

Answers for Reviewers

First, we would like to thank the reviewers for their time and for their positive and constructive review. Below we respond to the specific comments. All line numbers refer to the revised manuscript with tracked changes.

1 Answers for reviewer 1

- L422 (in the marked-up version): "The difference of temperature in the lower troposphere between March and May is great enough that March temperatures statistically favor more ice production at the expense of liquid droplets and May temperatures allow for relatively more liquid droplet formation." I'm not entirely sure about that argument still. Isn't in fact the difference between saturation vapor pressures over liquid and over ice largest at -13°C , corresponding to the most efficient WBF process? This corresponds very well to the most-frequently observed temperatures in May (Fig. 7b). At colder temperatures, WBF is also getting more limited again (albeit with a slower decrease than on the warm side of -13°C). Overall, based on WBF theory and the observed temperature distribution, I would maybe expect a more efficient WBF in May than in March?

We thank the reviewer for this comment. We agree that the difference between saturation vapor pressures over liquid and ice peaks near -12°C , and that this can favor rapid depositional growth of ice in mixed-phase conditions. However, the efficiency of the WBF process in controlling the ice phase ratio depends not only on this saturation difference but also on supersaturation and on temperature-dependent microphysical processes (Pruppacher and Klett., 1997). In addition, other temperature dependent processes can shape the ice phase ratio, including the temperature dependence of INP activity. Accordingly, we did not add additional material to attribute the temperature dependence of cloud phase to specific microphysical processes, as such an analysis is beyond the scope of this study. We note that the original version already avoids attributing the observed temperature dependence of the ice phase ratio to the WBF process alone.

- Supplementary Fig. I: You define the Spring cloud onset around 7 April, but then in Supplementary Fig. I show correlations for 3 weeks in March when not much change in low-level cloud cover is observed (see Fig. 1). Wouldn't it be better to in fact choose weeks within the spring cloud onset period to see if the increase in cloud cover is connected to those variables?

We agree with the reviewer and decided to replace Fig.I1. We added the relation between the cloud cover and the different parameters for each weeks of March, April and May.

- Supplementary Fig. H1: I suggest using the same colorbar range (90%) and colormap as for Fig. 1 to be consistent. Could you maybe also choose 1-3 days in March or April here instead of January? I know the orbits won't change, but if it's on the same colormap it would strengthen the argument even more if a similar pattern as visible in the two-week averages of Fig. 1 emerges after 3 days already.

We agree with the reviewer and decided to choose three days of the studied period with the colobar consistent with Fig.1.

- Fig. 3: I'm wondering a bit about the colorbar for the SR histograms. I'm not entirely sure why the frequency is shown on a logarithmic scale (this distracts from the most important bins)? In addition, large parts of the colormap are not being used (white, blue, green) as they correspond to very low frequencies. I suggest to adjust the scale and ranges accordingly.

We agree with the reviewer and came up with a new version of Fig.3 that is highlighting more efficiently the increase of low-level liquid-containing layers using a linear color scale.

- Fig. 1 caption: See Supplementary H for CALIPSO spatial sampling (not I).
We thank the reviewer for pointing out this mistake and modified the text.
- Fig. G1 caption: “top-of-the-atmosphere” (delete leading “n”)
We thank the reviewer for pointing out this mistake and modified the text.
- Supplementary section G should also be referenced in the main text in my eyes, and not just be a standalone discussion.
We thank the reviewer for this suggestion and agree that Supplementary.G should be more clearly cited from the main text. We have therefore added explicit references to Section G both where the springtime temperature increase over sea ice is introduced (L56) and again in the conclusion (L391). We note that we keep this material in the Supplementary Information because it provides supporting background rather than a primary result, but we agree it is relevant whenever the seasonal temperature increase over sea ice is discussed.
- Fig. 7: The contrast between the orange and pink lines is not very good, I suggest to change one of the colors for better readability.
We thank the reviewer for pointing out this mistake and changed pink for green, and uploaded a new version of Fig.7.
- Supplementary Fig. I1: Panels (b) and (c) are missing units for the x axes.
We thank the reviewer for pointing out this mistake and modified it.

2 Answers for reviewer 2

- Line 430: Equation G1 The vector symbols and dot product are incorrect here.
We thank the reviewer for pointing out this mistake and modified Eq.G1.
- Lines 286 – 289: “We investigated. . . sufficient moisture supply.” I find this sentence and the following sentence of results confusing. The meaning of “fraction of the contour” is unclear without a map, and without a figure of results I find the explanation confusing. The presentation of this information should be improved by making the wording more explicit if figures are to remain absent. Alternatively, adding figures that show the described data would improve the communication.
We agree with the reviewer. We decided to add the actual figure in the Supplementary material with a map of the different segments of the contour (Supplementary.J). We also use ”segment of the contour” instead of ”fraction of the contour” for a more comprehensive wording.
- The authors perform an additional analysis of the moist static energy convergence that is referenced in the introduction, but it is elsewhere missing from the manuscript. I think it would be useful to reference and briefly describe this additional analysis where it is relevant (e.g. in the description and analysis of figure 4).
We thank the reviewer for this comment and agree that the additional moist static energy (MSE) convergence analysis should be more clearly referenced where the spring temperature increase is discussed. We have therefore added a citation to Supplementary.G in the main text L391.