

# Answers for Reviewer 1

First, we would like to thank the first reviewer for its time and for its positive and constructive review. Below we respond to the specific comments. As requested by the reviewer, we also show some additional results and revised the text in the manuscript.

## 1 Overview

This manuscript uses 13 years of satellite lidar observations and in-situ measurements from the MOSAiC campaign to examine the drivers of the Arctic spring cloud onset. One main goal of the manuscript is to understand the drivers of the spring cloud onset. Specifically, the role of moisture advection vs. warming due to seasonal increases in solar insolation and temperature advection. Supporting previous literature, the authors find a relatively small role for moisture advection. They additionally show that the timing of the spring cloud onset is associated with warming from ice-dominated cloud regimes  $T < -13C$  to liquid-dominated cloud regimes  $T > -13C$ . The authors consequently infer that atmospheric warming is responsible for the spring cloud onset.

I find this manuscript well-written and motivated. The application of long-term satellite records to the spring cloud onset is valuable and interesting. The addition of in-situ ground-based lidar and radiosonde observations complements this top-of-atmosphere perspective nicely and care is taken to make fair comparisons between these data sources.

## 2 General comment

The authors demonstrate that moisture advection explains some variability but not the overall spring cloud onset and show that cloud phase is a strong function of temperature. However, there is relatively little focus on isolating the drivers of this temperature increase and it is ascribed to either solar insolation or remote temperature advection without much analysis. Specifically, both solar insolation and temperature advection are listed as potential drivers of the spring cloud onset, but manuscript's discussion of these processes is inconsistent and often lacking. I think the discussion of these processes should be clarified in the introduction and discussed with an eye towards future change in the discussion/conclusion. With these changes and other suggestions detailed below, I believe that the manuscript can be accepted with minor revisions. Beyond those edits, however, I think that the impact of this work would greatly benefit from the inclusion of some additional analysis to separate between remote and local drivers of the spring cloud onset. These suggestions along with other more minor comments are included below. While not necessary, I leave decisions regarding these suggestions to the editor and authors.

We agree with the reviewer that a more detailed discussion in order to disentangle processes responsible for the spring temperature warming over sea-ice, the increase of solar radiation and the temperature advection from the mid-latitudes, is needed here. For this reason, using the framework presented by Serreze et al., 2007 and ERA5 outputs only, we have computed both contributions: 1) the moist static convergence flux (see Eq.1) and 2) the net top-of-the atmosphere shortwave radiation budget.

$$\nabla F_{adv} = \oint_C \nabla \frac{1}{g} \int_{250hPa}^{sc} (c_p T + \phi + Lq)(\vec{v} \cdot \vec{n}) \quad (1)$$

where  $\vec{v}$  is the horizontal wind,  $\vec{n}$  the normal vector to the contour as defined in Fig.4., T the temperature, q the specific humidity  $c_p$  the specific heat of the atmosphere at constant pressure, L the latent heat of evaporation, C the spatial contour as presented in Fig.4 of the present article and  $\phi$  the geopotential.

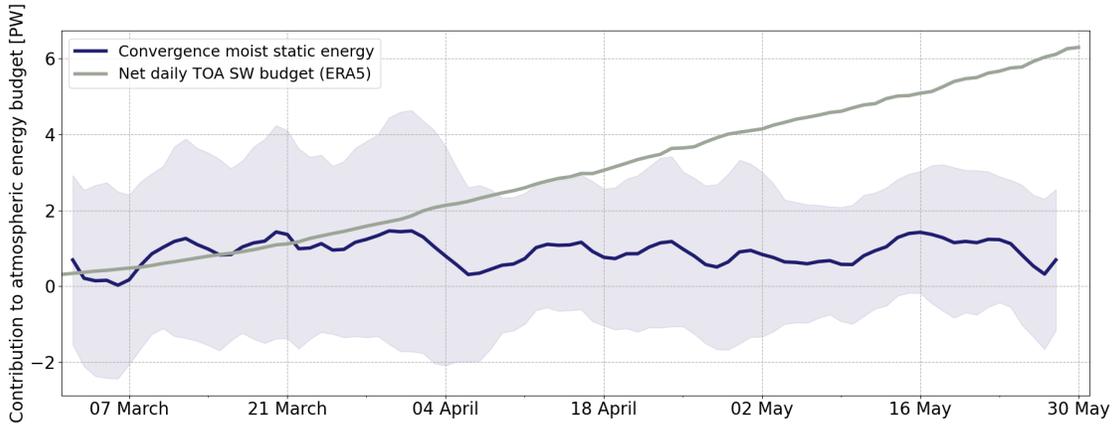


Figure 1: Contribution of horizontal transport of moist static energy and solar radiation to the total atmospheric energy stored over the sea-ice. The net top-of-the atmosphere shortwave radiation budget (grey line) is integrated over the surface defined in Fig.4 of the present article. The convergence of moist static energy is integrated along the same spatial contour as defined in Fig.4.

Figure above shows a close to constant contribution of the moisture static energy transport from the mid-latitudes to the Arctic between March and May, while the contribution of net solar radiation is (1) steeply increasing and (2) several times higher than the moist static energy transport contribution. Therefore, results presented here emphasize that the increase of lower troposphere temperature between March and May over the sea-ice is solely attributed to the increase of incoming solar radiation and not to the increase of energy transport from the mid-latitudes.

Since these results are consistent with previous estimations based on older version of ECMWF reanalysis (Serreze et al., 2007; Mayer et al., 2019), we add these information in the Appendix.I. We also added a short paragraph referring to the Supplementary material and to the existing literature in the introduction section (L67-73) in order to clarify the drivers of the spring temperature increase over sea-ice.

### 3 Minor comments

- Lines 12 – 14: “Overall. . . April”: The authors do not demonstrate that solar heating is responsible for the spring cloud onset. The contributions from local heating and remote temperature advection are not explored here.

We agree with the reviewer that this statement was not supported by the study. Following the general comment of the reviewer, we added information L67-73 and this statement is now supported by the literature and by the new figure in Appendix.G.

- Lines 50 – 51: “As spring. . . persist”: The role of solar insolation vs. remote transport is not examined here. Is there previous literature you can point to that identifies solar insolation as the dominant driver of low-level atmospheric warming in the spring?

We agree with the reviewer that this statement was not supported. Again, consistently with the general comment, we now added information in the introduction section (L67-73).

- Lines 51 – 56: I think it is valuable here to describe how the temperature dependence of cloud phase is mediated by dynamics and aerosols in addition to the WBF process. For example, Shaw et al. (2022) and Gjelvsik et al. (2025) both studied how model aerosol schemes impact the temperature-dependence of Arctic cloud phase while using CALIOP observations as ground-truth.

We agree with the reviewer that our paragraph on aerosol-cloud interaction was not clear enough. (i) We restructured that paragraph (L82-88) and (ii) added the reference of Shaw et al., (2022). However, we did not find the reference of Gjelvsik et al. (2025) but found a study of Filotoglou et al., (2019) also quantifying the role of aerosols on Arctic cloud phase using CALIPSO.

- Line 58: remove comma

Following the review modification, this sentence does not exist anymore.

- Line 81: CALIPSO sampling has high spatial and temporal resolution, but the spatial sampling is limited due to the small footprint size. How often does CALIPSO obtain a complete observation of the study area and how do the authors handle incomplete data at daily resolution?

We agree with the reviewer on the limited spatial sampling of CALIPSO and we do not handle spatial gap for a given day. However, we consider this a limited concern for the present analysis for two reasons. (1) CALIPSO observations accumulate rapidly: CALIPSO provides  $\sim 14$  polar overpasses per day over the Arctic. As shown in the figure below, all grid cells in our sea-ice domain are sampled within three days. (2) The Arctic low-level clouds considered here are both spatially extensive and temporally persistent. Previous studies report horizontal scales of 100–500 km for Arctic stratiform clouds and typical lifetimes of one to several days during late winter–spring (Shupe et al., 2011). Thus, while CALIPSO’s footprint does not resolve the domain instantaneously, the synoptic-to-multi-day persistence and large spatial extent of these clouds mean that the accumulated sampling (over 1–3 days and longer) captures the relevant cloud features, especially when considering 13 years of CALIPSO observations. This present paragraph and the following figure were added as Appendix.H and mentioned it in the caption of Fig.1.

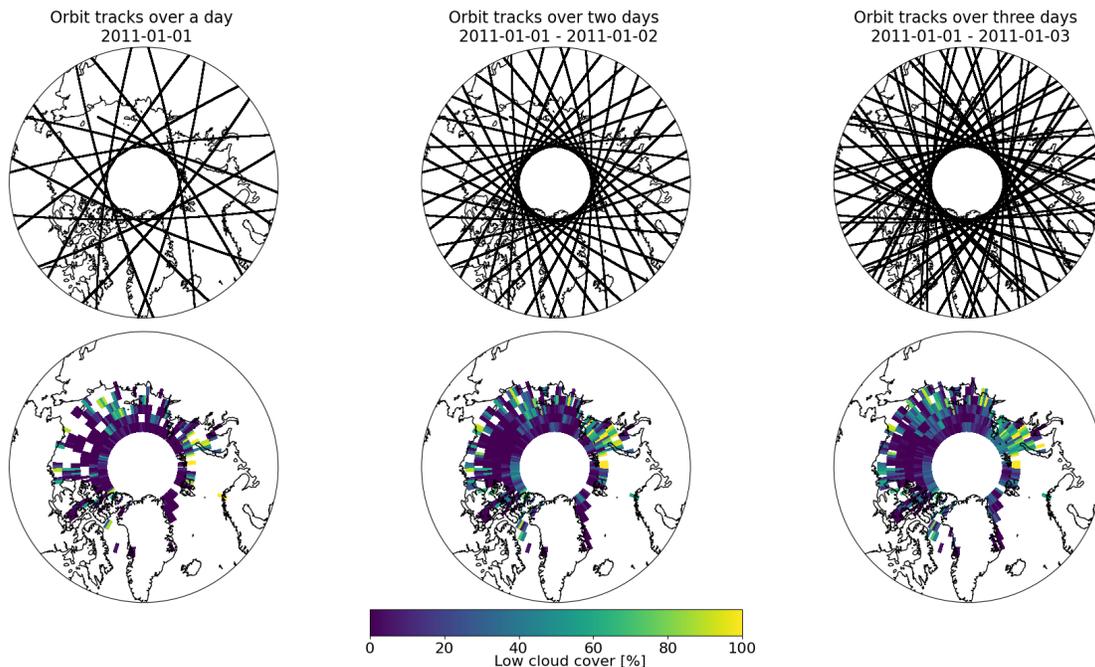


Figure 2: (top) Every CALIPSO orbit tracks over one, two and three days. (bottom) Maps of Low cloud cover averaged over one, two and three days from CALIPSO-GOCCP  $2^\circ \times 2^\circ$  daily gridded data.

- Line 102 (equation 2): Is additional nomenclature needed to indicate the threshold versus the attenuated total backscatter in the cross-polarized direction?

We agree with the reviewer and added information L133.

- Lines 107 - 109: I had to read this sentence multiple times before understanding and recommend reorganization. e.g. When a layer with  $SR \geq 30$  is located between 720m and 3200m only 17% of the underlying profiles are fully attenuated, leading to unclassified layers near the surface.

We restructured this sentence as suggested by the reviewer L142-144.

- Line 116: "ice cloud layers"  
We modified L150 as suggested by the reviewer.
- Line 116: suggest adding "at each isotherm"  
We modified L151 as suggested by the reviewer.
- Line 131: i.e.  
We modified L171 as suggested by the reviewer.
- Lines 134-135: "but does not...SR  $\leq$  30". This additional clause is a bit confusing and I recommend deleting it or moving it to a separate sentence.  
We modified L176 as suggested by the reviewer.
- Line 145: replace comma with "and" between temperature and relative humidity.  
We modified L191 as suggested by the reviewer.
- Line 151: Some technical understanding is assumed here and should be described. far-range channel is not previously defined/described. Complete overlap is also not defined.  
We thank the reviewer for pointing this omission and bring additional information L181-188.
- Line 172: saturate x saturated  
We thank the reviewer for pointing this mistake, we modified L223.
- Line 176: extra period "section. 2.1"  
We modified L227 as suggested by the reviewer.
- Line 195: Unclear what "missing saturation w.r.t. liquid 85% of the time" means. Is this a classification error and if so can you define it more clearly?  
We modified L246 as suggested by the reviewer.
- Figure 1 caption: Does each pixel have daily data? If not, what frequency of data do most pixels have and how are missing data accounted for? panel instead of pannel  
The frequency of data is highly dependent on the latitude band considered due to the CALIPSO near-polar orbit. While at 81N all pixels are observed everyday, it takes 2-3 days to have a complete map of the region over sea-ice. We refer to the answer we wrote for L81 that explain why we think it is not a critical concern for this study.
- Line 213: until the 7 May x until 7 May  
We modified L266 as suggested by the reviewer.
- Lines 219 - 222: I think that this information should be included as supplemental content if it is discussed.  
We agree and added additional maps for both events of 2015 in the Supplementary material.F.
- Figure 2: Ice clouds also appear to be least frequent in the atmospheric temperature inversion. Can you discuss the role of atmospheric dynamics here as opposed to the focus on atmospheric temperature?  
We agree boundary layer processes has a role on the minimu of ice cloud fraction observed in Fig.2b.  
We add a comment on that L289.
- Lines 236 - 237: wording is confusing, perhaps a comma is missing between 6% and below?  
We modified L291 as suggested by the reviewer.

- Lines 238 - 240: Does this mean that high ice clouds must be increasing since cloud ice clouds stay constant (line 236)? If this is the case, I would partition the probable thin ice clouds into categories below and above 3.2km.

There may be a misunderstanding here. The “probable thin ice” category is diagnosed using a lower scattering-ratio threshold than the standard CALIPSO-GOCCP cloud detection (i.e.,  $3 < SR < 5$ ) instead of ( $SR > 5$ ). Therefore, the percentages shown in Fig.3 are not included in the ice-cloud fraction reported in Fig.2b. As a result, it is possible for the ice-cloud fraction to show little or no seasonal variation while the probable thin ice occurrence exhibits a seasonal evolution, since the two diagnostics are computed independently. The independency between the “probable thin ice” category and the ice cloud fraction was clarified L179-180.

- Lines 240 - 241: Can the authors explain the mechanism why?

As highlighted in the previous answer, the probable thin ice category corresponds to thin ice clouds detected below the usual threshold of CALIPSO-GOCCP ice cloud fraction (Fig.2b). Although Fig.2b suggests a low ice-cloud fraction, the frequent occurrence of probable thin-ice detections (Fig.3) indicates that low-level atmospheric ice particles remain ubiquitous in spring over sea ice.

- Lines 241 - 244: “In addition... 720m” Can the authors explain why this matters/why it supports their conclusions?

Following the review modification, this sentence does not exist anymore.

- Lines 245 – 248: The structure and intent of this paragraph is confusing. I suggest rewriting this and the previous paragraph for cohesiveness and clearly explaining the author’s hypothesis on the importance of atmospheric ice particles.

We restructured the paragraph L292-299 as suggested by the reviewer.

- Figure 3: I recommend setting the x-axis maximum to May 30 as in Figure 1, splitting ice clouds into categories below and above 3.2km, and adding a second y-axis to show the seasonal evolution of solar insolation at 70 and 82 degrees latitude.

We thank the reviewer for these suggestions. (1) We modified Fig.3 x-axis maximum to May 30. (2) We refer to the answer for comment L238 on splitting the ice clouds, and therefore did not do this modification. (3) Thanks to the general comment of the reviewer, we think that the role of solar radiation is clearer now and the information on the evolution of solar radiation is less needed.

- Figure 4: Panel a: “time serie” x “time series” / Panel c: Annotated events should be labeled more clearly. / Label: “capture → captures”

We thank the reviewer for this review and modified Fig.4.

- Lines 254 - 265: This paragraph only considers advection as a moisture source, but what about local sources like the melting surface? If this contribution is known to be small please state that with an appropriate reference.

We agree with the reviewers and now stated in the Fig.4 caption that the local moisture source is negligible against the moisture transport from mid-latitudes based on Walter et al., 1995 and Serreze et al., 2007.

- Lines 270-272: If these data are described then they should also be shown in a referenced figure.

We agree with the reviewer and added new figures in the Supplementary Material (Fig.F1 and Fig.F2).

- Lines 265 – 280: This paragraph is really interesting. So anomalous moisture advection superimposes variability onto the existing seasonal cycle but does not modify it? Could the existing seasonal cycle be driven by the advection of moist static energy? If temperature is the driving factor, the energy needed to raise temperatures can either be sourced locally (e.g. solar warming) or remotely (advection from lower latitudes). This is an important distinction since these two sources may respond differently to global warming. I recommend repeating the moisture advection analysis (Figure 4) with low-level

moist static energy. This should quantify the role of all advective processes to the spring cloud onset. Additionally, increases in atmospheric heat content due to the absorption of solar radiation by the atmosphere can be calculated from CERES observations (or ERA5 fields) to quantify the role of the seasonal solar cycle. Here, radiation absorbed by the surface can be assumed to go into phase changes and ignored. This analysis should allow the authors to disentangle these processes as drivers of the spring cloud onset.

We thank the reviewer and refer to the answer to the general comment for precisions related to that question. Additional content on this point was added L67-73 and in the Supplementary material.

- Figure 5: caption: review color labels. The “white” color in panel a. appears tan to me.

We agree with the reviewer and modified Fig.5 caption.

- Line 284: Citation should be in parentheses

We agree with the reviewer and parenthesis were added L351

- Line 303: Why do all cloud fraction values decline steeply at 0C?

Temperatures above 0°C correspond to a very small subset of cases in our dataset (see Fig.7b). We therefore interpret the decline in cloud fraction above 0°C primarily as a sampling effect, rather than a robust physical signal. Investigating where and when these rare warm cases occur (specific regions/events) would require a dedicated analysis that is beyond the scope of this study.

- Lines 320 – 322: After previously focusing on solar insolation, temperature advection is now discussed. This is not consistent with the narrative described in the abstract and introduction, which focuses entirely on solar insolation. Please review all discussion of these climate processes and ensure they are consistent throughout the manuscript.

We agree with the reviewer and removed these lines. As said for the major comment, we decided to set a discussion on the relative contribution of solar radiation vs atmospheric transport on the temperature increase in the introduction section (L67-73).

- Lines 336 - 339: This seems to contradict your conclusion in lines 262-263. Can you clarify/reconcile these statements ?

We agree with the reviewer and added a sentence to clarify these statements L326.

- Line 355: expanse – expense

We thank the reviewer for pointing this mistake, we modified L424.

- Lines 368 – 374: “What does this conclusion say about the future of the spring cloud onset and sea ice melt onset? Does it imply that the contribution of the cloud onset will not push melt onset earlier?” I think an understanding of solar vs. advection driven warming would be especially valuable here since these processes will change very differently under global warming.

We agree with the reviewer that a discussion on the future of the spring cloud onset and spring melt onset would be valuable here. To our knowledge, there is no specific studies on the contributions of atmospheric processes on the projected earlier melt onset expected in Spring (Stroeve et al., 2018), and therefore it is hard to state for an increasing role of clouds on spring melt onset. We add a word on the future of spring cloud onset and sea-ice melt onset L448-454 as a perspective for future work.

- Figure C1: Caption should describe what the bolded and other lines represent

We thank the reviewer for pointing out this omission. We added information in the Fig.C1 caption

- Figure D1: Caption should describe what the filled region represents and why the dashed box is present.

We thank the reviewer for pointing out this omission. We added information in the Fig.D1 caption

# Answers for Reviewer 2

First, we would like to thank the second reviewer for its time and for its positive and constructive review. Below we respond to the specific comments. As requested by the reviewer, we also show some additional results and revised the text in the manuscript.

## 1 Overview

This study is based on the analysis of satellite observations of clouds and sea ice concentration in order to explain the cloud onset occurring from March to May in the Arctic.

The authors used the CALIOP-GOCCP lidar observations from 2008 to 2020 to determine cloud layer phases (liquid, ice and unclassified) and their occurrences. Sea ice concentration is determined from NSIDC observations, and temperature and humidity data are obtained from ERA5 reanalyses. In addition, analyses of ground based data (lidar observations and radiosoundings from the MOSAIC campaign) are included.

The results highlight that, in addition to the supply of moisture contributing to the increase of cloud occurrence during Arctic spring, the rise in air temperature (due to more solar radiation from March to May) plays a major role in the increase of the occurrence of liquid containing cloud. The authors conclude that the increase of temperature alters the balance of cloud phase formation, favoring the liquid phase formation at the expense of ice phase.

## 2 General comments

The paper is well written and well structured. The observations, as well as the methodology, are well described. In particular, the authors take care to apply the same method for both space and ground based observations. They demonstrate that water vapor transport is not a limiting factor for the spring cloud onset. They suggest that ice production processes lead to a depletion of moisture in early spring. They assume that, later during spring, the increase in air temperature is responsible for the transition from ice dominant clouds to liquid dominant clouds.

In my opinion, the goal of this study is clear and of interest to the scientific and Arctic communities. The data and methodology used are very well appropriate. Nevertheless, the main conclusion highlighting that liquid cloud amount increase with temperature on a global average is rather well established, and not particularly very new.

We would like to emphasize that the main contribution of this study, in our opinion, goes beyond documenting a temperature–phase relationship. Specifically, we first highlight that the increase in cloudiness over sea-ice between the first week of April and the first week of May is spatially homogeneous over the sea-ice and mainly due to an increase of liquid-containing optically thick clouds. The remainder of the paper disentangles the relative roles of moisture and energy transport from the mid-latitudes and solar radiation in driving this spring cloud onset over the Arctic sea-ice. (1) We assess the moisture transport contribution and show that, already in March, the moisture transported over the sea-ice is sufficient to support a "close to saturation" state. Indeed, it represents a larger fraction of the atmospheric moisture stored over sea-ice than later in May. This indicates that a lack of water vapor is unlikely to be the limiting factor for the timing of the spring cloud onset. (2) In parallel, we show that the springtime temperature rise over sea ice, mainly driven by the rapid seasonal increase in incoming solar radiation with no contribution by meridional energy transport, shifts the phase balance toward more liquid-containing clouds, thereby enabling the observed transition to optically thicker, liquid-containing, low-level clouds. Taken together, our results support the interpretation that the seasonal cycle is primarily forced by the increase of solar radiation, while strong moisture transport and other advection processes mainly modulate cloudiness on synoptic and daily

timescales. Therefore, we believe this study helps clarify the relative importance of the mechanisms driving the spring increase in cloudiness over Arctic sea ice and provides a context for future studies on the daily variability of cloudiness over the Arctic sea-ice. We thank the reviewer for this general comment, which motivated us to clarify the scope and reframe the manuscript around the relative roles of moisture/energy transport and solar radiation driven spring warming in setting the spring cloud onset.

The study could be made more robust and substantially improved by accounting for the interannual variability of cloud occurrences and by investigating deeper the link between cloud onset and additional environmental parameters (for example: warm air intrusions, dynamics and stability conditions, surface coupling. . .). Below are some ways to enhance the study before publication.

- First, the results are based on averages over the entire Arctic sea ice region and a long period of 13 years. The authors should study more in detail the interannual variability of cloudiness, sea ice concentration and thermodynamical parameters (temperatures, humidity). The extensive database (13 years, almost the entire Arctic region) makes such an analysis possible. For example: is there a link between the interannual variability of sea ice coverage and cloud occurrences (and phase), as well as temperature, humidity amount and their transport?

We agree with the reviewer that the 13 years of cloud observations should be used in order to study the interannual variability more in detail. We performed a short analysis by looking at weekly average of temperature, moisture, transport of humidity and moist static energy against low cloud cover, mainly for the period before the spring cloud onset to understand what could trigger an earlier spring cloud onset.

The Figure below emphasize (i) the strong relationship (correlation coefficient around 0.7) between temperature and the low cloud cover over the sea-ice (panel a). For a given week in March, warmer years are consistently cloudier, supporting our interpretation that temperature (via phase partitioning) is a primary control on low-cloud occurrence. (ii) In contrast, while moisture is generally sufficient to allow cloud formation in March on average, year-to-year variations in moisture content and poleward moisture transport still covary with low-cloud cover (panel b and c,  $R(\text{TCWV})=0.9$ , moisture flux  $R(\text{flux})=0.6$ ), indicating that dynamics can modulate cloudiness even if they do not set the seasonal timing of the spring transition.

We added the figure below in the Supplementary materials and added comments on the interannual variability L430 and L454

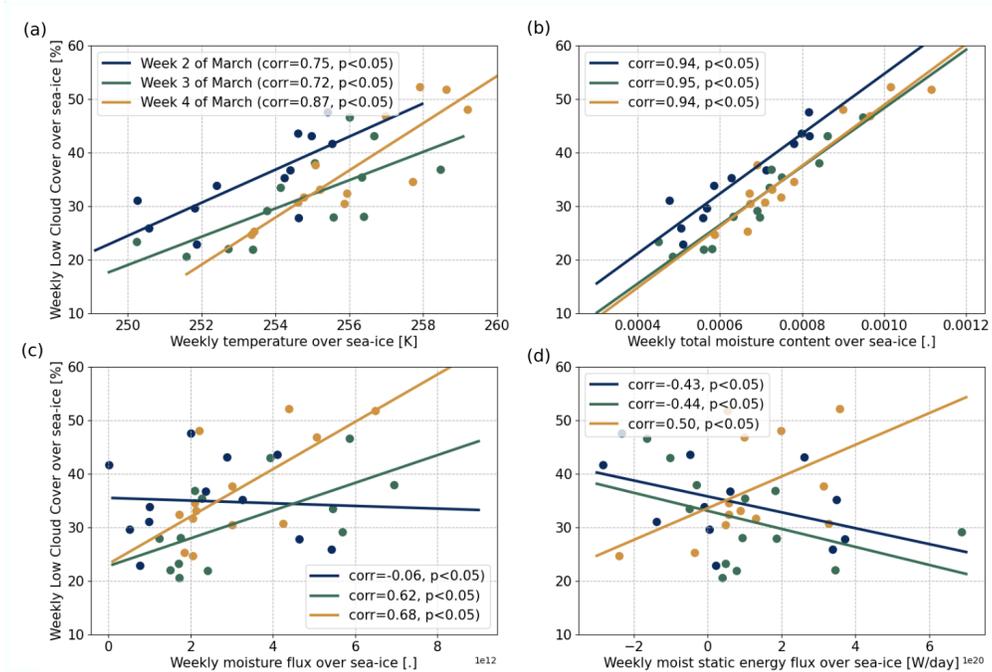


Figure 1: Relationship between weekly average low cloud cover over sea-ice and (a) lower troposphere air temperature, (b) moisture content, (c) poleward moisture flux and (d) poleward moist static energy flux.

- A second point is that the authors should investigate (in link with the interannual variability of cloud occurrences mentioned above) additional parameters influencing cloud occurrence variability. For example, since the study focuses on low clouds, it is important to examine their coupling with the surface. In addition, the link of cloud occurrence with warm air intrusions should be analyzed, including back trajectories to assess air mass transport.

We agree with the reviewer that to investigate the daily variability of cloudiness over sea-ice, an approach looking at air-mass transformation for single moisture intrusion events would be of high interest. However, the objective of this manuscript is different as we focus on explaining the consistent, pan-Arctic seasonal transition in low-level cloudiness, rather than diagnosing the mechanisms controlling individual events. For this purpose, we consider that quantifying (1) the seasonal evolution of large-scale moisture transport into the sea-ice domain and (2) the temperature control on cloud phase provides a relevant explanation for the observed seasonal increase in optically thicker, liquid-containing low clouds. Processes such as surface coupling/decoupling, aerosol–cloud interactions, and event-scale air-mass transformation are certainly important, but beyond the scope of the present seasonal-attribution analysis. We now clarify this scope in the manuscript and highlight event-based air-mass transformation as a direction for future work (L434-436).

- Finally, the present study considers the entire sea ice covered surface only. This area is very large, and figure 1 suggests that the increase in cloud occurrence is more pronounced in certain regions (for example near open oceans: Barents, Kara and Chukchi seas). Therefore, an analysis of the spatial variability of cloud occurrence in specific representative regions with different environmental conditions would be valuable. This could also include areas over open ocean or land, which present highly contrasting conditions in terms of temperature and humidity.

The primary scope of this study is to identify the processes that control the surface energy budget of sea ice during the spring transition, and thus to understand mechanisms directly relevant for sea-ice evolution. Therefore, we decide not to study more in details the spring cloud onset over the open water regions.

The regional discrepancies in spring cloud onset over sea-ice and the links to synoptic conditions are the focus of a full independent separate manuscript submitted in September 2025 currently under review.

### 3 Minor comments

- L 30 : “depending on definition of melt onset”: please give a brief summary of the commonly used definitions.

We agree with the reviewer and added information L34.

- L90 : Please explain how cloud properties could be influenced by boundary-layer processes.

We added one example of how boundary-layer processes can influence cloud properties L90.

- L 98: Please indicate the units of ATB.

We agree with the reviewer and added the unit L128.

- L 106-107: Multilayers clouds are also present. How are you sure that these unclassified are composed of liquid and not ice?

It is true we cannot state that the unclassified layers are ice only, and therefore we analyze it throughout the study as a probable hypothesis. Nevertheless, this hypothesis was further investigated by Cesana et al., (2016) using aircraft measurements, showing that it is likely that ice layers are overrepresented in the unclassified case over the Arctic.

- L 116: Why figure 6 is mentioned before fig 3, 4 and 5 ?

We agree with the reviewer and decided to remove this reference (L150-155)

- L118-122: As the authors explain that unclassified layers are clouds, why these layers are not taking into account in the cloud phase ratio?

We agree with the reviewer that it was not clear enough that we also computed the cloud (ice) phase ratio by considering unclassified cloud phase layers as ice layers in Fig.7. We added additional content in the Data section (L157)

- L118: You should change “cloud phase ratio” into “Ice phase ratio” or something else referring to “ice phase”, according to its definition. It will be more convenient to interpret.

We agree with the reviewer that “cloud phase ratio” was confusing and changed all instances for “ice phase ratio”.

- L132: The limit for low-level thick clouds (or ice cloud ratio) in terms of SR is not very pronounced in the figure 3 ( or in figure D1). Could you explain why you used the value of  $SR = 30$  as a threshold? Would using another value significantly change the results?

The value of  $SR = 30$  was chosen based on visual inspection of the evolution of the SR-altitude histograms. We added now Fig.3 these histograms averaged over 2-week periods to emphasize on the increase of this low-level optically thick white box (top panels). Looking at it, we can conclude that choosing another threshold would not critically change the results. The method was also clarified L174-180.

- L127-136: Please, explain with more arguments the interest of the histogram and the two categories you highlight?

The SR-altitude histograms are used to illustrate how the vertical distribution and optical thickness of 480m atmospheric layers evolve from March to May. In particular, they highlight two categories that are central to our interpretation. (1) The low-level optically thick category emphasizes the spring transition toward more opaque low clouds (2) The “probable thin ice” category emphasizes that CALIPSO detects frequent thin ice layers below the standard GOCCP cloud threshold. These layers are not included in the ice-cloud fraction based on ( $SR > 5$ ), but are consistent with the frequent presence of atmospheric ice particles (with Ice Water Content below the approximate CALIPSO-GOCCP detection limit of  $2.5 \times 10^{-3} g.m^{-3}$  based on Lacour et al., (2017)). We added these explanations L174-180.

- L162-167: There was some confusion when reading this section. I understand that CALIPSO-GOCCP may miss some thin ice clouds when  $SR < 5$  and you use the ground based observations for ensure that ice clouds could be detected when  $SR > 3$ . But, how can you be sure that the satellite and ground based lidars have the same cloud sensitivity? Could you for example provide the frequency distributions of the SR for the two systems (co-located along the MOSAIC track)?

We agree with the reviewer that the sensitivity of both lidars might be different. However, we cannot provide a quantitative comparison as the MOSAiC campaign was located in the North Pole blind zone of CALIPSO (over 81°N). Nevertheless, the motivation is not only set using observations by the MOSAiC lidar but also on the study by Lacour et al., (2017) showing that CALIPSO-GOCCP threshold do not allow to capture thin ice particles in the Arctic. This study was performed by doing quantitative comparison of Scattering Ratio with ground-based lidar (Summit Station).

- Figure C1: I see more clouds for SR between 1 and 3 on the figure ? You wrote the opposite in the text? This figure need more explanation, and you could add it in the manuscript.

Fig.C1 indicate that moving the SR threshold for cloud detection from 1 to 3 does not modify the ice cloud fraction. This means that no "objects" are detected between these two values of scattering ratios. While between 3 and 5, the ice cloud fraction decreases drastically, meaning that most of the ice clouds are detected in the SR range of 3 to 5. We modified Fig.C1 caption and expect the figure to be more redeable now.

- L183: "To understand low cloud formation.....humidity and temperature." Other parameters may impact the cloud formation, such as dynamics, surface coupling, stability, local sources or long range transport of aerosols... (see my major comment above)

We refer here to our answer for the second general comment.

- Section 2.1: I suggest to include open seas and MIZ areas. (see the major comment)

We thank the reviewer and answered this in the major comments section.

- L260-262: "Moreover...relative to March". This sentence is unclear. Please, rewrite.

We agree with the reviewer and modified L328-330 to make the point clearer.

- L265-280: For the blocking event, could you provide maps similar to figure F1 to identify the moisture decrease? In addition, do you have hypothesis for this cloud occurrence increase since it is not due to moisture advection ? A more detailed meteorological analysis of the situation would be helpful. Back trajectories could provide useful insights insight into air mass transport, and give an indication of aerosol transport. Moreover, this section well illustrates that cloud occurrences and moisture transport can vary substantially on a daily scale, and that this variability has to be considered, in addition to the annual averages presented in the paper.

We thank the reviewer for pointing out this omission and have added Fig. F2. We agree that synoptic-scale conditions can strongly influence Arctic cloud cover on daily timescales, superimposed on the seasonal cycle discussed here. We now emphasize the importance of investigating this daily variability further in the conclusion (L435).

- Figure 4: Blue light curves not really visible. Please improve.

We thank the reviewer for this review and modified Fig.4.

- L 284: "from" missing between "observations" and "Andreas et al"

We agree with the reviewer and modified L351.

- L 319: How does figure 7b change if you use only the cloud layer to determine the temperature distributions?

We provide a comparison of the Fig.7b temperature distributions computed using (i) all atmospheric layers and (ii) cloudy layers only. As expected, restricting the sample to cloudy layers shifts the distribution toward warmer temperatures. The shift is largest in April, which we interpret as reflecting

the greater contribution of cloudy layers in late April compared to early April, when temperatures are also warmer. We did not include this additional figure in the revised manuscript.

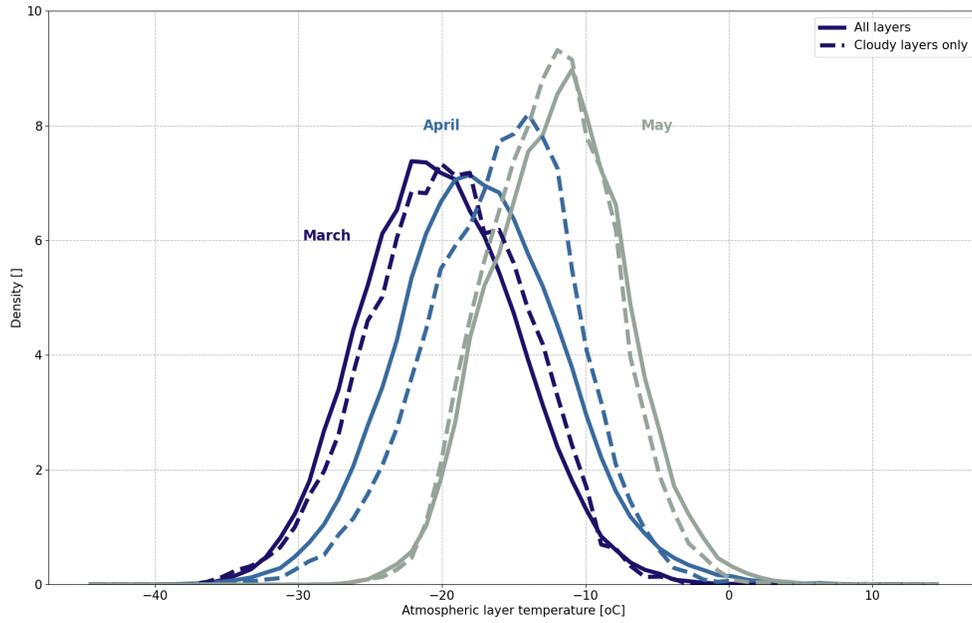


Figure 2: Temperature probability density function by considering all atmospheric layer as done originally in Fig.7 of the paper or considering cloudy layers only.

# Answers for Reviewer 3

First, we would like to thank the third reviewer for its time and for its positive and constructive review. Below we respond to the specific comments. As requested by the reviewer, we also show some additional results and revised the text in the manuscript.

## 1 Overview

The manuscript investigates the Spring cloud onset over the Arctic sea ice and examines different mechanisms (two of which were proposed in past studies) that could drive that increase in cloud cover. For this, the authors use 13 years of CALIOP lidar observations to identify seasonal trends in cloud cover over the Arctic sea ice. Reanalysis data is used for temperature and humidity calculations, while ground-based lidar measurements and radiosoundings during MOSAiC are used to compare the CALIOP climatology to a case study in Spring 2020. Several lines of argumentation point to the fact that the Arctic Spring cloud onset is connected to an increase in temperature and, consequently, a decrease in the efficiency of ice cloud processes, which drives a transition towards low-level, liquid-containing clouds.

Seen in the context of climate change, a Spring cloud onset that is mainly connected to a temperature increase might get triggered earlier in the coming decades, with potentially large implications for the radiative balance in the Arctic. Leveraging a long time series of spaceborne lidar observations, this paper marks an important and timely contribution to this research field. Thus, the manuscript fits very well within the scope of the journal.

The manuscript is structured in a very clear way, and the figures support the line of argumentation. I structured my review in the following way: I will start with a couple of general comments that I think should be addressed by the authors, followed by more specific comments below. Finally, I will give some hints for technical corrections.

## 2 General comments

- Due to the polar orbit of CALIPSO, there is a well-documented sampling bias at higher latitudes that can influence statistics when profiles are aggregated over a larger domain covering different latitude bands. The much larger number of profiles close to 82°N will dominate the overall sample domain statistics, if not addressed e.g. by utilizing random sampling with replacement. This might be especially important as these areas are farthest away from the moisture source of the ocean. I suggest that the authors consider this sampling bias and address it accordingly or, if already implemented, specify the how in the Data section (Sect. 2).

We agree that the present methodology should tackle the sampling bias by CALIPSO at high latitudes. We now apply an updated method (L163-167) which ensures that all 1° latitude bands have a similar number of profiles considered. We replaced Fig.6 and 7 (both use the instant CALIPSO observations) using that new method.

The figure below shows the difference between the new version of Fig.6 and the ancient one. As an example, the blue tick at  $-1^{\circ}C$  indicates that, at this temperature, the fraction of liquid-containing layer observed over all type of layers decreased by 1% (from 9% to 8%) between the two versions. Overall, this figure highlights that the partitioning difference of each layer types between the new and the ancient version of Fig.6 is below 1% except for temperatures below  $-35^{\circ}C$ , corresponding to a few number of occurrences. Therefore, no critical change are observed between the previous and the

current versions of Fig.6. Same conclusions are made for Fig.7 although we do not show here the difference between the new and the ancient version.

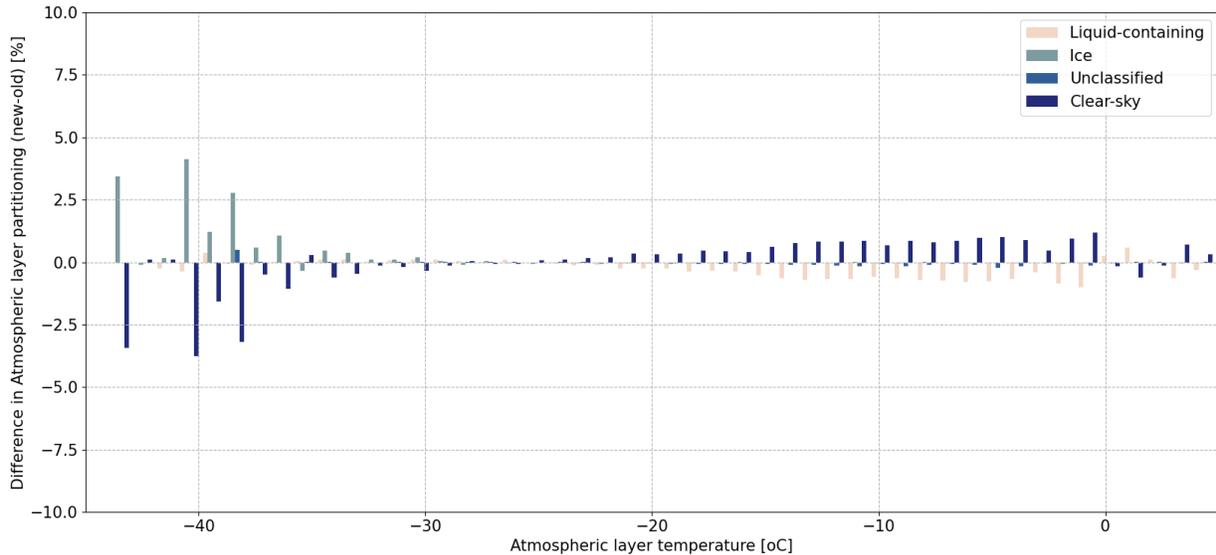


Figure 1: Difference between the new and the ancient version of Fig.6.

- Connected to the first comment, I feel like uncertainties related to the datasets used could be discussed more comprehensively. The sensitivity study and SR thresholds as well as the radiosounding intercomparison between MOSAiC and ERA5 go in that direction, but statements regarding the uncertainty in cloud cover estimates are missing. In addition, how do e.g. multi-layer cloud situations complicate the analysis or is the study limited to single-layer clouds? I am not so familiar with CALIPSO-GOCCP, but maybe that is already handled? I suggest adding a bit of information here.

We agree with the reviewer that a sentence is missing on the previous evaluation of the CALIPSO-GOCCP product and therefore information were added L188.

About the multi- vs single-layer clouds, we consider each 480m thick atmospheric layer (330m along orbit trackx90 across track) independently of whether they are located in a profile including other 480m thick cloudy or clear-sky atmospheric layers above or below. In this perspective, a single-layer cloud can be observed on several consecutive 480m thick atmospheric layers and a multi-layer cloud is only the succession of cloudy and clear-sky 480m thick atmospheric layers within the same profiles.

- One the main arguments regarding the driver of the Spring cloud onset in this study is the availability of enough moisture to ensure saturation w.r.t. ice already way before the Spring cloud onset in March. However, for these low clouds below 3.2 km, based on Fig. 2, it looks like the clouds are still well within the mixed-phase temperature range above  $-38^{\circ}\text{C}$ . It is my understanding that most of the ice clouds that form above  $-38^{\circ}\text{C}$  also require saturation w.r.t. water (unless the ice crystals have fallen from above and originally formed via pore condensation and freezing as the nucleation pathway). However, this process requires ice-nucleating particles, which are quite rare in the Arctic. Having that in mind, I wonder whether the saturation-argument w.r.t. ice is truly sufficient to explain the consistent ice cloud cover?

We agree with the reviewer that the moisture saturation w.r.t ice is a necessary but not sufficient condition for the formation of ice particles. For the case study of MOSAiC soundings, 60% of saturation w.r.t ice occurrence and no saturation w.r.t liquid within a profile were detected in association to ice particles detected by the MOSAiC lidar. Although the saturation condition is not sufficient, it is likely that most of the time there are enough INP or sedimentation of ice particles at higher altitudes to form new ice particles in the lower troposphere. We added a clarification on that L354-356.

Arctic glaciated clouds are actually not always observed with saturation w.r.t liquid. Ice particles can form also via deposition onto INP without saturation w.r.t liquid and also via sedimentation as highlighted by the reviewer (Pruppacher).

- I am wondering about the moisture intrusion analysis. It looks like this is done using a vertically integrated quantity, which is dominated by the boundary layer moisture coming in from over the ocean, pushing the cold dome farther north. Would it not be more accurate to look at the moisture at the cloud levels investigated here? Otherwise, one might not see a cloud response as the moisture intrusion investigated here happens mainly below the cloud layer.

I would like to clarify some of the points expressed by the reviewer. We show in the figure below the profile of convergence moisture flux for March, April and May integrated over the same contour as in Fig.4. This figure, in agreement with the literature (Naakka et al., 2018), shows that the maximum of moisture transport is around 0.5-1km. Therefore, since most of clouds over sea-ice are located below 1km altitude, with a maximum in the boundary layer (Fig.2), the maximum transport of moisture occurs mainly at the same level/slightly above most of Arctic clouds. We also do not see any change in the vertical structure of moisture transport between March and April, when the spring cloud onset occurs.

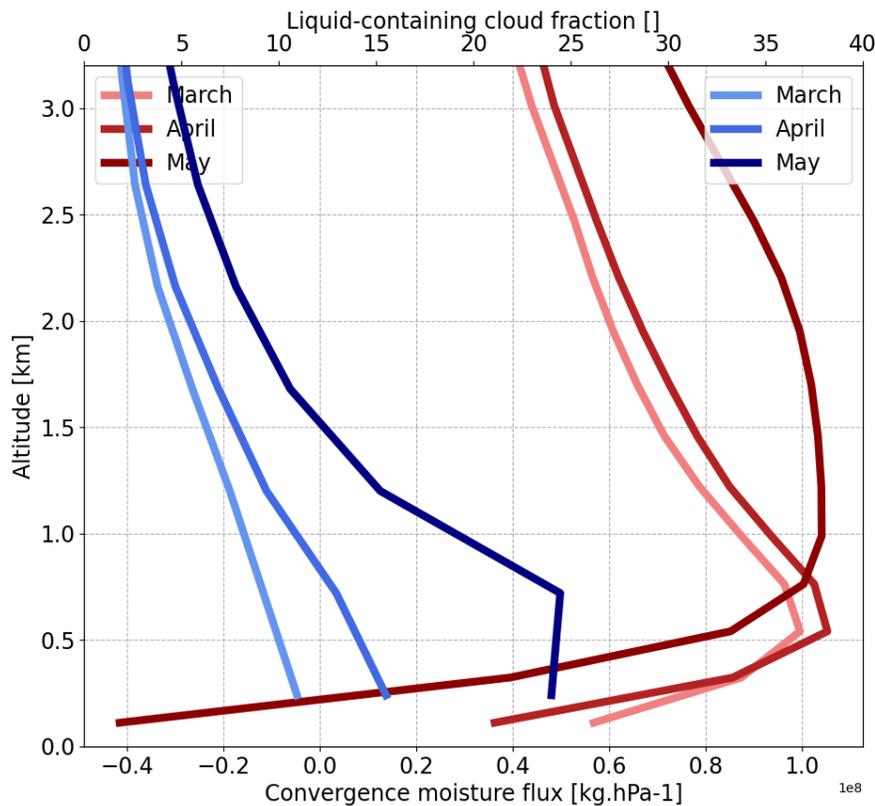


Figure 2: Mean monthly profiles of convergence moisture flux from the mid-latitudes (red curves) and liquid-containing cloud fraction from CALIPSO-GOCCP (blue curves).

- Regarding the latitudinal dependence of the timing of the Spring onset: I agree that this would in principle be an argument against some of the hypotheses discussed in the study. However, and related to my comment above, I wonder about the definition of the contour for the calculation of the moisture flux and the validity of relating it to a domain-averaged low cloud cover (Figure 4). I suggest using only intrusions from the ocean to the sea ice (and exclude the Canadian Arctic for example). Otherwise, the signal might get averaged out. Fig. 1 (bottom center panel) suggests that the Spring cloud onset indeed starts in those regions (Atlantic sector and Pacific sector).

We agree with the reviewer that a sensitivity study is needed here on the Fig.4 contour. Looking at the figure below, the poleward moisture flux is now computed by considering fractions of the contour over the North Atlantic and the Bering Strait only and the remaining contour over Siberia and North Canada. Overall, we do not see a steep seasonal increase of poleward moisture flux in any of the sectors related to the timing of the spring cloud onset. While it is true we observe a slight increase in the Pacific Sector in mid-April that might trigger the spring cloud onset slightly earlier in the Laptev Sea than in the Beaufort Sea, we also observe a slight seasonal decrease of the moisture flux in the Atlantic sector, although both region experience the spring cloud onset around mid-April. This shows again that regional differences in poleward moisture flux superimposes onto the existing seasonal cycle but does not modify it. We added a sentence on the sensitivity of the results to this "sector approach" in the main text L323-325.

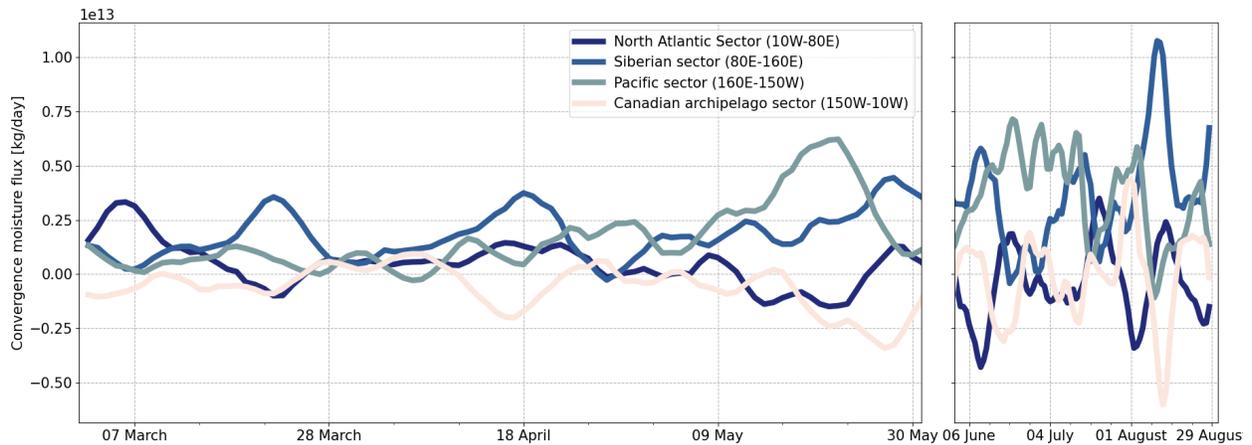


Figure 3: Caption

### 3 Minor comments

- Line62: I think it would be helpful to mention a specific example of how boundary-layer processes can influence low-level cloud properties here.

We agree with the reviewer and added one example L90 from Shupe et al., (2013).

- L102: Please provide a bit more detail here, such as that cloudy layers with cross-polarized ATB above this threshold are considered ice clouds, and layers with values below the threshold are considered liquid clouds.

We agree with the reviewer and added information L133

- L122: Please add how the statistics differ from previous studies, as I believe this to be important information for the reader.

We agree with the reviewer and added information L156

- L133: Category identification. Could you please specify why you chose altitude  $\geq 1.0$  km as an additional criterion for the low-level liquid clouds? From the inset of Fig. 3 it looks like an extension to 1.5 km could be feasible to increase statistics, but maybe the logarithmic colorbar for the frequency is tricking me a bit? It would be interesting to hear the reasoning behind. In addition, I like the two ways of looking at it, altitude vs. SR (Fig. 3) and phase ratio vs. SR (Fig. D1) histograms. However, it would be more illuminating in my eyes if Figure D1 would also be color-coded with the frequency, or maybe even better: have the same altitude vs. SR plot from the inset in Fig. 3, but instead of color-coding by frequency, color-code it by phase ratio. That way it should become immediately clear that the low-clouds with  $SR \geq 30$  are liquid-containing clouds.

We agree with the reviewer that the methodology to specify SR categories boundaries was not clear. Therefore we decided on Fig.3 to add the SR histograms for each 15-day periods. The definition of the SR-altitude low-level optically thick white box is based on visual inspection of the time evolution of these histograms (information added L175-180). We kept the bottom panel of Fig.3 to highlight clearly the frequency evolution within each white boxes.

It is true that such a representation of SR histograms color-coded with phase ratio would be more striking to highlight the relationship between SR and cloud phase. Therefore, we replaced Fig.D1 using the SR-altitude histograms build for each cloud phase type presented in Cesana et al., (2013).

- L133: Could you please include what percentage of cloud fraction is covered by the two categories introduced here, to get a feeling for how many cloudy layers are not included in one of the two categories?

We agree with the reviewer that such information was missing. (i) We added such quantification for the white optically thick box L177. (ii) Then, we remind here that the "probable ice detection" category is not included in the the cloud fraction Fig.1 and 2 as it is defined for a SR threshold ( $3 < SR < 5$ ) below the one commonly used by CALIPSO-GOCCP ( $SR=5$ ), we added L179 a sentence to clarify it.

- L165 and Figure C1: I do like the sensitivity test of ice cloud fraction to the SR threshold, but it is unclear to me what the different lines represent in Figure C1? Please specify in the text and also the figure caption.

We thank the reviewer for pointing this omission and we added additional information needed in Fig.C1 caption.

- L193: For the computation of the biases between ERA5 and MOSAiC radiosondes, why did you choose altitudes of 2km instead of the 3.2km, which is used for the computation of the cloud statistics? I think it would be best to be consistent here.

We agree with the reviewer, but we observe most of the bias close to the surface, and therefore averaging up to 3.2km altitude would give the impression of almost no bias from ERA5. We added this information L245.

- Section 2.3: The fact that ERA5 assimilated the MOSAiC radiosondes makes me wonder whether this section is a bit misleading. If you compare the temperature and humidities for one location that is fine, but the author's themselves mention that ERA5 likely performs worse at other locations and times. I understand why the section is included, and why it is hard to evaluate reanalysis in the Arctic. However, I feel stating the absolute biases like this is a bit misleading.

We agree with the reviewer that this comparison is limited by the assimilation of MOSAiC radiosondes, but we decided to keep it as a useful order-of-magnitude estimate of ERA5 biases.

- Section 3.2: I suggest including Fig. 3 as a sub panel in Fig. 2, as they are directly connected and having them side-by-side will foster comprehension by the reader. At the same time, I suggest removing the inset figure and make it a separate figure in the Data section (Section 2), potentially with a second histogram color-coded by phase ratio instead of frequency (see my comment to L133 above). That way methodology and results are a bit better separated in my eyes. It would also help as the SR frequency histograms are already discussed in Section 2.1 and are used for the definition of the two categories.

We decided to keep Fig.2 and 3 as separate figures to keep a clear separation between the main seasonal statistics (Fig.2) and the SR-altitude diagnostics used to motivate the white box categories (Fig.3). However, in response to the reviewer suggestion, (i) we removed the SR-altitude inset from Fig.3 and instead added it as a set of top panels, with SR histograms summed over 2-week periods, to more clearly illustrate how the distributions evolve from March to May and to justify the choice of the highlighted white boxes. (ii) Following the comment on L133, we also provide an additional version of the histogram color-coded by the cloud phase ratio in the Supplementary Material.

- Data availability: LWCRELIDAR data is only mentioned in the data availability section, but it is unclear where this data has been used in the study?

We thank the reviewer for pointing out this omission, we removed the statement on the LWCRE-LIDAR

## 4 Technical corrections

- Figure A1: Please increase the font size of axis and colorbar labels, caption: ‘based on the hourly solar angle only’ (remove additional ‘only’)  
We modified Fig.A1 as suggested by the reviewer.
- L85: Sections 4 and 5 explore [...]   
We thank the reviewer for pointing this omission and modified L114.
- L98:  $2.5 \times 10^{-3}$   
We thank the reviewer for pointing this omission and modified L128
- Equation 2: Please double-check the equation, because if I compare with Cesana and Chepfer (2013), there a several missing minus signs in the exponents of 10, also it should be made a bit clearer that this is  $\times 10^{-3}$  or  $\times 10^{-4}$  etc.  
We thank the reviewer for pointing this omission. The implentation of it for the present study was already using the correct discretisation line.
- Figure 2: Colorbar label liquid-containing cloud fraction closing bracket for unit ‘]’ missing  
We thank the reviewer for pointing this omission and corrected Fig.2
- L119:  $1^{\circ}\text{C}$  temperature bins  
We thank the reviewer for pointing this omission and modified L150.
- Figure 3: inset maximum scattering ratio label reads 100,000, whereas caption mentions range from 3 to 10,000. Please correct accordingly and check the main text for consistency.  
We thank the reviewer for pointing this omission and modified Fig.3.
- L134: optically thinner  
We thank the reviewer for pointing this omission and modified L178.
- Figure D1: Please specify that this is the ratio between ice and all clouds.  
Fig.D caption was modified based on comments from several reviewers.
- Figure B1: legend entries ‘resol vertical’  
We thank the reviewer for pointing this omission and modified Fig.B1.
- L169, L191: radiosonde  
We thank the reviewer for pointing this omission and modified L220 and L223.
- L176: Section 2.1 (remove additional dot)  
We thank the reviewer for pointing this omission and modified L227.
- L190, L196: radiosondes  
We thank the reviewer for pointing this omission and modified L241.
- Figure 2 caption: Colorbars are from [...] for the liquid-containing [...]   
We thank the reviewer for pointing this omission and modified Fig.2 caption.
- Figure 3: the inset is covering parts of the low-level thick liquid line.  
We modified Fig.3.
- Figure 4: caption ‘and captures most of the Arctic sea-ice extent in spring’  
We thank the reviewer for pointing this omission and modified Fig.4 caption.

- L254/255/257/258:  $\times 10^{13}$ . Please also correct all other instances.  
We thank the reviewer for pointing this omission and modified every instances.
- Figure 6: Caption and legend label: liquid-containing clouds instead of liquid clouds  
We thank the reviewer for pointing this omission and modified Fig.6 legend.
- L308: we further observe  
We thank the reviewer for pointing this omission and modified L377.
- L323: from generally ice-cloud dominated in March to liquid-containing cloud dominated in May  
We thank the reviewer for pointing this omission and modified L392.
- L351: over liquid-containing clouds  
We thank the reviewer for pointing this omission and modified L420.
- L355: at the expense  
We thank the reviewer for pointing this omission and modified L424.
- L370: before the end of the century  
We thank the reviewer for pointing this omission and modified L451.