RC1: 'Comment on egusphere-2025-5263', Anonymous Referee #1, 24 Nov 2025

This paper presents a wide-ranging exploration of low-level Arctic cloud occurrence and associated processes based on CloudSat-CALIPSO data. The topic is quite important given the role that clouds play in the Arctic system, and the satellite perspective given here is a valuable expansion on past information available for Arctic clouds. While I believe that many of the presented results should be published, this paper is not yet close to being ready for publication. Details are given below, but here is a summary of the key problems:

- There is not enough description of some key aspects, especially regarding the MLR analysis and how some of the parameters are calculated.
- The impact of the 7-day averages is insufficiently explored and discussed. 7-days will include significant transitions in airmasses and clouds that may obscure key results. Other averaging windows should be explored.
- The effect of the blind zone (<0.5km) is not sufficiently addressed. It should be evaluated compared to other observations and understood relative to the MLR analysis. The implication of this blind zone on distinguishing MPC and USLC needs to be explored. Additionally, the language used often suggests that this paper's results represent all low clouds, but an important sub-set of low clouds is missing. The language needs to be clear on this point throughout.
- It appears that the 0.5-3km height range is referenced to "mean sea level" instead of "above ground level," which is a major problem for all observations over land.
- The MLR analysis is not very effective at providing robust and convincing explanations for cloud patterns. Is it not clear which results are "significant" and which are not. Some conclusions that are drawn appear to contradict prior knowledge from the literature. These require more explanation and observational evidence. Additionally, the difference between LTS and MCAO (which are very much related to each other) must be taken into account in any interpretation of stability.
- There are many process-based conclusions drawn that invoke processes that are not actually observed (i.e., entrainment, surface fluxes, cloud persistence, etc.); speculation is ok but should be labelled as such and supported by real results.

I note that this is the first time I have seen this manuscript, but that it is apparently a reworked version of a prior submission. While I do not have access to the prior reviews, based on the authors' own description of what they have changed in the manuscript, it seems that they have only been partially successful in addressing the prior reviewers' concerns.

Overall, during my reading, the paper started out accessible and easy to digest but later became harder to follow, in part because of the vast amount of information and lack of coherent results from the MLR analysis. Based on all of this, the authors could consider splitting the manuscript in two parts: one that deals with the occurrence observations and more fully places them in context of other observations including a detailed assessment of the impact of the blind zone; while the second paper would provide a more clearly described MLR / coupling analysis. Of course, this sounds to be a bit contrary to what reviewers of the prior manuscript suggested (i.e., they apparently recommended pulling information from appendices into the main text). Thus, the authors could also stick with this single paper approach, if they think it is more effective. Either way, major revisions are needed before this manuscript could be considered for publication.

We thank the reviewer for this thorough and constructive assessment of the manuscript. We fully acknowledge that the previous version did not sufficiently address several key methodological and interpretative aspects, in particular regarding the multiple linear regression (MLR) framework, the treatment of temporal averaging, and the implications of observational limitations inherent to the CloudSat–CALIPSO sampling.

In response, we have substantially revised the manuscript. The description of the MLR methodology and of the environmental parameters has been expanded and clarified, and additional diagnostics have been introduced to better assess the robustness and significance of the statistical relationships. The sensitivity to temporal averaging has been explicitly investigated by reducing the averaging window from 7 days to 1 day.

Particular attention has also been given to the effect of the CloudSat blind zone. The analysis has been reformulated to consistently reference occurrences between clutter-height and 3000 m above ground level (AGL) rather than sea level, ensuring physically meaningful cloud occurrence estimates over regions with significant orography. The implications of the blind zone for low-level cloud detection, and in particular for the distinction between MPCs and USLCs, are now more explicitly discussed, and the scope of the results is clearly stated throughout the manuscript to avoid overgeneralization to all low-level clouds.

More generally, the interpretation of the results has been revised to avoid unsupported process-based conclusions. Speculative interpretations are now clearly identified as such and are only discussed where they can be reasonably supported by the observational evidence or by existing literature. The roles of stability-related metrics, including the relationship between LTS and MCAO, are treated in the method.

The manuscript structure has been improved to enhance clarity and readability, with a clearer separation between observational results and their interpretation. We address each of the reviewer's concerns in detail in the specific comments below and believe that these revisions significantly improve the robustness and clarity of the manuscript.

Specific comments.

- Line 30-33: This sentence makes it sound like it is an "overestimation" of the liquid cloud fraction. But I think that is not true. Perhaps it is meant that the liquid cloud fraction is "inaccurate," but this sentence should be clarified so the intent is not left unclear based on the sentence structure.

*Thank you for your comment. We have tried to make this sentence clearer : "**Modeling studies have shown that oversimplified or inaccurate parameterizations of microphysical processes (e.g., ice nucleation and droplet–ice interactions) can***

promote excessive freezing of supercooled droplets at low temperatures, thereby reducing the simulated liquid water content and leading to inaccuracies in cloud phase partitioning (Avramov et al., 2011; Ovchinnikov et al., 2014; Tan and Storelvmo, 2019).*"*

- Line 33-35: This notion has been well established long before Raillard et al.

*We have highlighted older studies in this paragraph : "***Earlier observational and modeling studies have demonstrated that the persistence of supercooled liquid droplets in Arctic mixed-phase clouds is governed by microphysical and dynamical processes rather than by temperature alone (Korolev and Isaac, 2003; Morrison and Pinto, 2005; Shupe et al., 2006; Ovchinnikov et al., 2014).*** *More recently, Raillard et al. (2024) confirmed that the commonly used temperature-dependent phase partitioning in climate model cloud schemes is unable to sustain supercooled liquid water at low temperatures, and therefore fails to accurately simulate Arctic mixed-phase clouds (MPCs)."*

- Line 37-39: This sentence is structurally incomplete. You could add commas after macrophysical and microphysical, which might make it better, or revise otherwise.

*Thank you, we have revised this sentence: "***The life cycle of these low-level clouds results from complex interactions between local microphysical, radiative, dynamical processes, and larger-scale environmental conditions (Morrison et al., 2012; Li et al., 2020a, b; Griesche et al., 2021). Recent developments in cloud microphysics and turbulence parameterizations (Raillard et al., 2024; Vignon et al., 2026), as well as the increasing use of observational constraints and process-oriented model evaluation (Kay et al., 2016; Tan and Storelvmo, 2019), have led to measurable improvements in the representation of Arctic clouds in both regional and global models. Therefore, continued synergy between long-term observations, targeted field campaigns, and model development is essential to further reduce uncertainties in Arctic cloud processes and their radiative effects.****"*

- Line 42-43: The studies in question are not "case studies." They are long-term observations at fixed observatories. One can certainly make the argument that they only represent specific locations, but not that they are case studies.

We thank the reviewer for this clarification. We have revised the wording to replace "case studies" with "long-term observations at fixed observatories" in order to more accurately reflect the nature of these datasets : ***"These long-term observations from fixed observatories have substantially increased our understanding of the physical processes that govern the life cycle of low-level clouds.****"*

- Line 45: "between" should be "among"

Thank you, we have changed the phrasing of this sentence accordingly : "Long-term satellite observations of the regional and seasonal distribution of low-level cloud types are therefore crucial to reduce the spread ***among*** *large-scale models on the annual cycle of the cloud fraction and cloud phase (Lenaerts et al., 2017; Taylor et al., 2019)."*

- Line 55: "peculiar" should be "particular"

*Thank you, we have changed the phrasing of this sentence accordingly: "**This is partly due to the ability of the instruments (lidar and radar) used to detect the particular microphysical structure of low-level MPCs, which generally consist of an upper layer dominated by supercooled water droplets and lower layers containing ice crystals (Shupe et al., 2006; Mc-Farquhar et al., 2007; Mioche et al., 2017; Moser et al., 2023).**"*

- Line 57-58: This comment is for space-based lidar only. Ground-based lidar views from the other side and does not have a problem observing the ice falling out of these clouds.

*We thank the reviewer for this clarification : "Because the **space-based** lidar cannot penetrate most of the optically thick liquid-topped layers, studies relying on the space lidar alone tend to combine real MPCs and liquid-only clouds into a single liquid-containing cloud type."*

- Line 61-63: This whole discussion (the whole paragraph) is based on the satellite perspective and not the ground-based perspective. It should be clarified at the top of the paragraph that this is only relevant for satellite perspectives. Additionally, there are a number of papers that define mixed-phase clouds from the system and process perspective, where even if there is a region of only ice below a region of liquid or mixed-phase it is classified as part of the same mixed-phase cloud system because that is where the ice formed.

*We thank the reviewer for this comment. We have clarified at the beginning of the paragraph: **"From a satellite-based observational perspective**, disagreements persist when comparing basic properties such as the occurrence of the thermodynamic phase of low-level clouds with active instruments. This is partly due to the ability of the instruments (lidar and radar) used to detect the particular microphysical structure of low-level MPCs, which generally consist of an upper layer dominated by supercooled water droplets and lower layers containing ice crystals (Shupe et al., 2006; McFarquhar et al., 2007; Mioche et al., 2017; Moser et al., 2023).**"***

- Line 15-113: Overall, this introduction is an incomplete representation of the background literature on this topic. Importantly, the missing literature discusses some of the points that this paper claims need more attention. I agree that more information is needed on Arctic low-level clouds, and thus this paper is indeed valuable, but the arguments for what this paper achieves relative to past work should be on more solid footing.

We thank the reviewer for this important comment. In the revised manuscript, we have made a particular effort to include key statements highlighting the progress already achieved in the literature on Arctic low-level clouds. Parts of introduction have been adjusted to better acknowledge these advances and to place our work within the broader context of existing studies.

At the same time, we have clarified and more explicitly articulated the specific aspects that remain uncertain and that our study aims to improve, thereby strengthening the positioning and added value of the present work relative to previous research.

- Line 129: Why is this thickness limitation put on supercooled liquid water layers? Is there a justification for it?

This thickness limitation is physically motivated and grounded in previous observational studies. We have added appropriate references to clarify this point. Delanoë and Hogan (2010) and Zhang et al. (2010) describe methodologies to identify supercooled liquid water layers using active remote sensing and show that these layers are typically physically thin, with vertical extents generally not exceeding about 300 m. Such layers are predominantly observed at temperatures between 0 °C and −40 °C (Hogan et al., 2004; Zhang et al., 2010). This physically based constraint was subsequently incorporated into the DARDAR cloud classification algorithm by Ceccaldi et al. (2013) to ensure consistency with observed cloud structures and to reduce misclassification between liquid and ice layers : "Supercooled liquid water pixels are defined by subfreezing temperatures (-40 °C < T < 0 °C), along with strong lidar attenuation, and for a layer thickness lower than 360 m (Hogan et al., 2004; Zhang et al., 2010)."

- Line 130-132: Does this statement mean no lidar attenuation and/or no radar detection signal? Or does it mean no lidar attenuation and/or yes radar detection signal? The language is unclear.

*We thank the reviewer for this comment. We have clarified the wording to explicitly state that weak lidar attenuation and the presence of a radar signal are not mutually exclusive and may occur simultaneously in ice-only cloud conditions : "**All remaining cloudy pixels at below-freezing temperatures are classified as ice-only when the lidar backscatter at 532 nm is weak (β_{532} < $2.10^{-5} m^{-1} sr^{-1}$), indicating no strong lidar attenuation, or when a radar detection signal is measured (CloudSat radar mask values greater than 30, indicating cloud detection); these conditions may occur simultaneously and are both indicative of ice-only clouds.**"*

- Line 134-143: It is good that this source of underestimation is acknowledged, however, at no point in the paper is the implication of this blind zone on the results discussed. There are many Arctic clouds that occur below 500m, as clearly shown by numerous aircraft- and ground-based observations. This seems like an important limitation that warrants further context and a detailed explanation of how it impacts the basic results/conclusions of the study. There should be an independent evaluation of these results based on, for example, ground-based observations at specific sites, to provide the reader with the anticipated underestimation of cloud occurrence due to this height limitation. Additionally, it is noted here that the blind zone for CloudSat is dependent upon surface type, yet I believe that the 500m lower limit is used everywhere. How does this impact the results over land, where the blind zone is known to be deeper ? Lastly, this statement about 500m being "a good compromise" between two factors is problematic. It should not really be about compromise but should instead be about ensuring that the classifications are accurate. The compromise language makes it sound like the trade off is between adding more data at lower levels while also allowing some contamination by ground clutter influences. In my mind that is not a good compromise because it just adds bad data into the data set in order to extend it to lower altitudes. Is that the intended compromise?

We agree with the reviewer that the results of the present study will be strengthened with an evaluation of the impact of the blind zone on cloud occurrences.

First, in this revised version of the manuscript, DARDAR data have been reprocessed taking into account: 1) the height above ground level (and not anymore above sea level) and 2) the variability of the height of the blind zone (and not anymore a fixed threshold of 500 m). Details about this new processing and changes compared to previous results are presented in the answer to the reviewer's comment below.

In order to evaluate the impact of the blind zone, comparisons with ground-based data from the Cloudnet network have been performed. Cloudnet observations benefit from multi remote sensing instruments (radar, lidar, see detailed description of data here: https://cloudnet.fmi.fi/), and provide a "Classification product" including different cloud types (see Hogan and O'Connor (2004) and https://cloudnet.fmi.fi/product/classification for details), comparable to those from DARDAR used in our study. The Cloudnet Classification products from Ny-Alesund (Norway, 78.923°N, 11.922°E) and Hyytiälä (Finland, 61.844°N, 24.287°E) Arctic sites have been used because they are the only ones, in the Arctic region, that are available when CALIPSO and CloudSat were operational (June to December 2016 at Ny Alesund, and March to August 2014 at Hyttiala). Thus, we can compare time-synchronized data.

To ensure representative occurrences comparable to ground-based observations, a square of 5° of latitude by 5° of longitude (± 2.5° in both latitude and longitude), centered over each ground site location has been chosen to calculate occurrences from DARDAR data.

Results are shown on the two figures below. Figure 1 presents the results for the Ny-Alesund site, and Figure 2 for the Hyttiala site. On each figure, occurrences of total (a), cold (b), warm (c), ice (d), mixed-phase (e) and cold liquid containing (f) clouds in the altitude range up to 3000 m above ground are presented. Weekly (blue colors) and monthly (green colors) occurrences have been computed. Ground-based occurrences are displayed in thick straight lines. Occurrences from DARDAR observations are presented for the old (dotted lines) and the new (thin straight lines) processings.

We note that the last cloud type (CLCC for cold liquid containing clouds) has been computed only for this evaluation work. Indeed, the supercooled liquid class is not included in the Cloudnet classification product. So, the USLC class could not be directly evaluated. Instead, and to be coherent with the Cloudnet product, the cold liquid containing clouds including the USLC or MPC cloud types have been computed from DARDAR data.

As expected, since the real clutter altitude is sometimes greater than the 500 m previous threshold (see details below), the new DARDAR processing presents lower occurrences values than the old processing, but not systematically, and not for all cloud types.

		Cloud type					
		Total	Warm	Cold	Ice	MPC	CLCC
Ny Alesund	Average	0,3	1,3	-2,1	5,2	0,1	-5,8
	σ	16,5	4,5	16,0	23,6	9,4	7,6
Hyttiala	Average	0,2	5,3	-1,7	-2,9	3,4	-2,3
	σ	8,4	5,5	10,8	8,1	7,1	8,2
Total	Average	0,2	3,3	-1,9	1,1	1,7	-4,0
	σ	12,6	5,2	13,1	17,5	8,2	7,8

Table 1: Averages and standard deviations of the differences between ground-based and space-based cloud occurrences for Ny Alesund site, Hyytiala site and both.

A good level of consistency is generally observed when comparing ground-based and space-based occurrences. The weekly and monthly variabilities are very similar on both observations for the two observation sites. Differences are observed in terms of values of occurrences, in particular on the weekly occurrences. These differences can come from the blind zone which may affect the space-based products. This could impact the occurrences with systematic higher occurrences from ground. But they also may be due to the representativity of the 5x5° box compared to the site location. In order to quantify the differences, we calculated their average (for the monthly occurrences only) in the table above.

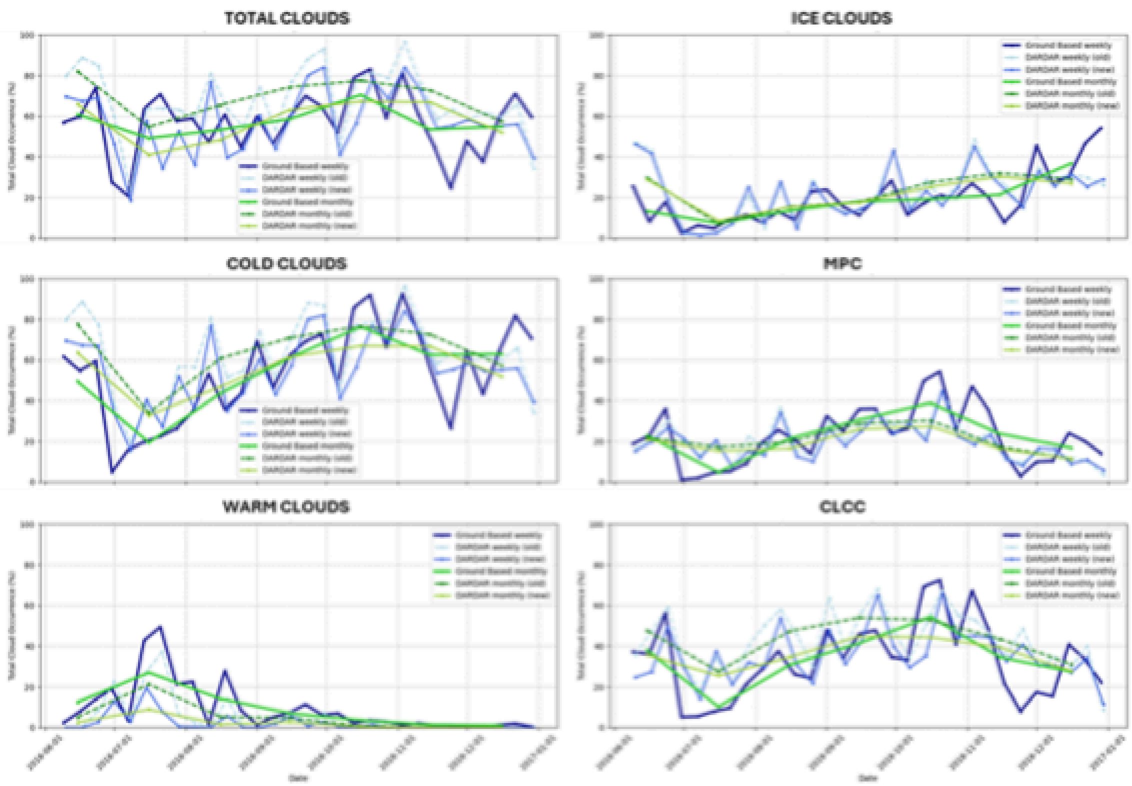

Figure 1: Comparison of DARDAR occurrences with ground-based site at Ny Alesund

The averaged differences remain very small, with only a few percent, and especially well distributed around zero (from -5.8% to +5.2%, as seen also in the figures), meaning that no bias is present, thus no under or overestimation of occurrences seems to be made. The standard deviations show non-negligible values (up to 23 %). So, the main source of uncertainties comes probably more from the spatial representativity of the data (comparison of 5x5° box to one location), than from the blind zone.

In conclusion, the cloud occurrences presented in this paper have been evaluated from ground based observations at Ny Alesund and Hyttiala sites to estimate the effect of the blind zone near the ground on space observations. Time synchronized data and similar cloud products have been compared. The results show a good agreement between the two datasets, with some differences, but no systematic bias. This leads to the conclusion that the impact of the ground clutter on the low altitude cloud occurrences from DARDAR presented in the paper remains limited.

In the revised manuscript, we propose to show these results in a new section (2.2 Comparison with ground-based data). Only the figure corresponding to the Ny-Alesund site will be kept, the Hyttiala site results will be added in a supplement material.

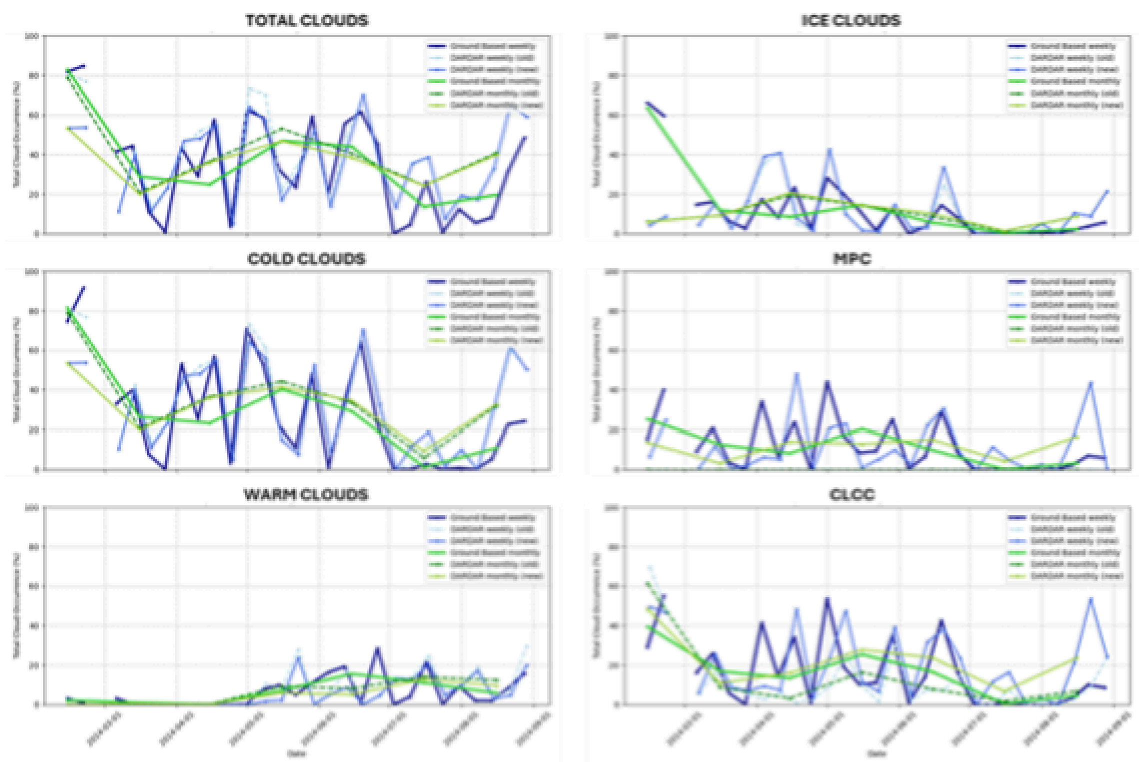

Figure 2: Comparison of DARDAR occurrences with ground-based site at Hyytiala

- Line 157: This says 4 categories, but then the list gives 5. The last 3 are subsets of the 2nd. So somehow the language around this should be adjusted.

We thank the reviewer for pointing out this inconsistency. We have clarified the classification by distinguishing between broad thermodynamic categories (warm and cold clouds) and phase-based subcategories within the cold-cloud class:

*"**Accordingly, low-level clouds are first separated into two broad thermodynamic categories: warm clouds (T > 0 °C) and cold clouds (T ≤ 0 °C). Within the cold-cloud category, three phase-based subcategories are further distinguished: ice clouds, unglaciated supercooled liquid clouds (USLCs), and mixed-phase clouds (MPCs), which are described below** :*

- *Warm clouds: consisting exclusively of liquid water droplets at T > 0 °C.*
- *Cold clouds: including all cloud types with temperatures below 0 °C, i.e., all clouds except warm clouds.*
- *Ice clouds: composed entirely of pixels of ice crystals.*
- *Unglaciated supercooled liquid clouds (USLCs): composed solely of supercooled liquid water droplet pixels.*
- *Mixed-phase clouds (MPCs): characterized by a mixture of pixels of different phases (i.e., pixels of liquid water droplets and pixels of ice crystals and/or droplets and crystals in the same pixel)."*

- Line 162: The USLC class is emphasized in this paper as being an important new topic. First off, I do not agree that it is a new focus, as liquid-only clouds have been discussed in the literature on Arctic clouds since the 1970s. But, beyond that point there are some significant uncertainties with identifying this cloud type and distinguishing it from others. My list of comments/questions include:
    - There is no indication in this manuscript of how CloudSat observations are used to determine if there is ice precipitating out of low-level, supercooled liquid-containing clouds. Given that ice production in these clouds is often very weak, how does the long range gate of CloudSat impact its ability to identify falling ice? What is the reflectivity threshold used to identify ice from other types of hydrometeors?

CloudSat observations are used to identify precipitating ice through the radar reflectivity signal and the associated CloudSat radar mask provided by the CloudSat Data Processing Center. The CloudSat radar mask algorithm is described in detail by Marchand et al. (2008). Mask values range from 0 to 40 and provide an estimate of the confidence in radar detection, with values above 30 corresponding to confident cloud detection and a false detection rate lower than 2 % (Marchand et al., 2008). Following the methodology of Ceccaldi et al. (2013), the presence of ice crystals is inferred when such confident radar detections are present (mask values > 30).

This approach does not rely on a fixed reflectivity threshold in dBZ to discriminate ice from other hydrometeors, but instead combines radar detection confidence with thermodynamic and lidar constraints within the DARDAR framework. Due to the long-range radar gate of CloudSat, very weak ice precipitation, which is often characteristic of low-level supercooled liquid-containing clouds, may remain undetected.

CloudSat observations do not allow a robust discrimination between snow and cloud ice in the absence of Doppler velocity measurements. Consequently, within the DARDAR framework, radar-detected frozen hydrometeors are grouped into a single "ice" class. In principle, information from vertical gradients of radar reflectivity could provide insight into the nature of frozen hydrometeors; however, such an approach is highly challenging given the coarse vertical resolution of CloudSat (~500 m), which limits the robustness of gradient-based diagnostics. As a result, no explicit threshold is applied to distinguish between snow and cloud ice, and all radar-detected frozen particles are classified as ice.

- The ice in statiform mixed-phase clouds is typically at the bottom, because it falls out of the liquid layers. These measurements are unable to observe the lowest heights of the atmosphere where that ice would be. Based on robust ground-based observations, there are many Arctic clouds with a top around 500-700m with ice falling out that would be in the blind zone of these measurements (and thus erroneously identified as USLC). What is the impact of these cases on the results? This would be a one-way issue where USLC is overestimated at the expense of MPC.

We thank the reviewer for this very relevant comment. We acknowledge that the ice phase in stratiform mixed-phase clouds is typically located near the liquid cloud base. Consequently, observations that are unable to sample the lowest atmospheric levels may miss this ice phase, potentially leading to an overestimation of unglaciated supercooled liquid clouds (USLCs) at the expense of mixed-phase clouds (MPCs).

To address this limitation, the latest version of our processing code no longer relies on a fixed lower-altitude threshold (e.g., 500 m), but instead uses the radar clutter height to define the lowest observable level for each profile.

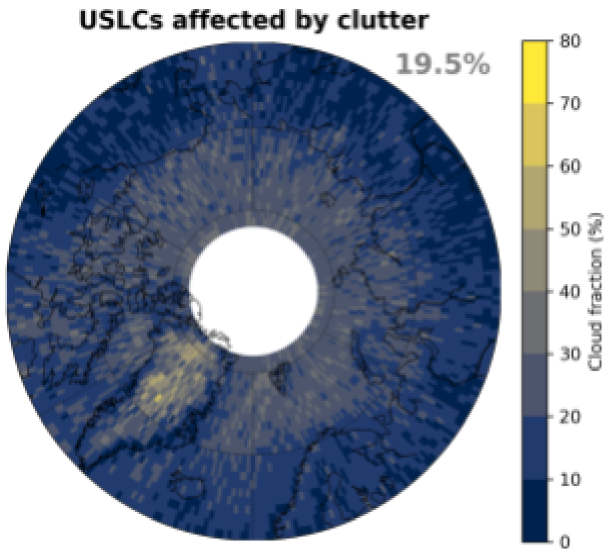

Figure 3: USLCs fraction affected by surface clutter

However, since the lower part of some USLCs may still be undetected when they intersect the radar clutter, we further performed dedicated statistics to distinguish between USLCs that are in contact with the clutter and those that are not. USLCs touching the clutter may therefore include cases where a precipitating ice phase exists below the observable range, and thus potentially correspond to MPCs with an undetected ice component.

Figure 3 shows that approximately 20 % of the identified USLCs intersect the radar clutter. These cases are therefore unlikely to be affected by undetected low-level ice precipitation, indicating that the potential overestimation of USLCs due to this effect remains limited.

- For any of these clouds, it has not been clarified how liquid-phase precipitation factors into the classifications. Presumably most of the liquid phase precipitation from shallow clouds would be in the "warm clouds" category. However, there is certainly drizzle that also occurs in mildly supercooled clouds. This notion is also physically consistent: If there are indeed such a high fraction of USLC as suggested here, this must mean that there are simply not enough INPs to form ice. It is well known that there are also not many CCN in the Arctic (apart from pollution events), meaning the formation of large drops and thus drizzle formation. The challenge here is that drizzle and falling ice look similar from a radar perspective. Some comment on this issue is needed.

It should be noted that liquid-phase precipitation is not explicitly distinguished in the present cloud phase classification. In shallow warm clouds, liquid precipitation is therefore implicitly included in the warm-cloud category. However, drizzle can also occur in mildly supercooled clouds, particularly under Arctic conditions characterized by low concentrations of both ice-nucleating particles (INPs) and cloud condensation nuclei (CCN). Such environments favor the persistence of supercooled liquid water and the formation of relatively large droplets, making drizzle formation physically plausible even in the absence of ice. From a remote-sensing perspective, this introduces an inherent ambiguity, as drizzle-sized liquid drops and falling ice crystals can produce similar radar reflectivity signatures. As a result, some liquid precipitation may be misclassified as ice in radar-based detection, especially when lidar information is attenuated. This limitation is associated with spaceborne active sensors and is discussed in the Methods section.

*We propose an additional paragraph to discuss these uncertainties associated with the detection of USLCs: "**Due to the CloudSat CPR's coarse vertical resolution, very weak ice precipitation, which is commonly observed in stratiform mixed-phase clouds, may remain undetected. Furthermore, without Doppler velocity measurements, the cloud radar cannot reliably distinguish between cloud ice and snow. Consequently, all radar-detected frozen hydrometeors fall into a single ice category within the DARDAR classification framework. Additionally, the lowest observable altitude is determined by the changing height of the radar clutter. Ice precipitation occurring entirely below this level cannot be detected, which could lead to an overestimation of the occurrence of USLCs. To assess this effect, we distinguish between USLCs that overlap the height of the clutter and those that do not. As the majority of identified USLCs (80 %) do not overlap with the clutter, the potential bias associated with undetected low-level ice precipitation is expected to remain limited (see Figure S3 in the Supplement). Furthermore, although liquid-phase precipitation is not explicitly included in the classification, it is implicitly incorporated into the warm-cloud category. Drizzle can also occur in supercooled liquid containing clouds. Drizzle-sized liquid drops and falling ice crystals are expected to produce similar radar reflectivity signatures. Therefore, ambiguity in phase attribution is inherent when relying on spaceborne active sensors, particularly under conditions of lidar attenuation.**"*

- Line 166-169: In one sentence it is stated that the occurrence of MPCs can be "overestimated" compared to others. Then in the next statement it is noted that these issues lead to "uncertainties." An overestimate is a bias not an uncertainty. It is important to be clear with true sources of uncertainty (i.e., could go either way) versus bias (only goes one way) as these are important for the interpretation of the results.

Thank you, we have corrected the sentence: "We showed that these cloud-type classification issues lead to systematic ***biases*** *in cloud occurrence of about 15 % for ice, 20 % for liquid, and 10 % for mixed-phase clouds (see Supporting Information of Bazantay et al., 2024)."*

- Line 181: I do not see where ~3% comes from in the Supplemental material. I do see a reference to 5% and some other numbers. How do you arrive at this estimate of 3%?

Yes, absolutely, it's a mistake: "This limitation introduces a seasonal bias in cloud climatologies, with a slight overestimation of winter occurrences ***(≈ 5%)****, especially over oceans (Noel et al., 2018)."*

- Line 190-192: LTS, EIS, and MCAO are all similar measures, just defined at different heights. What is the meaningful difference among these for this application?

We agree with the reviewer that LTS, EIS, and MCAO are closely related metrics, as they all describe aspects of lower-tropospheric thermal stratification. However, they are not redundant for the present application, as each index emphasizes a different physical regime and vertical scale that is directly relevant to the formation, persistence, and phase partitioning of Arctic low-level clouds.

LTS represents an integrated measure of large-scale static stability, typically defined between the surface and the mid-troposphere (700 hPa). It primarily reflects the synoptic-scale thermodynamic environment and is only weakly sensitive to the detailed structure of near-surface inversions. As such, LTS is useful for characterizing broad atmospheric regimes in which low-level clouds occur above all surfaces.

EIS extends the LTS concept by accounting for the lifting condensation level and the moist adiabatic lapse rate, making it more directly related to the strength and height of the capping inversion. EIS has been shown to be a more relevant predictor for stratiform cloud regimes, particularly in marine and polar environments, as it better represents the thermodynamic constraints on vertical mixing and the maintenance of low-level cloud layers.

MCAO, in contrast, does not primarily describe static stability, but rather quantifies surface-driven thermal forcing associated with cold-air outbreaks, typically characterized by strong temperature contrasts between the surface (exclusively above ocean) and the overlying air mass. MCAO is closely related to turbulent heat fluxes, enhanced vertical mixing, and rapid cloud regime transitions. It is therefore particularly relevant for understanding cloud formation and phase evolution during periods of strong *surface–atmosphere disequilibrium, which are common over open water and retreating sea ice in the Arctic.*

Although these three indices are correlated, their combined use allows us to disentangle different physical controls on low-level clouds: large-scale synoptic stability (LTS), inversion strength (EIS), and surface-forced instability and turbulence (MCAO). Considering all three metrics provides a more complete characterization of the thermodynamic environments associated with low-level clouds, and helps to avoid attributing cloud variability to a single, potentially oversimplified stability measure. We have expanded the description of the LTS, EIS, and MCAO indices in the manuscript to emphasize the different aspects of atmospheric stability and surface forcing they capture, despite their conceptual similarities :

"In addition, three metrics are calculated to characterize the vertical structure of the lower troposphere and the advection of heat and moisture at a regional scale:

– Lower-Tropospheric Stability (LTS; Wood and Bretherton, 2006) is defined as the potential temperature difference between the free troposphere and the surface:

$$LTS = \theta_{700hPa} - \theta_{surf} \quad (2)$$

LTS provides a bulk measure of static stability across the lower troposphere and has been widely used as a large-scale predictor of low-level cloudiness (Zhang et al., 2009; Taylor and Monroe, 2023). Under Arctic conditions, where low-level clouds often reside well below 700 hPa, LTS primarily reflects the thermodynamic contrast between the cloudy boundary layer and the overlying free troposphere, rather than the stability within the cloud layer itself, and low LTS values should not be interpreted as convective instability.

– Estimated Inversion Strength (EIS), originally proposed by Wood and Bretherton (2006), provides a more physically meaningful estimate of the inversion strength at the top of the boundary layer. EIS refines the LTS by correcting for the moist-adiabatic temperature structure below the inversion:

$$EIS = \theta_{700hPa} - \theta_{surf} - \Gamma^{m}_{850hPa} (Z_{700} - LCL) \quad (3)$$

where Γ_m^{850hPa} is the moist-adiabatic lapse rate evaluated at 850 hPa, Z_{700} is the geopotential height of the 700 hPa level, and LCL is the lifting condensation level height. Larger EIS values indicate a stronger inversion, which limits entrainment from the overlying free troposphere and promotes the persistence of low-level clouds (Naud et al., 2023).

– The Marine Cold Air Outbreak index (MCAO; Fletcher et al., 2016) reflects the effects of cold-air advection and air–sea interaction. This index is calculated over oceanic regions and defined as the potential temperature contrast between the ocean surface and the lower troposphere at 800 hPa:

$$MCAO = \theta_{skin} - \theta_{800hPa} \quad (4)$$

Positive MCAO values indicate that cold air is advected over a relatively warm surface. It is associated with enhanced turbulent heat and moisture fluxes, increased

boundary-layer mixing, and convective cloud development (Slättberg et al., 2025). While LTS and EIS primarily describe static atmospheric stability, MCAO accounts for dynamically driven processes linked to synoptic-scale circulation and mesoscale cloud organization (Fletcher et al., 2016). MCAO is a useful indicator of low-level cloud occurrence during cold-air outbreak (CAO) and warm-air advection (WAA) conditions."

- Line 195: "… account for the type of surface underlying the cloud:…"

*Thank you, we have changed the sentence: "In this study, we also **account for the type of surface underlying the cloud**: land, open ocean, or sea ice."*

- Line 202-209: I assume that AOD information is only available above clouds, correct? Given the strong stratification in the Arctic, how is it known if this aerosol interacts with the clouds? Broadly, given the small effect found for the aerosol, the uncertainties with observing it, and the uncertainties involved with trying to relate the observed aerosol with the observed clouds, I do not see the value in including this parameter in the study. If it remains, a lot more supporting information is needed. But in my opinion, this represents unnecessary and un-useful information that detracts from the manuscript's other messages.

We thank the reviewer for this important comment. The Cloud–Aerosol Discrimination (CAD; Liu et al., 2009, 2019) algorithm is applied to the entire vertical profile to distinguish cloud particles from aerosols. In the framework of this study, the aerosol optical depth (AOD) is therefore computed by vertically integrating aerosol extinction above the cloud top.

Despite this physically consistent definition, our results indicate only a very weak influence of AOD on cloud occurrence. This limited signal is further compounded by substantial observational and interpretative uncertainties related to aerosol–cloud interactions in this context. These include sampling limitations, potential attenuation of the lidar signal in the presence of optically thick liquid clouds, and the possible vertical decoupling between aerosol layers and the cloud.

In light of these limitations, we agree that the inclusion of AOD does not add significant value to the main objectives of this study and may detract from its core messages. We therefore decided to remove AOD from the set of predictors used in the multiple linear regression analyses presented in this work. This revision simplifies the analysis and strengthens the focus on the dominant thermodynamic and dynamical drivers of cloud occurrence.

- Line 212-217. The text states that a 7-day average is "well suited to capture synoptic variability." By "capture," I take this to mean "resolve" in some sense. However, I do not believe that 7-day resolution is sufficient to resolve the synoptic variability that drives Arctic clouds. Yes, these clouds can be long-lived, but there is a great deal of variation that happens on scales well less than 7 days. Thus, any given 7-day sample will contain a range of conditions that may not be narrowly representative of the state that is suggested by the average over this time period. As an example, simply examine a timeseries of surface temperature at a given Arctic location. 7-day averages will substantially diminish the variability related to synoptic-scale variability of the clouds. Or from another perspective, it is highly likely that there will be a significant synoptic shift within most 7-day samples (i.e., a shift in air mass associated with a front) such that the 7-day value would aggregate information from two (or more) entirely different circumstances. I understand that there is a trade-off here, and that a longer time period is needed to get statistically robust observational signals, but it is also important to be clear that averages in this way will erode the ability to clearly distinguish the relationships between environmental factors and cloud type, which vary on time scales that are not well "captured" by 7-day resolution data. I suspect that some of the odd behavior in the MLR analysis (see below) might be due to this issue.

We thank the reviewer for this very relevant comment. We agree that a 7-day temporal averaging does not resolve synoptic-scale variability in the Arctic and can indeed smooth out a substantial fraction of the variability that drives cloud evolution. As pointed out by the reviewer, synoptic conditions in the Arctic can change on time scales well shorter than one week, and a 7-day average may aggregate distinct air masses and meteorological regimes, thereby obscuring the physical relationships between environmental conditions and cloud type.

In response to this comment, we have revised our methodology and now use a 1-day temporal segmentation instead of 7-day averages. This choice allows us to better represent a wider range of synoptic conditions and to avoid averaging over distinct meteorological situations within a single time window. While this reduces the amount of temporal smoothing, it still provides sufficient sampling to ensure statistically robust results.

- Line 249-250: There is a statement here about the procedure removing the 25% least sampled grid cells. I do not understand this concept. Does it mean that there should be 25% of the grid cells missing in Figure 5? Perhaps this means something else. It should be clarified in the text.

Thank you for raising this point, as the description of the filtering procedure indeed required clarification. The 25% threshold does not refer to a fraction of grid cells being removed, nor is it applied to cloud occurrence values. Instead, it is based on the sampling density, defined as the number of individual DARDAR observations available per grid cell and per day.

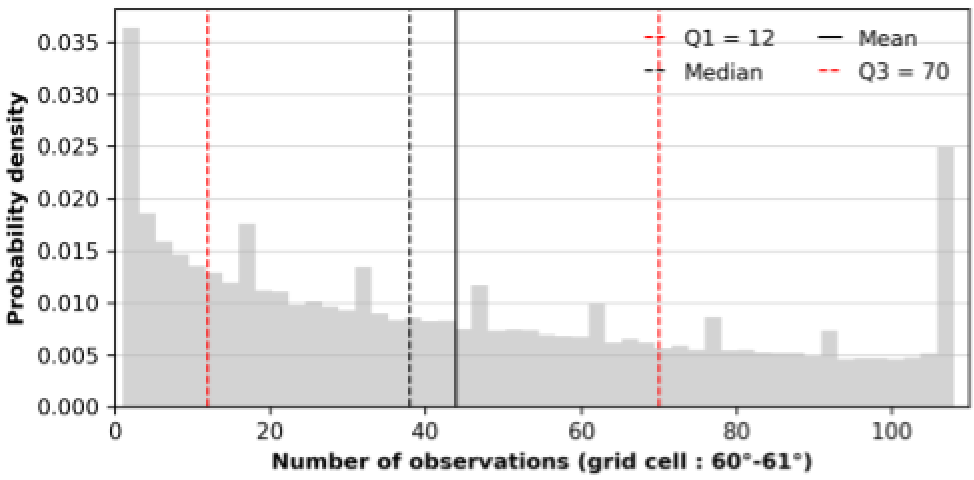

Figure 4 : Distribution of the number of satellite observations per grid cell (60°–61°N)

To determine a statistically meaningful minimum sampling threshold, we first examined the distribution of the daily number of observations (Figure 4) per 1° × 1° grid cell in the lowest-latitude band (60–61°N), which corresponds to the region with the weakest spatial sampling. The first quartile (Q1) of this distribution is found to be 12 observations per day. This value is therefore adopted as a minimum sampling criterion.

This threshold is then uniformly applied to all grid cells and all regions, such that only days with at least 12 individual observations per grid cell are retained in the analysis. Days with fewer observations are excluded, as they are not sufficiently sampled to provide statistically reliable cloud occurrence and associated thermodynamic parameters.

The purpose of this procedure is to ensure that the multiple linear regression analysis is based on a minimum and consistent level of observational support across the domain, thereby improving the robustness and statistical significance of the derived predictors. We will revise the manuscript accordingly to explicitly state that the filtering criterion is based on the Q1 of the daily sampling density in the least-sampled latitude band, and that its sole objective is to guarantee adequate sampling rather than to remove spatial information.

*We hope that this new paragraph is clearer: "**During the eight-year study period, the observational dataset used in the MLR analysis consists of approximately 2,450,300 data points distributed across 1° x 1° grid cells. However, some low-latitude grid cells can sometimes contain a limited number of observations. This makes the calculation of cloud occurrence frequencies and associated statistics less robust. To ensure adequate sampling, a minimum sampling threshold for the number of observations per grid cell is defined based on the first quartile (Q1) of the distribution of daily observations in the lowest latitude band (60°–61°N), where sampling is weakest. We find a Q1 value equal to 12 observations per day. Therefore, only 1° × 1° grid cells containing at least 12 DARDAR pixels per day are retained in the cloud occurrence and MLR analyses.**"*

- Line 260: Would be useful to note here again that Pan-Arctic means 60-82N over all *surfaces.*

*"Across the Pan-Arctic **(defined as the area between 60°and 82° N, encompassing all surface types)**, the median annual occurrence of low-level clouds ($OC_{all\text{-}ll}$) **at altitudes ranging from the clutter height and 3 km (AGL)** is 47.7 % (Fig. 2a)."*

- Line 263: 48.8% of the time, not 48.8% of 50.9%.
- Line 263: "evolve" should be "exist"

*Thank you for these two comments, we have modified the sentence: "**The vast majority of these low-level clouds occur in thermodynamic conditions where cloud temperatures are below 0 °C. These cold low-level clouds are present more than 46 % of the time ($OC_{cold\text{-}ll}$; see Fig. 2b).**"*

- Line 264: "This occurrence fraction decreases…."

We have modified the sentence: "**Their occurrence progressively decreases toward lower latitudes, with the most pronounced reduction observed over oceanic regions.**"

- Line 264-265: This is a sentence fragment; please re-write.

"**The vast majority of these low-level clouds occur in thermodynamic conditions where cloud temperatures are below 0 °C. These cold low-level clouds are present more than 46 % of the time ($OC_{cold-ll}$; see Fig. 2b). Their occurrence progressively decreases toward lower latitudes, with the most pronounced reduction observed over oceanic regions. In contrast, warm liquid clouds are comparatively rare in the Arctic. The median occurrence of these clouds (OC_{warm}) generally remains below 1.5 % (Fig. 2c).**"

- Figure 2: There is a major flaw in the analysis revealed by Figure 2 that appears to not be understood by the authors (at least it is not discussed). I'm surprised a paper could get this far without this issue being detected. There is significant cloudiness over Greenland as noted by multiple studies. Shupe et al. (2013) and Lacour et al. (2017) show results from Summit where clouds occur about 60+% of the time and liquid-containing clouds occur 20-30% of the time. The results presented here suggest that there are no clouds over central Greenland. Based on the appearance of the results (diminishing cloud fraction towards the center of Greenland), I surmise that this is because this study references the height range of 0.5-3 km relative to MSL! The center of the Greenland Ice Sheet is above 3 km altitude, and thus no cloud fraction in Figure 2. Presumably the same is true over other land areas. This issue has serious implications for the results of this study such that results over land are not comparable to observations over the ocean. This concept brings up two points for me: 1) If I am wrong about this whole notion and the 0.5-3km layer is actually defined relative to local ground level, then an explanation is needed for what is happening in Greenland. 2) If I am right about this notion, then either the whole analysis needs to be corrected so that it is defined as 0.5-3km relative to local ground level across the whole domain, or all land surfaces need to be removed from the analysis because they are not comparable to ocean regions. Additionally, the ground-clutter issue would also extend further into the 0.5-3 km MSL height level and would need to be addressed.

We thank the reviewer for raising this critical point. The reviewer is correct in identifying the origin of the apparent absence of clouds over central Greenland in Figure 2 (of the paper). In the original analysis, the altitude range (0.5–3 km) was defined relative to mean sea level (ASL). As a result, regions with high surface elevation, such as the central Greenland Ice Sheet (above 3 km ASL), were effectively excluded from the analysis, leading to artificially low cloud fractions over these areas.

Following the reviewer's comment, we have revised the entire analysis to define altitude relative to the local surface (above ground level, AGL). This correction makes cloud occurrence over high-elevation land surfaces directly comparable to that over the ocean and resolves the spurious absence of clouds over central Greenland. In addition, and consistent with comments raised earlier in the review, we no longer rely on a fixed lower-altitude threshold of 500 m. Instead, the lowest usable altitude is now defined using the radar clutter

Surface type	Mean clutter height (km)	Median clutter height (km)
Ocean	0.810	0.660
Sea Ice	0.803	0.660
Land	0.901	0.833

Table 2. Statistical data on clutter altitude based on surface type (ocean, sea ice, and surface)

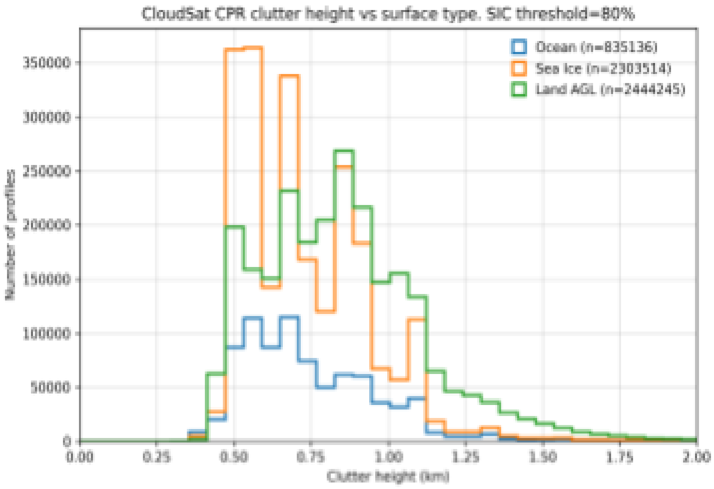

Figure 5: Surface-dependent variability of CloudSat CPR radar clutter height over ocean, sea ice, and land. n corresponds to the total values of analyzed profiles.

height for each CloudSat profile. This adaptive approach accounts for surface-dependent radar contamination and is particularly important over elevated and heterogeneous terrain.

To document and justify this methodological revision, we performed a dedicated statistical analysis of the CloudSat CPR radar clutter height as a function of surface type (ocean, sea ice, and land). The results (Figure 5 and Table 2) show that clutter heights are lowest and most tightly distributed over open ocean and sea ice (median of 660 m and mean of 810 m) and are consistently higher and more widely distributed over land (median value of 833 m). The pronounced tail toward higher clutter heights over land highlights the strong influence of surface elevation and terrain complexity, and demonstrates that a fixed lower-altitude threshold would be inappropriate, particularly over regions such as Greenland.

All figures and results have been updated accordingly with an analysis of clouds between the altitude of the clutter and 3000 m AGL. The influence of clutter on cloud occurrences and phase is discussed with comparison to ground data. With these changes, cloud occurrence over Greenland is now consistent with previous ground-based observations (e.g., Shupe et al., 2013; Lacour et al., 2017).

- Section 3.1: This section is full of important results about clouds. While the definition of low-level clouds is provided (0.5-3km) the results are presented as if they represent all low-level clouds. The danger is that modelers (one of the study's stakeholders) will take these results as metrics for their model. However, numerous papers from ground-based stations (Nomokonovo et al. at NyAlesund, Shupe et al. at Summit, Dong et al. at Alaska, Shupe et al. at Alaska and Eureka, etc.) show that clouds below 0.5 km make an important contribution to low-level cloud occurrence fraction. Thus, the results presented here are not for all low-level clouds, but for a subset. It is essential that the stakeholder community is not led to believe that the results presented here represent all low-level clouds. Thus, this point needs to be made more clearly here and elsewhere throughout the paper. Even better would be some analysis of how biased these results could be due to the near-surface blind zone.

We thank the reviewer for this important comment and fully agree that the results presented in Sect. 3.1 does not represent all low-level clouds. In the revised manuscript, we have clarified throughout the text that the analysis is restricted to clouds detected between the upper limit of the radar clutter and 3000 m above ground level (AGL). Clouds occurring below the clutter altitude, i.e. within the near-surface blind zone of CloudSat, are therefore not included in the statistics presented here.

As explained earlier, the updated processing explicitly accounts for the true altitude of the clutter and expresses cloud heights relative to the surface (AGL), rather than sea level. While this approach ensures physically meaningful cloud occurrence estimates over regions with significant orography, it necessarily excludes clouds located entirely within the blind zone. We acknowledge, as highlighted by the reviewer, that ground-based studies at Arctic sites such as Ny-Ålesund, Summit, and coastal Alaska demonstrate that very low-level clouds can contribute substantially to total low-level cloud occurrence.

To address this limitation, a dedicated subsection has been added to the Methods section presenting comparisons between DARDAR-based cloud occurrences and ground-based observations at several Arctic sites. These comparisons explicitly aim to evaluate the potential bias introduced by the near-surface blind zone (see above).

We have also revised the language throughout the manuscript to avoid any ambiguity regarding the scope of the results and to ensure that readers, including the modeling community, clearly understand that the reported statistics apply to a subset of low-level clouds, namely those observed above the clutter up to 3000 m AGL. The implications of this limitation are now explicitly discussed in the text, and references to relevant ground-based studies are included to place our results in the appropriate observational context.

- Line 301: The annual variation of cloud occurrence is characterized as a bimodal distribution. This is not a distribution, it is a timeseries. It is more appropriate to talk about multiple seasonal maxima instead of bimodality, here and throughout the paper.

*We have modified this sentence as well as all sentences using the term "bimodal": "**The results are presented in Figure 4. It shows that in most regions, the annual cycle of total low-level cloud occurrence exhibits two distinct seasonal maxima.** "*

- Line 327: How do you draw this conclusion about "meridional transport of heat and moisture" from the results that are presented? This point may indeed be true, but the reasoning for how one can come to this conclusion from the presented results needs more support.

We thank the reviewer for this comment. We agree that the manuscript did not provide sufficient direct evidence to support a mechanistic interpretation in terms of meridional heat and moisture transport. The corresponding statement has therefore been removed from the revised manuscript to avoid over-interpretation of the results.

- Figure 4: The text in this figure is hard to see because it is so small. Additionally, it is hard to tell what the actual numbers are because the thickness of the lines is large compared to the granularity of the axis. Lastly, the axis ranges are different among the different columns making intercomparison of the cloud types difficult. This is a clever way to represent a lot of data, but it somehow needs to be made clearer.

We thank the reviewer for this helpful comment. In the revised version, the original figure has been split into two separate figures in order to increase the size of the radar plots and improve the readability of the text and axis labels. In addition, the radial axis limits have been fixed and made identical across all panels, allowing a more direct comparison of the normalized coefficients between cloud types and regions. Finally, the line widths and marker sizes have been adjusted to better match the axis granularity, which improves the visual clarity of the plotted values. These changes significantly enhance the interpretability of the figure while preserving the compact representation of the results.

- Line 359: The negative correlation with OCwarm is minimal or even the opposite for most seasons and is only really observed in summer in some regions.

Thank you for your comment. Here are the two new sentences: "The specific humidity at 700 hPa is positively correlated with OC_{ice} and negatively correlated with OC_{USLCs}. ***SH_700hPa is also anticorrelated with OC_{warm}, except in autumn over high-latitude regions, where a weak positive correlation is observed."***

- Line 362-364: These results imply that specific humidity at 700 hPa is anti-correlated with IWV. This is a remarkable result (that I have a hard time believing). Is there further evidence for this or could it somehow be a result of the 7-day windows used for the analysis?

Thank you for pointing this out. In the revised analysis, this apparent anti-correlation between specific humidity at 700 hPa (SH_700hPa) and integrated water vapor (IWV) is no longer observed. When using the updated MLR framework, SH_700hPa and IWV are found to be positively correlated, which is physically consistent. This behavior is also evident in Fig. 1, where both variables exhibit similar patterns.

Moreover, the revised MLR results show that, when considering all cloud types together, the influence of IWV is generally not statistically significant, whereas SH_700hPa remains a dominant and significant parameter. This suggests that the previously inferred anti-correlation likely resulted from methodological limitations of the earlier analysis, including the use of 7-day temporal aggregation windows, rather than reflecting a robust physical relationship.

We have therefore revised the manuscript to remove this misleading interpretation and to clarify the respective roles of SH_700hPa and IWV in the updated analysis.

- Line 370 (and elsewhere): This section presents some perplexing results that seem to defy fundamental physical process understanding. For example, here it says that ice clouds are related to less stable atmospheric conditions, and below it is noted that USLC are associated with more stable conditions. There is a lot of literature on this topic. Optically, ice and liquid clouds are vastly different, with the former typically being optically thin and the latter being optically thick. Thus, over land and sea ice surfaces for much of the year, the surface will cool much more under ice clouds compared to liquid clouds where the surface can actually warm. (The same is true over the ocean in winter, although the ocean temperature is less responsive.) At the same time, the liquid clouds themselves will more readily cool (cloud top radiative cooling) and vertically mix the atmosphere making it less stratified. These processes push the liquid clouds towards radiative equilibrium with the surface (i.e., the classic LWN approaching 0 W/m2), while the ice clouds would not be (i.e., a big deficit of LWN at surface). Thus, from this first-order set of processes, the ice clouds should be associated with a more stable boundary layer, and the liquid clouds with a less stable boundary layer. However, one of the strongest and most consistent signals in the MLR analysis is the inverse relationship between ice clouds and LTS, and the direct relationship between USLC and LTS. Assuming there is not an error in the analysis, to me this suggests that the LTS parameter does not typically represent the stability of the cloud layer itself (which would behave as outlined above) but instead it represents the difference between the ABL (i.e., cloudy, near-surface environment) and the free troposphere above the cloud layer. This would be a remarkable result, but I'm not clear how it happens. If this is indeed true, it would be great for the paper to describe exactly how it is true, i.e., what is the mechanism?

Building on this previous point, it is fascinating to see the difference in the MLR between LTS and MCAO. LTS is the difference in potential temperature between 700 hPa and 1000 hPa, while negative MCAO is the difference in potential temperature between 800 hPa and the surface. The surface and 1000 hPa are fairly close to each other, so the main difference here is the difference between 700 hPa and 800 hPa, and the sign. For ice clouds, the MCAO signal is indeed weakly of the opposite sign compared to LTS, but is pretty close to 0! The relative relationships are less clear for the other cloud types and sometimes both LTS and MCAO are the same sign in the MLR. This implies that there is some funky behavior happening between 700 and 800 hPa, or that the MLR has some issues (whichever it is should be explained). The relative behavior of LTS and MCAO is central to any discussion about the cloud states. MCAO is arguably more representative of the cloud itself, as the clouds themselves more often encompass 800 hPa than they do 700 hPa. On the other hand, 700 hPa is more often above the cloud in the free troposphere. Thus, to make the claims in the text about the relation of cloud types to stability requires invoking both of these parameters.

We thank the reviewer for this thorough and insightful comment, which helped us clarify the physical interpretation of the stability-related results and substantially improve the manuscript.

We fully agree with the reviewer that, based on first-order radiative–turbulent processes, optically thin ice clouds are generally expected to be associated with a more stable boundary layer, whereas optically thicker liquid-containing clouds tend to promote cloud-top radiative cooling, enhanced turbulent mixing, and reduced stratification within the cloud layer. Our intention was not to contradict this established physical framework.

Following the reviewer's remark, we clarified throughout the manuscript—particularly in the Methods section—that LTS does not directly represent the stability within the cloud layer itself. Instead, LTS provides a bulk measure of the thermodynamic contrast between the cloudy boundary layer and the lower free troposphere. In Arctic conditions, where low-level clouds typically evolve well below 700 hPa, variations in LTS should therefore be interpreted as changes in this large-scale vertical thermodynamic structure, and low LTS values should not be interpreted as convective instability.

Within this framework, the inverse relationship between ice cloud occurrence and LTS, and the direct relationship between USLCs occurrence and LTS, do not imply that ice clouds are associated with a less stable cloud layer. This interpretation is further supported by the seasonal evolution of LTS over oceanic regions reported by Yu et al. (2019), which exhibits a seasonal cycle opposite to that of low-level ice cloud occurrence, with higher LTS values in summer and lower values in winter. Rather, they indicate that ice clouds preferentially occur under conditions characterized by a reduced thermodynamic contrast between the boundary layer and the free troposphere, often accompanied by enhanced moisture at 700 hPa and lower geopotential heights, consistent with synoptic-scale influences. In contrast, USLCs are more frequent under strongly stratified large-scale conditions, even though local cloud-top radiative cooling may promote turbulence within the cloud layer.

We also expanded the discussion of the complementary roles of LTS and MCAO, as highlighted by the reviewer. MCAO, defined between the surface and 800 hPa, more directly represents the thermodynamic environment encompassing most low-level clouds, whereas LTS, defined between the surface and 700 hPa, more frequently includes air above the cloud top in the free troposphere. The contrasting MLR signals between LTS and MCAO therefore suggest that cloud occurrence is influenced by distinct thermodynamic controls acting at different vertical levels.

Finally, we revised the text to explicitly refer to both parameters when discussing cloud–stability relationships, emphasizing that LTS primarily reflects large-scale stratification above the cloud layer, while MCAO is more representative of near-surface and in-cloud processes. These clarifications significantly strengthen the physical interpretation of the results and better align the discussion with established conceptual models of Arctic cloud–stability interactions.

- Line 372: "the presence of moisture at higher altitudes": What does this mean? I thought the analysis was for clouds between 0.5-3 km. What are the "higher altitudes" and the "upper cloud layers" mentioned here?

Thank you for this comment. We agree that the wording "higher altitudes" and "upper cloud layers" was imprecise and potentially misleading. In this study, the analysis is restricted to low-level clouds located between the radar clutter height and 3 km AGL. The reference to "higher altitudes" does not imply cloud layers above this range, but rather refers to the upper part of the low-level cloud layer itself, closer to its cloud top.

Specific humidity at 700 hPa is used here as a proxy for the thermodynamic and moisture conditions in the lower free troposphere immediately near the top of low-level clouds.

Enhanced moisture at this level can favor ice crystal formation and growth near the cloud top through depositional growth, without implying cloud processes occurring at higher altitudes.

We have revised the manuscript to clarify this point and avoid confusion.

- Line 375: With Figure 4 as presented, I cannot tell what the actual numbers are, but the values for AOD appear to be inconsequentially small. Are these significantly different from 0?

We thank the reviewer for this comment. Following this remark, aerosol optical depth (AOD) has been removed from the revised version of Fig. 4 (and 5 for the new version), as its contribution was indeed very small and its statistical significance was unclear. As a result, the question of the significance of AOD no longer arises in the updated analysis.

For the remaining parameters, we have improved the figure readability and interpretability by explicitly indicating the statistical significance of the MLR coefficients. Distinct symbols are now used to represent the p-values associated with the t-test of each coefficient, allowing the reader to clearly identify which parameters are statistically different from zero. These changes ensure that Fig. 4 (and 5 for the new version) now more clearly conveys both the magnitude and the statistical significance of the displayed coefficients.

- Line 387: "lack of representativeness of linear regression models". So are these models not representative? The whole analysis is based on the notion that they are, but if it is now stated that they may not be, what is the implication for this whole line of analysis? This statement really supports the need for a clearer discussion of what this MLR analysis really means and how reliable / representative it actually is.

We thank the reviewer for highlighting this important point. We agree that the wording "lack of representativeness of linear regression models" was misleading and could be interpreted as questioning the validity of the entire MLR-based analysis. This was not our intention. The MLR framework is used here as a statistical diagnostic to identify first-order relationships between cloud occurrence and large-scale environmental parameters.

To further strengthen the robustness and interpretability of the MLR results, several methodological refinements have been implemented in the revised analysis. First, the coefficients of determination (R^2) are now explicitly reported for all MLRs, providing a quantitative measure of model performance. Second, MLR results associated with low explanatory R^2 ($R^2 < 0.1$) are excluded from the analysis and are not displayed in the figures. Third, the statistical significance of each regression coefficient is now assessed using a Student's t-test, and this information is explicitly conveyed in the figures through distinct symbols.

The more complex and heterogeneous seasonal and regional behavior of mixed-phase clouds therefore reflects the intrinsic complexity of the processes controlling their occurrence. The manuscript has been revised accordingly to clarify these points and to remove any ambiguous wording.

- MLR Analysis in general: This MLR analysis brought more questions than answers. First, it is hard to see the real numbers in the figure, and it is not clear which values are significantly different from 0. Perhaps there is some symbol that could delineate the signals that are significantly different from 0. Also, as outlined above, there are some results that seem to defy the basic physical understanding of these clouds, and those results have largely gone without supporting analysis to describe why. Additionally, there are some apparent differences between USLC and MPC that need to be better described. At a basic level those two cloud types are not that different from each other; the primary component of each is the supercooled liquid water. Both have similar radiative cooling that should drive cloud-scale buoyant motions to sustain the clouds. The primary difference is that in MPC there is weak ice formation while in USLC there apparently is not (although above I outline some questions about the ability to always identify the difference). To ensure that the MLR analysis has meaning, there needs to be significant additional analysis of these results and clear description of the mechanisms involved.

We thank the reviewer for this comprehensive and constructive comment. We agree that, in its original form, the MLR analysis required clearer presentation, stronger statistical support, and a more explicit discussion of the physical interpretation, particularly regarding the differences between USLCs and MPCs. The revised manuscript has been substantially improved to address these concerns.

(1) Readability of the figures and statistical significance

To improve the interpretability of the MLR results, the figures have been redesigned. The numerical ranges of the axes have been harmonized across regions and cloud types, allowing direct comparison of coefficient magnitudes. In addition, the statistical significance of each MLR coefficient is now explicitly indicated using distinct symbols based on the p-values of the Student's t-test. This enables the reader to immediately identify which coefficients are significantly different from zero. Furthermore, the coefficient of determination (R^2) is now reported for each MLR, and results associated with low explanatory strength ($R^2 < 0.1$) are excluded from the figures. These changes ensure that only statistically meaningful and interpretable relationships are shown.

(2) Physical consistency and interpretation of the MLR results

We acknowledge that some relationships highlighted in the original version appeared counterintuitive or insufficiently supported. In the revised analysis, these interpretations have been carefully reassessed. Ambiguous or speculative statements have been removed, and the discussion has been refocused on statistically robust and physically plausible relationships. The MLR is now explicitly framed as a diagnostic tool that identifies first-order statistical associations between cloud occurrence and large-scale thermodynamic parameters, rather than as a causal model. When results differ from simple physical expectations, they are now presented without over-interpretation, and discussed in the context of the limitations of large-scale predictors.

(3) Distinction between USLCs and MPCs

We agree with the reviewer that USLCs and MPCs share many similarities, particularly the dominance of supercooled liquid water and the impact of radiative cooling in sustaining cloud-scale turbulence. The revised manuscript now places stronger emphasis on their key microphysical difference : the presence of ice in MPCs. Rather than treating these cloud types as fundamentally different dynamical entities, the analysis highlights how their statistical relationships with environmental parameters vary with conditions favorable or unfavorable to ice formation. In particular, MPC occurrence is shown to be associated with colder and/or less stable conditions and enhanced moisture availability near cloud top, whereas USLCs are preferentially linked to warmer and more stable environments where ice formation remains limited. This distinction is now explicitly summarized in a dedicated concluding paragraph to avoid ambiguity.

(4) Scope and reliability of the MLR approach

To clarify the meaning and reliability of the MLR analysis, we have expanded the methodological description and discussion. The MLR framework is now clearly presented as a complementary statistical approach that helps identify dominant environmental controls on cloud occurrence across regions and seasons, rather than as a complete physical model of cloud processes. The addition of a R^2 filtering and a coefficient of significance provides an objective measure of robustness. While the revised discussion acknowledges that MPCs variability reflects the combined influence of multiple interacting processes that cannot be fully isolated using linear regression alone.

Overall, these revisions substantially strengthen the clarity, robustness, and physical interpretability of the MLR analysis. We believe that the revised presentation now better reflects both the capabilities and the limitations of the approach, and more clearly articulates the meaningful differences between USLCs and MPCs identified by the analysis.

- Line 423: “Spring” should be “spring”

Thank you for this correction : “The impact of cold-air outbreaks on MPCs is stronger during the transitional seasons (spring and autumn), whereas the advection of warm air and moisture above the oceans sustains the liquid phase of warm clouds in summer and of USLCs in spring (Fig. S9 in the Supplement).”

- Line 430-432: Here it is stated that “entrainment” and “heat and humidity fluxes” drive the distribution of phase partitioning. However, the analysis has not examined entrainment or surface fluxes. Thus, unless there is more analysis that actually shows the role of these processes, this can only be considered speculation and should be labelled as such.

We thank the reviewer for this important remark. We agree that the original wording overstated the role of entrainment and surface heat and moisture fluxes, as these processes were not explicitly analyzed in this study. The paragraph has therefore been revised to remove speculative statements regarding the direct influence of entrainment and turbulent surface fluxes on cloud phase partitioning. The discussion is now strictly limited to interpretations supported by the statistical relationships identified in our analysis, and references to turbulent-driven processes are framed more cautiously, emphasizing the need for future dedicated studies rather than drawing conclusions based on the present results.

- Section 3.3.3: It has not been explained how coupling was calculated. Presumably this is related to the cloud-level potential temperature versus the surface potential temperature. Is this determined for each individual observation and then averaged over 7 days for all observations in a given grid cell? Or is there somehow an average potential temperature profile that is then analyzed? The details here matter. Moreover, just like all other parameters, cloud-surface coupling state can vary quite a bit on sub-7 day time scales (see the results from ASCOS), such that it is not clear what this coupling analysis even means.

We thank the reviewer for highlighting this important point. We agree that the original manuscript did not sufficiently explain how the cloud–surface coupling was diagnosed, and that the methodological details are essential for interpreting the results. In the revised version of the manuscript, we have added a dedicated subsection explicitly describing the coupling calculation.

The coupling diagnosis follows the methodology introduced by Sotiropoulou et al. (2014) and subsequently applied in several studies, including Griesche et al. (2021). More specifically, we adopt the simplified version proposed by Gierens et al. (2020). In this approach, the vertical profile of potential temperature (θ) is examined starting from the cloud base down to the surface. For liquid-containing clouds, the reference level corresponds to the base of the liquid layer, whereas for ice clouds, the cloud base is used as the reference height.

For each individual cloud profile, the cumulative mean of θ between the cloud base (liquid layer base) and the surface is computed. If the difference between the cumulative mean of θ and the local θ value exceeds 0.5 K, the cloud is classified as decoupled; otherwise, it is considered coupled. This criterion is applied profile by profile, using the instantaneous thermodynamic structure associated with each cloud observation.

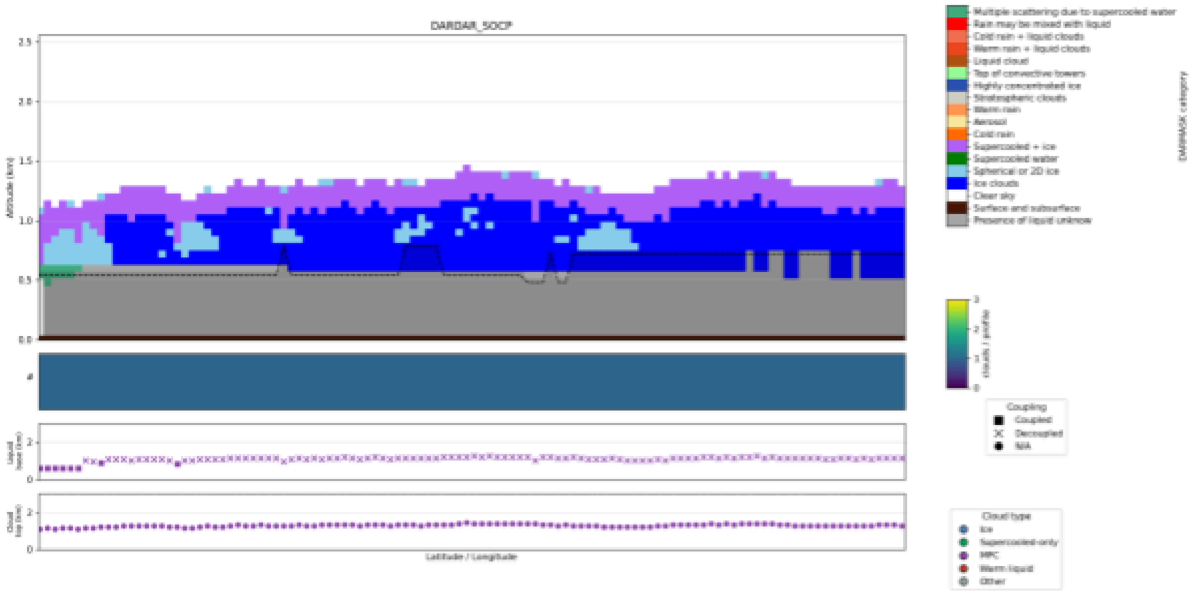

Figure 6. Vertical cross-section of cloud classification derived from the DARDAR-SOCP treatment along a satellite track. The top panel shows the vertical distribution of hydrometeor classes as a function of altitude (km), with colors indicating the different DARDAR-MASK categories (legend on the right). The black dashed line indicates the altitude of the radar clutter, below which data are excluded from the analysis. The second panel displays the number of cloud layers detected in each vertical profile along the track.The third panel shows the altitude of the liquid cloud base (km). Symbols indicate the diagnosed cloud–surface coupling state: squares correspond to coupled clouds, crosses to decoupled clouds, and circles to cases where the coupling state is not available (N/A).The bottom panel presents the cloud-top height (km) together with the identified cloud type for each profile, distinguishing ice clouds, supercooled-liquid-only clouds, mixed-phase clouds (MPCs), warm liquid clouds, and other cloud types.

Importantly, no temporal or spatial averaging of the potential temperature profiles is performed prior to the coupling diagnosis. Each cloud profile and its associated θ profile are analyzed independently. The coupling state is then compiled together with the other cloud properties in the daily output files. Consequently, the subsequent daily (and weekly, in earlier versions) statistics represent the aggregation of individually diagnosed coupling states, rather than a coupling inferred from an averaged thermodynamic profile.

We fully agree that cloud-surface coupling can vary on timescales ranging from less than a day to several days, but also on low-level clouds very close to the ground. Our approach does aim to quantify the statistical prevalence of coupled cloud states at spatial and temporal scales across the Arctic. We hope that this clarification and the associated limitations are now explicitly mentioned in the revised manuscript.

- Line 444: This statement suggests that the coupling state is dependent on whether or not cloud level turbulence is related to surface fluxes. While this may be true over ocean, it is not necessarily true over other Arctic surfaces. Indeed, cloud generated turbulence is also an important factor and the proximity of the cloud-driven mixed-layer to the surface can lead to a coupled situation (i.e., no intervening potential temperature gradient), independent of surface fluxes.

We thank the reviewer for this important clarification. We agree that cloud–surface coupling is not solely determined by surface-driven turbulence, particularly over Arctic land and ice surfaces, where cloud-generated turbulence and the vertical proximity of the cloud-driven mixed layer to the surface can also lead to a coupled state, independent of surface fluxes. To avoid this ambiguity, the statement in line 444 that could be interpreted as implying a surface-flux–controlled coupling has been removed. In addition, a new paragraph has been *added to the methodology section providing a detailed description of how cloud–surface coupling is diagnosed in this study.*

- Line 465-466: This process should be described in more detail. How do increases in air temperature lead to instability? Warm air over a colder surface would instead lead to stability near the surface. Since this text is talking about a maximum in surface coupling, it is this atmosphere-surface interaction that is important. Please explain.

We agree that this statement was ambiguous, and since the underlying mechanism cannot be robustly inferred from our analysis, we have chosen to remove this speculative interpretation from the manuscript.

- Line 472: "these results are only representative of low-level clouds as a whole." Actually, the results are representative of clouds between 0.5-3 km. This is different than low-level clouds as a whole.

*We thank the reviewer for this clarification. We agree that the original wording was ambiguous, as our analysis does not encompass all low-level clouds but is restricted to clouds located between clutter-height and 3 km above ground level. The text has been revised accordingly to explicitly specify the altitude range considered in this study: "**However, these results are only representative of clouds located between the clutter height and 3 km AGL, regardless of their thermodynamic phase.**"*

- Line 483: "surface" instead of "surfaces"

Thank you for this correction: "Interestingly, our results also show that the average cloud top height of ice clouds coupled with sea ice **surface** is close to 2500 m, which is higher than over open sea (Fig. S7b)."

- Line 501: This result has not been shown. I did not see any information about surface turbulent heat fluxes impacting phase partitioning. Some information that may be relevant for such processes might be implied from some of the data, but clear analysis of that data and clear conclusions regarding interpretation of that data must be presented before drawing such a conclusion.

We thank the reviewer for this comment. We agree that the statement in question was too speculative and not sufficiently supported by the analyses presented in the manuscript. As no explicit analysis of surface turbulent heat fluxes and their impact on cloud phase partitioning is provided, this conclusion could not be robustly justified. The statement has therefore been removed from the revised manuscript to avoid overinterpretation of the results: "Overall, our results indicate that the highest fraction of coupled cold clouds is usually above open water when strong temperature gradients occur between the surface and the lower troposphere (winter, spring and autumn). However, the seasonal cycle of coupled clouds varies significantly with the cloud type (ice, mixed, and supercooled liquid) and the surface conditions. In the following section, we will discuss how these surface processes, in combination with larger-scale thermodynamic conditions, affect the occurrence of MPCs and USLCs."

- Figure 7: These results are seemingly inconsistent with those from Griesche et al. (2021), who observe that coupled clouds have more ice than decoupled clouds. Griesche et al. have recently submitted a new paper further supporting their conclusion. While these papers are for shorter periods of time at singular sites in the sea ice, they use sensors that are much better suited to examine this problem.

We thank the reviewer for raising this important point. We agree that earlier versions of our results could appear inconsistent with the findings of Griesche et al. (2021), who showed that coupled clouds tend to contain more ice than decoupled clouds based on detailed observations at individual sea-ice sites. In response to this concern, we have reprocessed the entire dataset and revised the cloud–surface coupling diagnosis, by redeveloping the coupling method in our treatment code following the approach originally proposed by Sotiropoulou et al. (2014) and later simplified by Gierens et al. (2020).

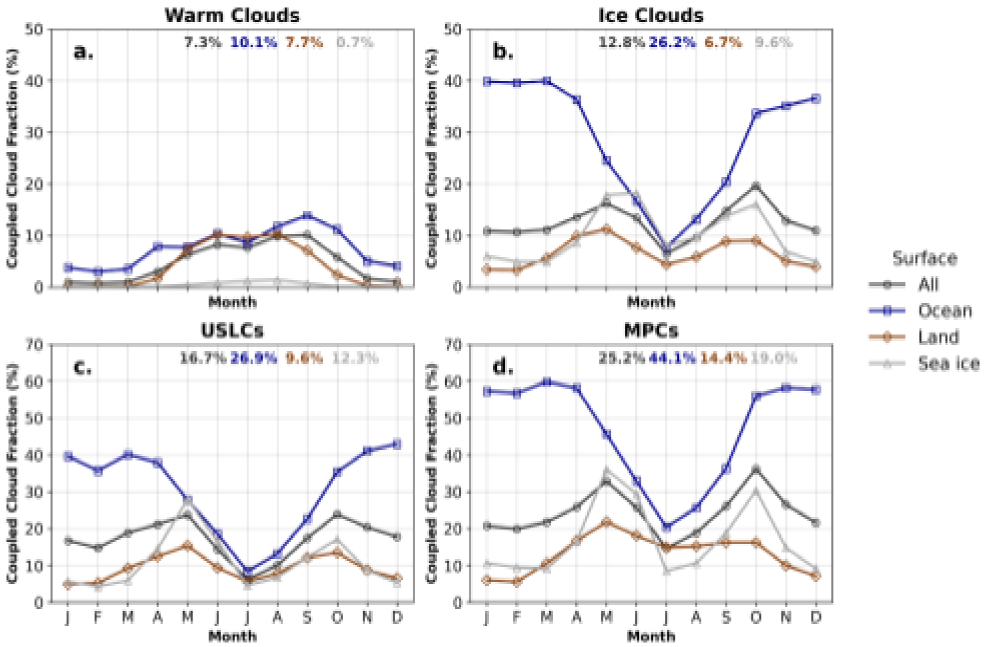

Figure 7: Monthly evolution of low-level coupled cloud fractions over different surface types (ocean, land, sea ice, and all surfaces). Panels show (a) warm clouds, (b) ice clouds, (c) USLCs, and (d) MPCs.

The updated analysis, illustrated in the revised Figure 7, is now more physically consistent with these previous observational studies. In particular, the new results show that mixed-phase clouds (MPCs) are more frequently coupled to the surface than purely liquid clouds (USLCs) across all surface types, and especially over open ocean and sea ice. This behavior is in line with the interpretation that coupling favors cloud regimes with enhanced ice production. While our analysis relies on spaceborne observations with coarser vertical resolution than the ground-based sensors used by Griesche et al. (2021), it provides a pan-Arctic, climatological perspective that complements their site-specific studies.

- Line 532-533: While I appreciate the attempt to compare to surface observations, this comparison is not very functional as presented. One value is a multiyear, pan-Arctic value while the other is a value from 6 weeks of observations at one location. It would be much better to isolate the satellite observations at the time of year and location of the ground-observations (even including multiple years would be fine) in order to attempt to compare processes in a more appropriate way. A better attempt at comparisons of this nature would improve the manuscript substantially. For example, during the process of reviewing this paper, I have done multiple comparisons where I visually extracted data at specific points to compare to ground observations from different papers. A more thorough analysis of this type would be quite useful to confirm or contextualize some of the main findings.

We thank the reviewer for this constructive comment. In response, we have substantially revised the manuscript. First, we have added a dedicated paragraph in the Methods section describing a new comparison framework based on two in situ sites, Ny-Ålesund and Hyytiälä. For these sites, satellite observations are now colocalized in time and space with ground measurement periods. We compared both the variation in occurrences and the cloud type.

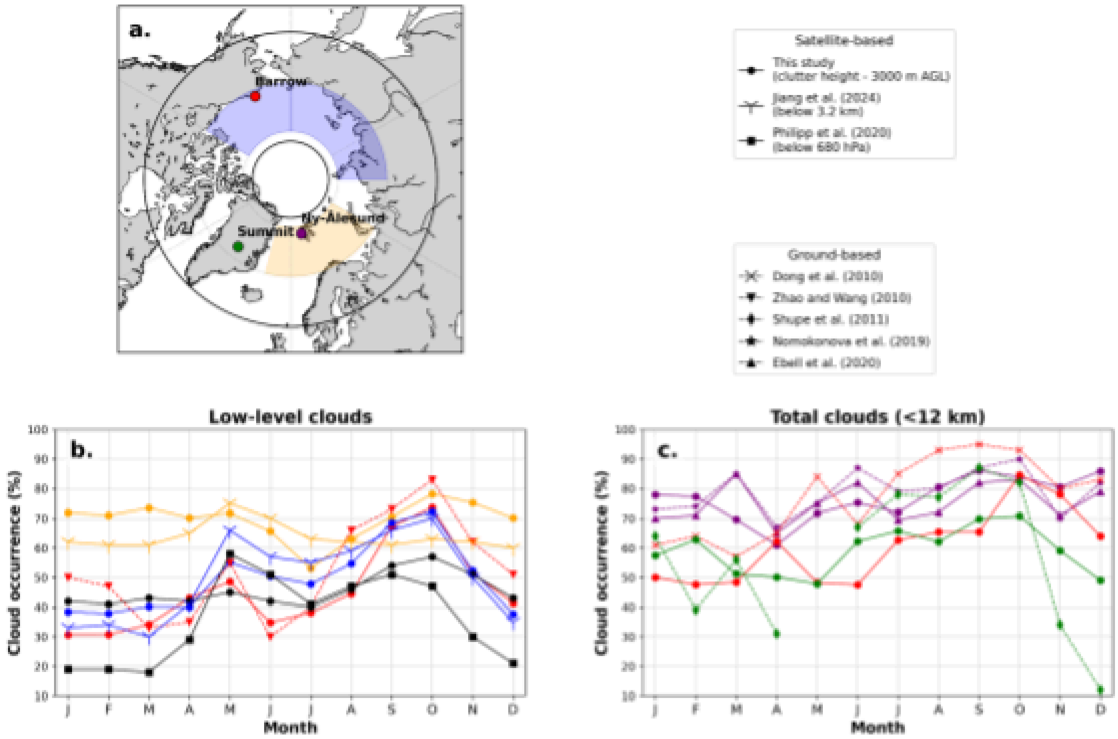

Figure 8: Monthly Arctic cloud occurrence derived from ground-based and spaceborne remote-sensing observations: (a) low-level clouds; (b) total clouds (<12 km).

Second, in the Discussion section, we added a new figure (figure above) that explicitly compares our results with in situ observations and satellite estimates from previous studies. This comparison is performed separately for low-altitude clouds and for total cloud over the entire atmospheric column, allowing for a clearer assessment of the consistency and differences between observation systems.

For all of the comparisons we conducted and updated, we respect the geographical area and temporal variations of the studies. We believe that these additions greatly enhance the functional value of the comparison, better contextualize our main conclusions, and directly address the reviewer's concern about the relevance of the initial comparison.

- Line 540: "breakup" itself has little relevance. Better to say "… the minimum extend of the pack ice…"

We thank the reviewer for this suggestion. The term has been revised accordingly, and "breakup" has been replaced in the manuscript : "The first seasonal peak can be attributed to changes in weather conditions (e.g., cyclonic flows, warm air advections, etc.) that occur at the end of winter (Yu et al., 2019). The high cloud cover observed in autumn is probably a consequence of the changing surface conditions as suggested by Taylor and Monroe (2023) and Cesana et al. (2024). During this season, the ***minimum extent of the sea ice*** *facilitates the supply of moisture from the surface."*

- Line 555: I believe here you mean "ice clouds" not "clouds"

We thank the reviewer for pointing this out. The term has been corrected to "ice clouds" in the revised manuscript.

- Line 557-558: This conclusion is a stretch. There is literature indicating that moisture entrainment aloft can help sustain mixed-phase clouds, but, to my knowledge, this has not been shown for ice clouds. Also, ice clouds do not have the radiative cooling that liquid clouds do, and thus there is little turbulence to drive entrainment. This statement should be removed or supported by more evidence.

We thank the reviewer for this important remark. We agree that the statement was speculative and not sufficiently supported by the analysis presented. The corresponding sentence has therefore been removed from the revised manuscript.

- Line 574-575: Based on the information I can extract from Figure 2, Nomokonova et al. show more liquid-containing clouds than identified here.

We thank the reviewer for this comment. As pointed out, the comparison with Nomokonova et al. required further clarification. In the revised manuscript, we have substantially improved this comparison by adding a dedicated figure and by performing a temporal and spatial colocation of the satellite observations with the ground-based measurements to the extent possible (see above). The corresponding text has been updated accordingly.

- Line 583-585: Here is again more speculation that is not labeled as such and is not supported by observations or analysis presented in this paper. No information has been presented on entrainment or INPs, and little has been provided on aerosols at all.

We agree that this statement was not sufficiently supported by the results shown, especially in the absence of direct information on entrainment, INPs, or aerosol properties. The speculative discussion has therefore been removed from the revised manuscript.

- Line 595-597: This is opposite of the result suggested by Griesche et al. (2021, and newly submitted).

We thank the reviewer for pointing out this apparent inconsistency. As mentioned above, a substantial effort has been made to revisit the analysis of cloud–surface coupling in order to *ensure more robust and coherent results. The methodology has been refined accordingly, and a new dedicated paragraph has been added to the Discussion section to clarify the comparison with Griesche et al. (2021) and to better contextualize the differences. The revised text now provides a more consistent interpretation of coupled cloud occurrence: "**At the pan-Arctic scale, our results show that approximately 17 % of low-level clouds are coupled to the surface. When restricting our analysis to a period comparable to that of the ASCOS campaign (Tjernström et al., 2014) and to a similar geographical domain (limited to 82° N in our case), we find that the fractions of coupled total low-level clouds and MPCs are approximately 15 % and 17 %, respectively. These values are consistent with the 23 % reported during ASCOS (Sotiropoulou et al., 2014), despite differences in the sampling strategy and cloud detection methods employed. In particular, a substantial fraction of the low-level clouds obtained from our dataset is located below 500 m. This altitude range can be strongly affected by radar clutter and surface contamination, thus leading to an underestimation of the fraction of coupled clouds. We show that the spatial distribution of coupled clouds exhibits pronounced contrasts. Higher fractions of coupled clouds are observed over the open water of the Atlantic and Pacific sectors than over continental and sea ice regions. From late autumn to mid-spring, this fraction exceeds 50 % in the seas surrounding Svalbard, which is slightly lower than the value of 60 % reported by Griesche et al. (2021).**"*

- Line 609: remove "or breakup" as nothing has been shown about this process.

We thank the reviewer for this comment. The term "or breakup" has been removed from the revised manuscript: "It also indicates that parameters such as air temperature, LTS, humidity, and MCAO are more strongly correlated with cloud-type occurrences than sea ice extent."

- Section 5: This is now the second time some of these results have been summarized. The partitioning of information between Section 4 and 5 should be re-assessed to ensure a clear delineation and to minimize redundancy.

These two sections have been rewritten to avoid repetition. However, it can be challenging to discuss a specific result without first providing an overview of it. We hope the reviewer will be satisfied with the new version of the discussion and conclusion.

- Line 625-627: This statement attempts to "segregate" USLC and MPCs because of their differences. However, it is not clear that they are so different physically. From a basic physical standpoint, they both have populations of supercooled liquid water droplets. One has appreciable ice observed by CloudSat, the other does not. However, it is not clear the extent to which the cloud ice is actually present but just not observed (issues outlined above). Moreover, it is unclear if there is drizzle in these USLC clouds that would serve a similar mass loss role to ice. From a radiative standpoint, the ice plays little role, so the overall differences would have to come down to detailed properties of the cloud liquid (that are not shown here). There could be some differences in lifetime, but there are many aspects that would have to be considered to unravel that possible difference (and those aspects have not been mentioned or addressed here). The MLR analysis shown here does reveal some differences, but it is not clear how much those are driven by spatial differences in where these sub-sets of clouds occur versus actual process differences (i.e., is it clear from the data that at the same location and time of year MPC and USLCs behave differently at a statistically significant level?). Based on my long experience with these clouds, I speculate that it is quite possible that the main difference could simply be that some airmasses consume all their available INPs before the clouds lose all their moisture. To some degree this has to be true because, according to most ice nucleation theory, if there are appropriate INPs available they will nucleate ice. Overall, I think the conclusions should stick to what can be concluded from the data that has been presented. Speculation on the meaning of results is fine, but it should be labelled as such and based on some evidence.

We thank the reviewer for this detailed and constructive comment. We fully acknowledge the instrumental uncertainties associated with spaceborne cloud observations, in particular the limited sensitivity of CloudSat to small ice particles and the resulting ambiguity between the absence of detected ice and the presence of ice below the detection threshold. These limitations are well known and have been explicitly considered throughout this study. We have made a concerted effort to quantify and mitigate their impact by carefully documenting potential sources of misclassification and systematically discussing the associated uncertainties.

Despite these observational limitations, the results presented in this study suggest consistent and statistically robust differences between USLCs and MPCs in terms of their large-scale environmental conditions. These differences are observed across multiple regions and diagnostics and therefore cannot be attributed solely to instrumental effects or spatial sampling biases. In particular, MPCs are preferentially associated with colder thermodynamic conditions and more frequent marine cold-air outbreaks (MCAOs) in the lower troposphere, which are known to favor the formation and persistence of an ice phase beneath a supercooled liquid layer.

Conversely, USLCs tend to occur in environments that are comparatively less favorable for ice formation. Under such conditions, ice production may be limited by a reduced availability or effectiveness of ice-nucleating particles (INPs), consistent with existing ice nucleation theory, or by thermodynamic constraints that suppress efficient ice growth. In addition, even when ice is present, its concentration may remain too small to be detected by CloudSat, leading to an observational classification as USLCs.

We emphasize that our interpretations are intentionally restrained and remain closely tied to the observational evidence presented in the manuscript. While the role of INPs provides a physically plausible framework for interpreting the observed differences, we avoid strong process-level claims that cannot be directly supported by the available data. The manuscript has been revised accordingly to clearly distinguish robust, observation-based conclusions from more speculative interpretations.

- Line 637: The paper has not shown any diagnostics of orographic lifting. Indeed, it appears that the analysis may have a significant bias for observations over land, and especially over orographic features (see above). Thus, I would not trust any of the results related to orography. This also raises an important question related to my earlier comment about the height reference relative to the land surface: if MSL is used instead of AGL, how are some of the parameters calculated ? i.e., 1000 hPa is below the surface of most mountains. Indeed even 700 hPa is below the surface over central Greenland.

We thank the reviewer for raising this important point. We agree that the present analysis does not explicitly diagnose orographic lifting processes, and we therefore do not intend to draw process-level conclusions related to orographic forcing. The discussion has been revised accordingly to avoid over-interpreting results in regions where orographic effects may play a role.

Regarding the potential bias over land and mountainous regions, we emphasize that all atmospheric parameters used in this study are computed only where valid values are available. When pressure levels fall below the surface (e.g., 1000 hPa or 700 hPa over elevated terrain such as central Greenland), no values are defined in the input datasets. In these cases, the corresponding parameters are not calculated and are assigned missing values (NaNs). These NaNs are systematically excluded from all subsequent analyses, including the multiple linear regression (MLR). As a result, grid points and time periods for which surface elevation exceeds the considered pressure levels do not contribute to the statistical relationships derived from the MLR analysis. This ensures that the regression results are not contaminated by unphysical values arising from pressure levels located below the surface.

We have added a dedicated clarification in the Methods section to explicitly describe this treatment of pressure levels below the surface and the handling of missing values in the statistical analysis. This addition clarifies how topographic constraints are accounted for and ensures that the MLR analysis is based solely on physically meaningful atmospheric conditions. While this approach limits the representativeness of results over high orography, it avoids introducing spurious signals and strengthens the robustness of the conclusions drawn from the available data.

- Line 660: The "longevity" of USCLs has not been examined here. Rather, a collection of snapshots are analyzed jointly. These observations do not say much about longevity.

We thank the reviewer for this comment. We agree that the present analysis is based on independent satellite overpasses and does not allow us to assess the temporal persistence or longevity of USLCs. The term "longevity" has therefore been removed from the revised manuscript to avoid any potential misinterpretation.

- Line 661-662: This statement has not been shown.

We thank the reviewer for pointing this out. The statement has been removed from the revised manuscript, as it was not sufficiently supported by the analysis presented.

- Figure S1: Top box should be "unknown"

Thank you, this change has been made.

- Figure S2c: I assume this table is for total and MPCs that are low clouds (0.3-5km). That should be included in the caption. Figure S3, S5, S7: I assume these figures are for total low clouds (0.3-5km). That should be included in the captions.

A clarification regarding the altitude range of the analyzed clouds has been added to all captions.

References :

Hogan & O'Connor (2004). Facilitating cloud radar and lidar algorithms: the Cloudnet Instrument Synergy/Target Categorization product. https://www.met.reading.ac.uk/~swrhgnrj/publications/categorization.pdf