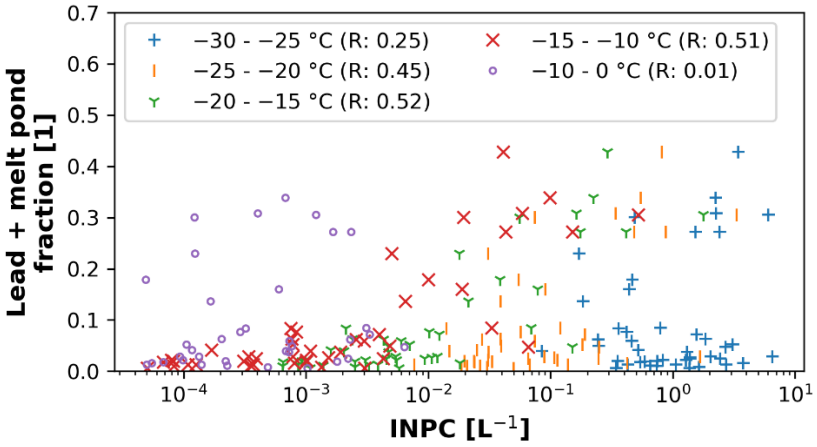

Figure 2: Lead and melt pond fraction against INPC active between -30 °C and 0 °C. The different colors and markers indicate different temperature intervals. The correlation coefficient R between the lead and melt pond fraction and INPC and the respective temperature interval is given in the legend.

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).

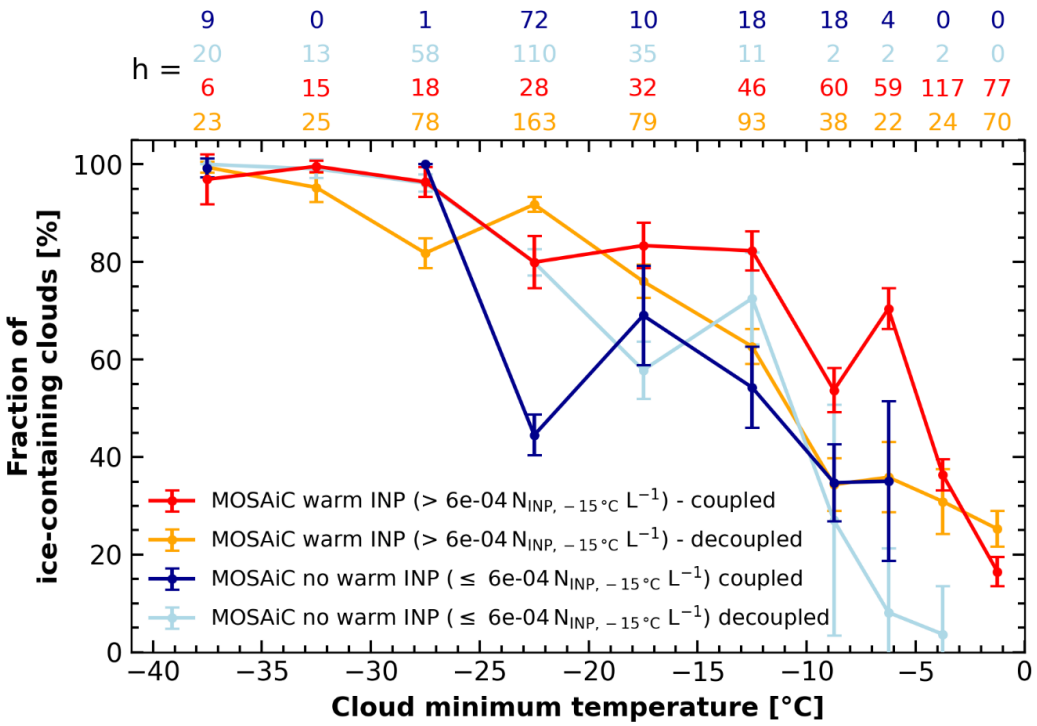

Figure 3: Fraction of ice-containing clouds as a function of cloud minimum temperature separated by INP concentration from surface-based filter samples. As threshold the 30th percentile of the INPC at T = -15 °C was used (= 6x10^{-4} INP L^{-1}). The data was further separated into coupled and decoupled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