REVIEWER No. 3

The article "Asymmetries in winter cloud microphysical properties ascribed to sea ice leads in the central Arctic" studies the impact of the presence of leads on cloud properties based on data from the MOSAiC campaign. The authors efficiently use the synergy of different datasets to constrain several parameters (coupled vs. decoupled cases, different lead fractions...). The study highlights the importance of considering the leads to study the properties of Arctic clouds. The dataset, the method and the analyses seem to be robust and the results are convincing. The authors show some interesting results: For example, the lead fraction has an important influence on the cloud thickness and on the ice water path. I have a few comments (see below) that I would like the authors to consider, but the topic and content of the paper are within the scope of the journal, so I recommend publication.

General comments:

1. I wonder if the authors looked at the effect of melt ponds or what they think about it. Would it have a similar effect as the leads (maybe weaker effect)?

   Reply: As reported by (Creamean et al., 2022), melt ponds occurrences coincide with high concentration of INPs sampled during the MOSAiC expedition. Therefore melt ponds can be thought as sources of nucleating particles necessary for cloud formation. However, as also shown by (Creamean et al., 2022) in their Figure 2, the occurrence of melt ponds happens during the Arctic summer mainly after May, when air temperature is close to zero or even slightly above 0°C, which makes melt ponds not very efficient as sources of sensible heat. On the contrary sea ice leads are efficient sources of latent and sensible heat during winter where air temperatures ranged from -45°C to -10°C. This is one reason our manuscript focuses in the wintertime, so the effects be leads can be stressed and better isolated from the cloud observations. This remarks are added to the manuscript introduction in lines 57 to 64.

2. The authors use the Cloudnet dataset based on observations to retrieve cloud properties. Like any observation, Cloudnet should have an error in the observations and in the retrievals, but this is not shown here. I expect that this uncertainty should appear in the results, or at least be discussed.

   Reply: This is an important observation and a column in Table 2 has been added with the uncertainties reported by the literature where retrievals are based on. Moreover, a sensitivity study has been performed for cases where the radar reflectivity is modified by ±3, ±10, and ±20% of the original values and found that the final ice water path (IWP) relative error ranges from ±1.9% to ±13%. See answer to reviewer 2 for detailed explanation.

3. Methodology: The considered wind profile is measured at the RV Polarstern and not at the leads. Therefore, I think Figure 4 is misleading, but I understand that it is considered constant between the leads and RV Polarstern: How true is this hypothesis, have the authors quantified the possible biases from a change in WVT along the way (between the lead and Polarstern)?

   Reply: Thank you for pointing out this, the manuscript's Figure 4 has been modified to indicate the wind profile is measured at the RV Polarstern. In order to assess the hypothesis that the wind direction at maximum WVT gradient can be assumed constant *within the 50km radius considered in the manuscript, we performed back-trajectory analysis using the Lagrangian back-trajectory tool Lagranto* (Sprenger & Wernli, 2015) *. The trajectory of WVT is tracked from the altitude where the wind direction is taken i.e. maximum gradient of WVT, this shows that the assumption is quite plausible since the back-trajectories show a considerable agreement with the assumed wind direction within the 50km radius. In Figure 1 is shown the back-trajectory for WVT (gray band) at different times. Only between 18:00 UTC and 20:00 UTC it has been observed that the back-trajectory moves earlier towards west as compared with the wind direction showing a deviation between the conical sector (black lines) and the back-trajectory, which can be explained by inaccuracies on the hourly ERA5 input for Lagranto. The animation showing the back-trajectories for the entire day is added as supplementary material*.

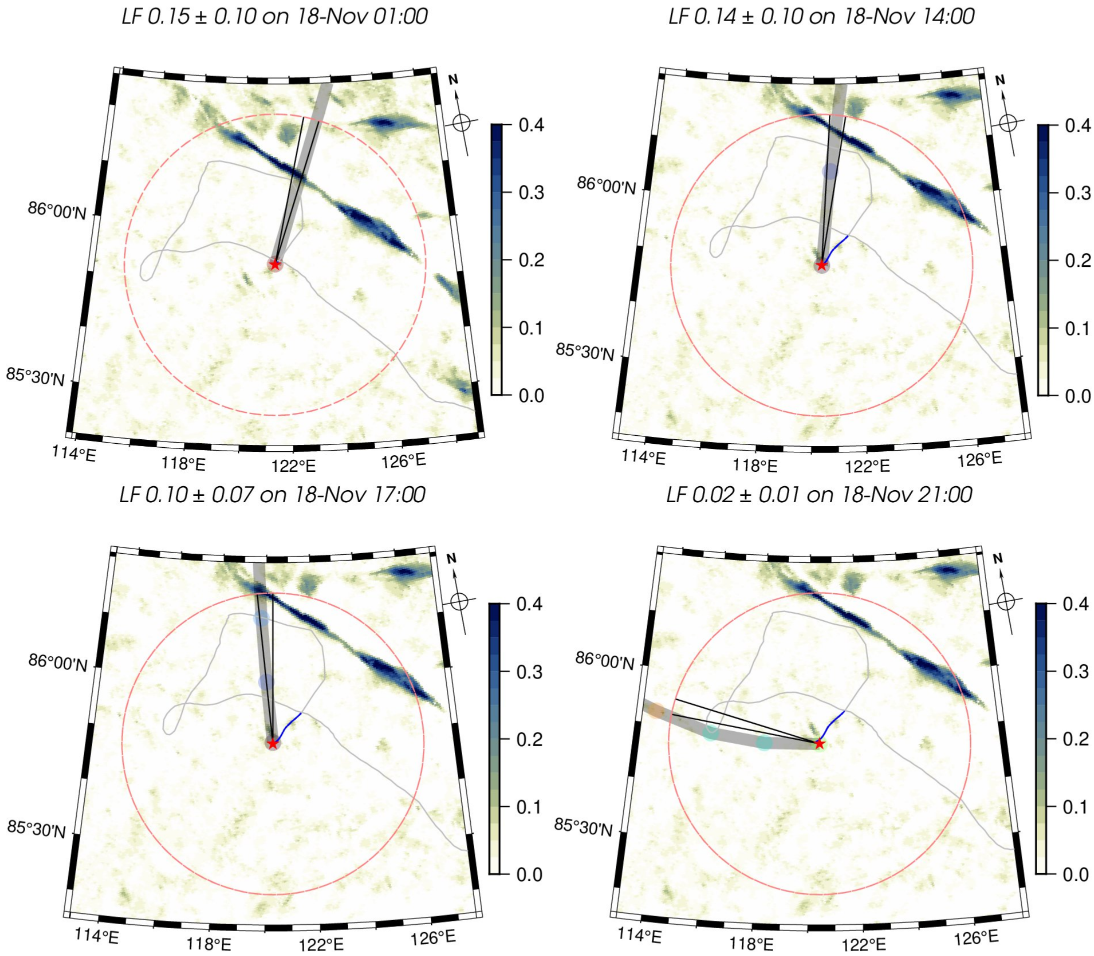

Figure 1 Sea ice lead fraction from November 18th, 2019. The two radial back lines encompass the conical sector considered within 50km radius for the analysis based on the wind direction. The gray band indicated the WVT back-trajectory path using Lagranto, with the circles along the path indicating hourly backward steps of the trajectories.

4. Do the authors look at what types of clouds we are mainly looking at in the study? In terms of mixed-phase clouds, are they mainly the typical mixed-phase clouds with precipitating ice below a liquid layer, or are they more mixed?

   Reply: Mixed-phase clouds are the dominant cloud type during Arctic winter and MOSAiC. In the analysis we are considering non-precipitating mixed-phase clouds as well as clouds with precipitating ice below the liquid layer as it is shown in the case study from November 18th, 2019. We are starting to do further research whether or not the amount of precipitating ice below the liquid layer has any correlation with the presence of *sea ice leads, but for the current manuscript precipitating mixed-phase clouds are not separated from the analysis.*

5. Figure 7: From the plot, it appears that clouds are mixed phase in the coupled situations and only ice in the decoupled situations. Is this always the case? Can the authors comment on this? And the next question is: Could we use the presence of mixed phase to detect coupled situations (or vice versa)? Could we use the presence of leads (and coupled situations) to detect mixed phase clouds (or vice versa)?

   Reply: Thanks for the question. The case shown in the manuscript's Figure 7 can be misleading in the sense that mixed-phase clouds can also be classified as decoupled since the classification is based on the location of WVT relative to the cloud layer. As shown in manuscript's Figure 9 (a) and (d), decoupled mixed-phase clouds are also present in the analysis. Regarding the second question: Although our results show that coupled mixed-phase clouds are more frequent, it is not feasible to identify those mixed-phase clouds directly as coupled since there are still a considerable amount of decoupled mixed-phase clouds with and without lead fraction. Based in our statistical analysis it can be infered that coupled mixed-phase clouds occur with a 63% frequency when LF>0.02, but we cannot detect the mixed-phase clouds only based on the presence of leads.

6. I was wondering if the authors considered the effect of aerosols. Leads could be a source of marine aerosols and therefore affect the thermodynamic phase of the cloud. I guess Polarstern might have measurements of this. The increase in moisture would be the most important effect on cloud properties, but aerosols might not be negligible.

   Reply: Aerosols in fact have been observed during MOSAiC. In particular samples of ice nucleating particles (INP) have been collected during MOSAiC. (Creamean et al., 2022), showing that INP concentration are found to be persistent among the months from Octover to April mainly between the range of temperatures from -25°C to -15°C (Creamean et al., 2022) , with higher INP amount sampled during periods with high lead occurrence and wind speeds above 5 m s^{-1}. Therefore , as highlighted by (Creamean et al., 2022) the high fractional occurrence of ice in clouds below 3km (low-level clouds) in winter implies that observed small INPs could serve as important role in cloud ice formation. However due to the fact that the surface is predominantly frozen the local source of INPs is locally limited. Thus, it is plausible to support the hypothesis that leads play an important role as sources of sea spray by windy conditions during the wintertime. We consider that the sea ice leads as sources of INP like sea-spray can be advected along the WVT, and therefore included in our analysis as part of the coupled/decoupled classification. However, since no continues INP sampling has been performed, we cannot separate our dataset based on INP concentration.

Minor comments:

1. Title: I do not like the term "Asymmetries" because it emphasizes more a difference than an asymmetry. Also, I found that this term is more associated with geographical differences, but that may be just me, but I recommend changing the title.

   Reply: Thank you for the comment. We use the term Asymmetries to emphasizes the differences found between coupled and decoupled clouds at different sea ice lead fraction states. Therefore it is highlighted that the methodology for coupling the sea ice leads with the clouds via the WVT mechanism, proposed in the manuscript, has *uncovered asymmetries in the statistical distributions of coupled and decoupled clouds. In other words, if the methodology presented were able to produce symmetric coupled/decoupled distributions then the conclusion would be that sea ice leads coupled to clouds via WVT have no effect on the cloud properties*.

2. Abstract: There is a lot of technical details in the abstract that could be removed here.

   Reply: Thank you for the suggestion, the abstract has been simplified.

3. Line 42: We usually refer to the Wegener-Bergeron-Findeisen process. Then the citation Wegener 1911 could be added
   Reply: Thank you for notice that. We refer now as the Wegener-Bergeron-Findeisen process and the citation has been updated.

4. Wegener, A. (1911). Thermodynamik der atmosphäre. JA Barth
   Reply: Thank you for the information, the citation has been updated.

5. Line 118: "We note that leads…" Do the authors mean that in this situation the leads are considered to be sea ice? If so, I wonder if they quantify the error from this.
   Reply: This has been reported by (Rückert et al., 2023) during warm air intrusions (WAI) in April 2020 where thin ice hampers some sea ice concentration retrieval methods. One reason to use the merged MODIS-AMSR2 SIC product is to circumvent this problem since the MODIS sensor is sensitive to thermal emission thus not affected by the thin ice events. The estimate error can be observed in the manuscript's appendix Figure C1 where the April WAI events overestimate sea ice openings up to 15%. The Sentinel 1A lead fraction product is based on the divergence/convergence of the sea ice, therefore not sensitive to thin ice but rather to the relative movement of the sea ice.

   Line 160: "described as following", at first, I thought the authors were explaining the identification of sea ice leads, but they are describing the potential influence of leads on cloud properties. I suggest changing the sentence.
   Reply: The identification of sea ice leads has been extensively explained by (von Albedyll et al., 2021) and it is not within the scope of the manuscript. In line 160 we were describing the methodology used to link the sea ice leads with the clouds via the water vapour transport. The sentence has been simplified and adapted to highlight this (now Line 165) "The conceptual model proposed to identify the influence of sea ice leads on the cloud properties observed aloft the RV Polarstern 's central observatory is depicted in Fig. 4 and described as following:"

6. Line 166: The acronym CO is confusing because it is already used for coupled. Perhaps it is not necessary to have an acronym for Central Observatory since it is not used that often.
   Reply: Thank you for the suggestion, CO has now been dismissed as acronym in the manuscript.

7. l. 180: are of having -> are having
   Reply: Thanks you for the correction, it has been amended.

8. l.190: "meaning the lidar signal is attenuated by low-level liquid clouds", I am not sure I understand. Does this mean that the algorithm does not detect the low level cloud, but the signal is attenuated by the cloud and therefore the measurements are biased? Have

the authors quantified the effect on the results?
Reply: This means the lidar is mostly attenuated by detecting the lowest liquid cloud. Cloudnet will classify this lowest cloud as pure liquid or mixed-phase cloud, but in case a second layer of mixed-phase cloud is present above then Cloudnet is not able to classify it as mixed-phase since no information from the lidar is available due to the signal attenuation. This is a well-known limitation of classification algorithms based on radar-lidar synergy. For our analysis we consider only one single cloud layer, and based on our statistical results we found that coupled clouds are mostly low-level clouds therefore less likely to be affected by the limitation of the Cloudnet classification due to lidar signal attenuation. Moreover, a recent algorithm development by Schimmel et al., 2022) has demonstrated that this issue can be improved by exploiting the information from cloud radar Doppler spectra. Application of this technique to the MOSAiC data however requires further data analysis and will be considered for future work, which was indicated at the end of the Discussion and Outlook section, now in Lines 524-526 in the updated manuscript.

9. l. 201: The constant g appears in equation 1, so it should be defined there.
Reply: Thanks for noticing this, constant g is now described after equation 1.

10. l. 273: CMLH of below -> CMLH or below
Reply: Thanks for pointing out the error, this has been corrected.

11. l. 277: to be take -> to be
Reply: Thanks for pointing out the error, this has been corrected.

12. l. 332: “clear”, I would be careful with the term "clear" because some points are not within 3 sigma. Also from Figure 8, I wonder how the fit is done.
Reply: Thank you for the comment, “clear” has been deleted. The fit is done by using a power relationship between LWP and LF, i.e. $LWP = a*LF^b$, with a and b fitting constant. This has only been done to highlight the relationship found within variables and not implying a physical law that link these two properties. Same for the fit in manuscript’s Figure 11.

13. l. 347: “Figure 8 … -10° C km-1” I am not sure I understand the sentence, can the authors rephrase it?
Reply: We apologize for the confuse phrasing. The sentence has been simplified and rephrased as “Figure 8 (d) indicates that Γ_{cloud} is often close to the moist adiabatic lapse-rate 6.5 °C km^{-1} (dashed horizontal line in Fig. 8(d)). The negative Γ_{cloud} values represent cases with a temperature increase within cloud layer or inversion at cloud top”.

14. Figure 11 caption: the subscripts (c) and (d) are not correct.
Reply: Thank you for noticing this. It has been corrected.

15. Section 5 Conclusion and Outlook: What is missing is a discussion section where the various results are summarized. This is done in Figure 4, but I would go a bit further before the conclusions. I do not think much is needed, but just highlighting what the results bring to the model would be enough.
Reply: A paragraph has been added after the list of conclusion points at the Discussion section: “The findings presented here can be used as valuable constrains to evaluate cloud microphysical parameterizations for the Arctic system. The different features on ice

water fraction χ_{ice} found when coupled and decoupled cases as a function of cloud top temperature are analyzed is a result that deserves to be deeply investigated by validating it with long-term observations but also by a better understanding of the model's cloud physic that can lead to explain the finding."

16. l. 559: WVF -> Do you mean WVT?
Reply: *Thank you for notice this. It has been corrected*.

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