

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 ϕ the geopotential.

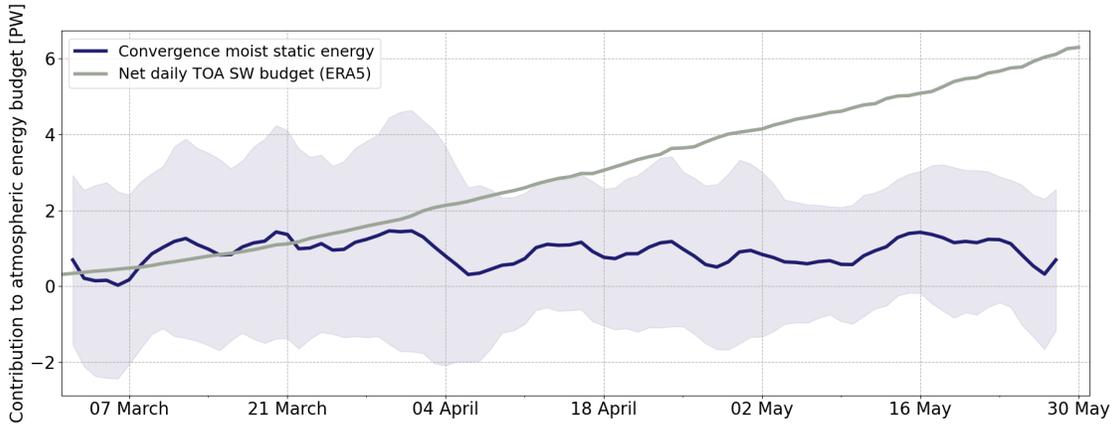


Figure 1: Contribution of horizontal transport of moist static energy and solar radiation to the total atmospheric energy stored over the sea-ice. The net top-of-the atmosphere shortwave radiation budget (grey line) is integrated over the surface defined in Fig.4 of the present article. The convergence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

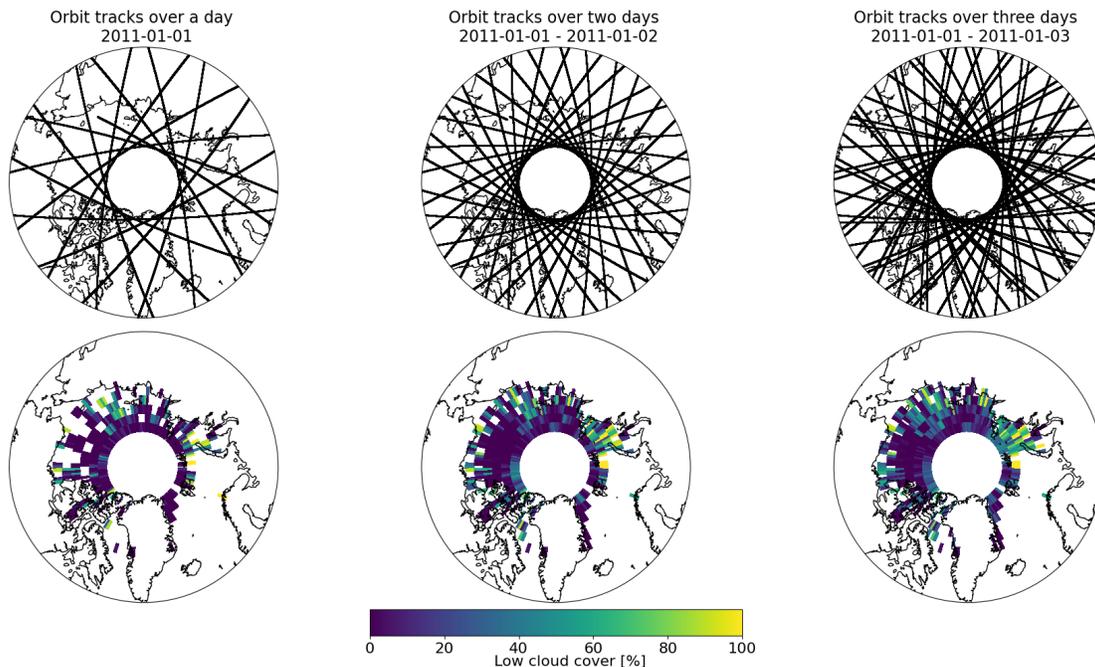


Figure 2: (top) Every CALIPSO orbit tracks over one, two and three days. (bottom) Maps of Low cloud cover averaged over one, two and three days from CALIPSO-GOCCP $2^{\circ} \times 2^{\circ}$ daily gridded data.

- 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