

# 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  $-1^{\circ}\text{C}$  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  $-35^{\circ}\text{C}$ , 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.

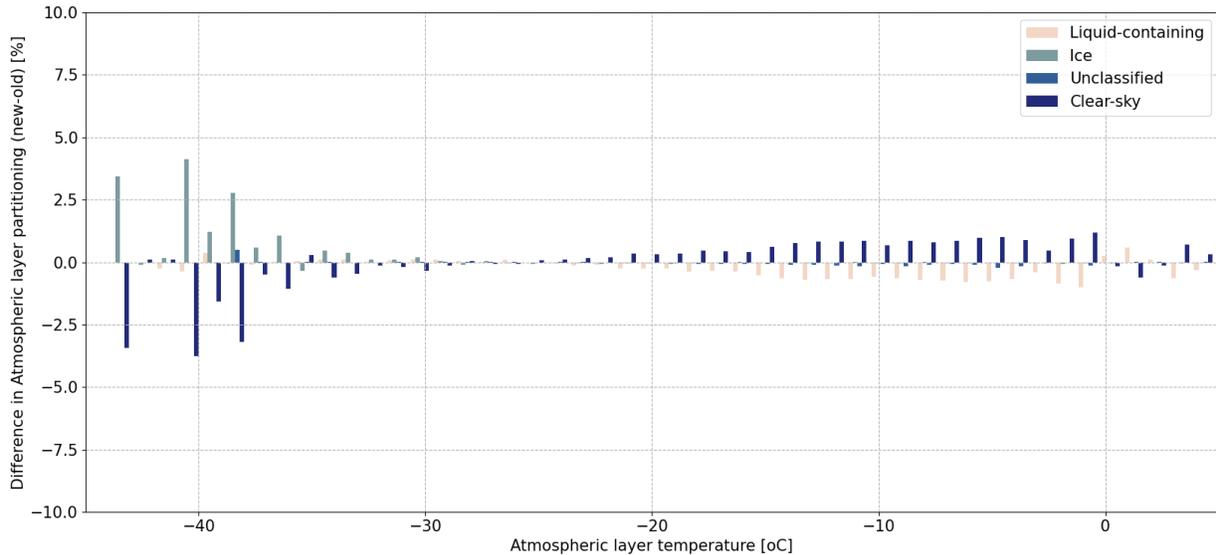


Figure 1: Difference between the new and the ancient version of Fig.6.

- 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.

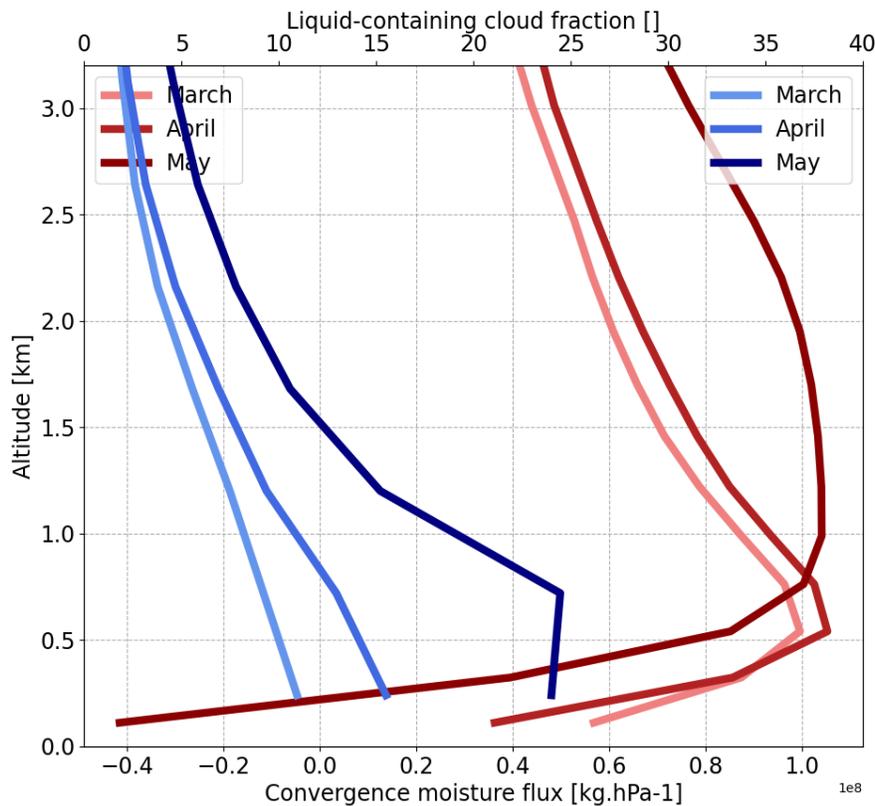


Figure 2: Mean monthly profiles of convergence moisture flux from the mid-latitudes (red curves) and liquid-containing cloud fraction from CALIPSO-GOCCP (blue curves).

- 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.

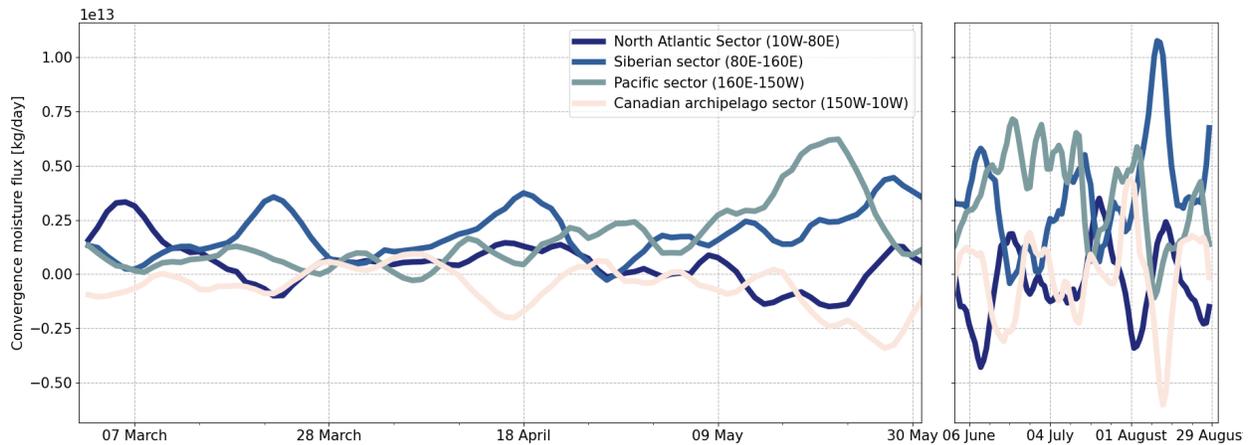


Figure 3: Caption

### 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.