Response to the reviewer comments on the manuscript:
"Airborne observations of cloud properties during their evolution from organized streets to isotropic cloud structures along an Arctic cold air outbreak"

We thank the two anonymous reviewers for diligently reading and carefully reviewing our manuscript and providing us with useful comments and suggestions to improve the quality of the manuscript. A list of all reviewer comments and questions (written in ***italics***) as well as our response (written in regular) is given below. Whenever we provide information in which line changes were made we refer to the line numbering of the revised manuscript.

Comments Reviewer 1:

This paper investigates micro- and macrophysical properties and transition of marine boundary layer cloud in an Arctic cold-air outbreak over the Norwegian Sea. It also investigates the role of surface heat fluxes, buoyancy forces, and vertical wind shear.

Major comments

1. *The Introduction does not develop the raison d'être of this study. It provides a textbook summary of cloud streets, then summarizes two papers (one from 1996 with airborne observations and one from 2023 with satellite observations). Then the reader is left with the question why, where does this lead? Nowhere. What follows is the standard paper section summary, then Section 2. What is the objective? Literature should only be cited as part of a rationale for the study.*

We appreciate the reviewer's feedback on the introduction. Our intention to give a summary on the available studies on cloud streets is the complexity of the processes that lead to the formation of cloud streets in CAOs. This aims to bring all readers to the same level of understanding needed to fully understand the following analysis. However, we agree, that the discussion was partly to long and deviated from the main scientific question. Therefore, we sharpened the introduction. Our discussion specifically addresses cloud streets within the context of CAOs and roll convection, highlighting the contested mechanisms driving roll formation, particularly in CAO scenarios, a complexity often simplified in introductory texts.
To do so, we discussed here the phenomenon of cloud streets in the context of CAOs and roll convection and their dynamic causes. Especially we emphasized that the reasons for roll convection (and cloud streets) are controversial discussed in the literature and that classical theoretical mechanisms for roll generation does not explain rolls in several CAO situations.

2. *Section 5 explores the dynamic reason for the observed change in cloud depth and cloud macro/microphysical changes during repeat passes over the same location. The changes over the course of a few hours are relatively minor, and a variety of synoptic to mesoscale factors may have contributed to this, e.g. changes in surface wind speed or surface heat fluxes, or large scale subsidence. As an example, Fig. 1 reveals a remarkable mesoscale convergence band emerging from the apex of ice-free ocean north of Svalbard. Clearly this feature is east of the area of interest (although it may well have been included in passes #2 and 4, see Fig. 4b), but other leads (polynyas) could generate mesoscale circulation stretching far downwind. Alternatively, a slight change in flow trajectory and thus fetch from the ice edge or MIZ could bring about the observed change. I recommend examining multiple satellite images, as well as Arome Arctic model output, including Lagrangian trajectories.*

   We do not rule out the impact of changes of mesoscale factors. Indeed, the observed change of windspeed and stability parameter did change for some reason. This can be one of the given parameters (wind speed, surface flux, subsidence). However, all these factors finally change the stability parameter -H/L that we analyzed. As we argue, that the stability is the major indication for changing the cloud horizontal structure, there is no urgent need to investigate the primary cause of the change. Of course, it would be highly interesting to identify the major mesoscale driver, but this is beyond the scope of our manuscript.

3. *If I understand correctly, the estimation of -H/L in Section 5.1 only depends on the wind speed. This is an oversimplification. The wind speed supposedly is measured at a level of 90 m, and dropsonde data are used for this. Are the 4 times mentioned in Table 1 the 4 dropsonde times, associated with the passes shown in Fig. 5a? The bulk aerodynamic approach has its limitations (e.g., assumptions about the exchange coefficients), and normally is applied to 10 m wind and 2 m T/q, not 90 m wind (Brümmer's soundings may have lacked vertical resolution?). One cannot assume that the 90 m temperature is constant, in fact Fig. 7e shows a variation of ~1K, and H varies too (as mention in the Abstract). The eddy correlation fluxes approach (eqn 5) is much preferred, even at a FL above 90 m within the convective BL, and it appears that the data are available. This approach requires averaging over a substantial distance, and this integral approach results in a more representative outcome than a point measurement. Spaceborne Synthetic Aperture Radar (SAR) surface wind speed imagery shows significant small-scale variations in wind speed in the cloud street regime, presumably associated with coherent roll circulations, in other words, the wind speed variations are not representative of a larger area and do not demonstrate a trend. I do not place much credence in the -H/L values listed in Table 1, and I would not classify regimes based on the apparent small variations across the -H/L threshold of 15. I cannot agree with the conclusion arising from Sections 5.1 and 5.2. CARRA does show a weakening of the BL wind over a 9 hr period. In short, the paper does not prove its basic conclusion stated in Section 6: "For $-H/L < 15$, turbulence generated by wind shear dominates, favoring the formation of organized cloud streets. However, as $-H/L$ increases beyond this threshold, buoyancy becomes increasingly significant, and the organized cloud streets collapse into more isotropic cloud patterns. In our case study, the reduction in wind speed is the dominant factor increasing $-H/L$, disrupting the organized convection which is required to sustain cloud streets."*

-H/L depends here only on wind speed, because H almost does not vary here in time (as can bee seen in Fig.7e) as well as the sea surface temperature and the temperature in the CBL. So, this is not an oversimplification but comes from the observation within the time period analyzed. Using a bulk approach to calculate -H/L of course is a simplification. We agree that calculating -H/L using the fundamental equation (5) would be desirable, but turbulent flux measurements were not available at that positions and times to identify the transition from cloud streets to isotropic cloud patterns. We are aware about that the bulk approach is a rough estimation and discussed that critically in the last paragraphs of section 5.1 also in conjunction with other bulk parameters used in other studies for roll convection in similar way. And yes, the profile data exhibits fluctuations (same as in Brümmer (1996)), as no time average is available. Anyhow, as mentioned in the last paragraph of section 5.1, the reduction in wind speed which increases the values of -H/L seems to be not just a fluctuation, as it can be seen through the entire CBL from 12.35 UTC. We used the 90m-level (instead of 2m) not only for comparability with Brümmer (1996), but also for technical reasons. The dropsonde measurements have a vertical resolution of 5 m within an altitude range below 1000 m, like mentioned in line 70. Attempting to derive reliable data at a 2 m altitude implies a finer resolution than the specific operational capability. Therefore, very close to the surface (2m) the estimation/interpolation of the wind speed and temperature for that height is very uncertain, as close to the surface temperature as well as wind speed hold the largest vertical gradients there. Just to give you an idea, the fall rates can be around 11 – 13 m/s. This means the sonde passes through the final 2 m in a fraction of a second (roughly 0.15 to 0.18 s).
We added the following explanation for the choice of the 90m-height (line 252):

„Dropsonde data for U_{90} and air temperature were taken from the 90 m level to ensure reliability, given the 5m vertical resolution of the measurements and to minimize uncertainties associated with the lowest few meters above the surface. “

In addition, due to the uncertainty of estimating -H/L, the threshold of -H/L<15 cannot be interpreted as a strong fixed threshold. However, the tendency of the -H/L stability parameter derived from the dropsonde measurements agree with the observed cloud structure. Smaller -H/L values are linked to cloud streets because shear is dominating, while larger values can be linked to isotropic cloud patterns, because buoyancy is dominating. To make that clearer in the manuscript we added the following sentence (line 272):

“We like to mention that the critical value of 15 for −H/L should not be understood as a switch for free rolls or no free rolls. With increasing values from about 15 the pattern of free rolls can be expected to become more and more unclear.”

4. *Finally, the writing style can be improved. The text is very figure-centric. Rather, the figures should merely serve to confirm statements in the text, e.g., “A similar trend, but with higher uncertainty, is also visible in Fig. 6b, which shows the correlation between A and B” is better stated as: Consistent with this, A and B are negatively correlated, although the correlation is weaker (Fig. 6b). There are many other examples.*

We thank the reviewer for highlighting the figure-centric writing style in the previous version of the manuscript. We agree that the figures should primarily serve to support the statements made in the text. We have carefully revised the manuscript to emphasize the observed trends and relationships.

5. ***In general, caution is warranted with the interpretation. This paper is based on a remarkably small dataset, and slim evidence.***

   Thanks for this comment. Reviewer 2 had a similar comment, and we agree that the sample size is limited, as our conclusion is based on a single case study with repeated airborne observations and dropsonde profiles. While we do not claim statistical generality, the strength of our finding lies in the temporal consistency: the transition from organized cloud streets to isotropic cloud patterns occurs in parallel with a clear and continuous decrease in wind speed in the boundary layer, as shown by the dropsonde profiles and CARRA data. To make that clearer in the text, we revised the summary and conclusion (Section 6) and clarified that this finding is based on a case study (line 348):

   „While this finding is based on a single case study, it provides valuable observational support for theoretical and modeling studies that propose wind shear as a critical factor in maintaining cloud street organization."

Minor comments

L26: "The inflection point in the vertical cross-roll wind profile turned out to be too weak for that kind of dynamic instability". The inflection point cannot be too weak. Is it that the cross-roll wind shear is too weak?

Thank you for that comment. Indeed, the wording here was not precise. We meant:
"...the cross-roll wind shear at the inflection point in the vertical cross-roll wind profile turned out to be too weak..."
Nevertheless, we sharpened the introduction and made it more precise. That's why we removed this sentence.

L36: "… had in common that a moderate CAO was simulated …" Do you mean that helical roll circulations were simulated? The CAO simply is a large-scale condition of cold air advection and strong surface heat fluxes.

We actually meant the following:

"… cannot be responsible for the simulated roll development in that CAO situations".

But because we restructured the introduction, we also removed this sentence.

L38: "the more TKE production by wind shear and the less by buoyancy plays a role in the entire ABL" better: … the more TKE production in the ABL is dominated by shear rather than by buoyancy.

Thanks, we revised the sentence as follows (line 33):
"The smaller the value, the more TKE production in the ABL is dominated by shear rather than by buoyancy."

L38: define the Monin-Obukhov length here, rather than on line 252. Or at least mention the definition in words here.

Thank you for your suggestion. We agree that introducing the Monin-Obukhov length (L) earlier improves clarity. We changed the text at line 31 to include a brief definition:

"Herein H is the top of the ABL and L the Monin-Obukhov stability length, which characterizes the relative influence of buoyancy and shear on turbulence generation. The smaller the value, the more TKE production in the ABL is dominated by shear rather than by buoyancy."

The full mathematical definition remains in Section 5.1, where it is directly applied in the analysis.

Fig. 3: the cloud fraction identification through separation across the red/blue channel ratio is not clear to me. What is the physical basis? An IR camera probably would have been better. The obvious limitation evident in Fig. 3a,b, that a slant view overestimates the nadir view albedo for any cloud of finite thickness, should be mentioned.

Thanks for this comment. The identification of cloud fraction using the red/blue channel ratio is based on the difference in spectral reflectance between clouds and the ocean surface. Clouds reflect more in the red and near-infrared spectrum compared to the open ocean, where absorption is stronger in the red and near-infrared bands. This contrast allows us to separate between cloud-covered and cloud-free areas.
We agree, an IR camera would have been beneficial for cloud detection. However, given the available instrumentation, the red/blue ratio method provided a practical and effective way to estimate cloud fraction.

Furthermore, we acknowledge the limitation that the slant view from the fish-eye camera may overestimate the nadir-view albedo for clouds of finite thickness. We have now added a statement in Section 3, line 157:

"It should be noted, since the fish-eye camera images are taken at an oblique angle, the derived cloud fraction may be slightly overestimated compared to a nadir view, especially for thicker clouds. This effect should be considered when interpreting the cloud fraction results."

L221: pls provide more detail about how M is computed, limitations, and why M=0.01 can be used as threshold for (un)rimed particles. I suspect it uses in situ microphysics data only. Maherndl et al. (2024) describe two techniques.

We used the in situ method from Maherndl et al. (2024) and have added additional information in the revised manuscript, line 210:

"To derive M, we used the in situ method from Maherndl et al. (2024), which is based on in situ observations of particle shape. Only a subset of particles can be used to derive M (for CIP, particle diameters must be larger 210 µm; for PIP, larger 1400 µm), because small particles have round shapes due to imager resolution. For a detailed description of the method and its limitations, we refer the reader to Maherndl et al. (2024). We consider particles with M < 0.01 to be unrimed due to their nearly identical scattering properties to particles with M = 0 (Maherndl et al., 2023)."

L226: lack 4 ◊ track 4

Thanks, we corrected it to "pass 4".

On L228, it is stated that "Cloud streets show a stronger shear at cloud top with higher turbulence (higher TKE)" and "isotropic cloud patterns show a stronger buoyancy". This is not demonstrated yet in the paper.

Thanks for this observation. We acknowledge that this is not stated in the paper and revised the sentence in line 221 to reflect this more cautiously:

"Cloud streets are typically associated with stronger vertical shear at cloud top, which enhances turbulence (higher TKE), while the transition to isotropic cloud patterns coincides with a reduction in wind shear, suggesting a shift toward buoyancy-driven convection."

Fig. 5a: does it show the flight level of Polar 6 only? I see only 4 tracks, one for each pass. On L88, Polar 5 flight level is mentioned to be 1000 m above cloud top. How well synchronized were the two aircraft? Apparently not well in 2 of the 4 tracks.

Thanks for the question. Figure 5a represents only the flight level of Polar 6, which conducted in situ cloud particle measurements. While Polar 5 primarily performed remote sensing observations from approximately 1000 m above cloud, in an altitude of ~3000 m. The aircraft were indeed well collocated throughout the flight. To show you, please take a look at this little clip (https://speicherwolke.uni-leipzig.de/index.php/s/ijdpK4F5TGptb8H), which shows how well both airplanes were collocated during this whole research flight. To clarify that, we added the following sentence in Section 4.2, line 196:

"Polar 5 and Polar 6 were well collocated during the flight. Polar 5 flew slightly behind Polar 6 to enable dropsonde deployments without affecting the in situ measurements from Polar 6. As a result, the measurements from both aircraft are directly comparable."

In addition, we want to mention here that we achieved also a good collocation during pass 1 and pass 3. However, Polar 6 was flying on different altitudes and that’s why we don’t compare the microphysical properties for these passes.

L295: “because other parameters which might control the appearance of free rolls here, keeps constant” ◊ because other parameters appear to vary less than wind speed?

Thank you for this suggestion. You are right, the original phrasing was unclear. We changed it to, line 290:

”… because other parameters appear to vary less than wind speed.”

Fig. 7: I suggest monotonically changing hues (color intensities) for the 4 different times. Easier to interpret

Thanks for your suggestion. We considered your suggestion adjusting the color scheme to use monotonically changing hues. However, we believe that the current color scheme effectively differentiates the four time steps. In particular, the two distinct colors clearly differentiate between cloud streets and isotropic clouds, which is a key aspect of our analysis.

Fig. 8: “CARRA wind field on 4 April 2022 …” I suggest changing this caption : “… at 430 m ASL, which corresponds to cloud top height near point DS”

Thank you for your suggestion. Upon reviewing the figure, we realized that the originally stated altitude of 430 m was incorrect. The correct altitude is 90 m, and it does not represent cloud top height. We have updated the figure caption accordingly to:

“CARRA wind field on 4 April 2022 at an altitude of 90 m for different time steps (a to d).”

Comments Reviewer 2:

This study uses airborne observations to investigate the temporal evolution of cloud street structures and how changes in these structures affect both macrophysical and microphysical cloud properties, as well as the radiation budget. The authors introduce a new method called the "cloud street index" to describe the transition from organized cloud streets to a more isotropic cloud pattern.

Major comments:

1. Readers can easily become confused by the various flight paths and dropsondes from different aircraft. The terms "specific location", "the same location", "the before mentioned location" are repeated throughout this manuscript, leading to further confusion. It would be helpful to replace these phrases with "location DS" and provide latitude and longitude coordinates for different points. Additionally, consider modifying Figures 1, 5(a), and 7 to clearly illustrate which flight paths and dropsondes correspond to each aircraft.

Thanks for the comment. We agree that the repeated use of vague phrases such as "specific location" or "the same location" may cause confusion, especially given the involvement of multiple aircraft and dropsonde deployments. We now consistently refer to the dropsonde deployment site as "DS location" throughout the manuscript. The coordinates of DS (79.1218°N, 3.0574°E) have been added to the caption of Figure 1and in the text. And we updated the Figure captions of Figure 1 and 5a.

2. One of the major findings of this study is that "decreasing wind speed drives the transition from cloud streets to isotropic cloud patterns." This conclusion is primarily based on the vertical profiles from five dropsondes and the CARRA wind field at four different times. However, the sample size used to support this conclusion is quite small and insufficient to definitively claim that decreasing wind speed is the cause of this transition. While we did observe decreased wind speeds in isotropic clouds, this does not necessarily indicate that the decrease in wind speed is responsible for driving this transition.

Thanks for this comment. We agree that the sample size is limited, as our conclusion is based on a single case study with repeated airborne observations and dropsonde profiles. While we do not claim statistical generality, the strength of our finding lies in the temporal consistency: the transition from organized cloud streets to isotropic cloud patterns occurs in parallel with a clear and continuous decrease in wind speed in the boundary layer, as shown by the dropsonde profiles and CARRA data. To make that clearer in the text, we revised the summary and conclusion (section 6) and clarified that this finding is based on a case study (line 348):

„While this finding is based on a single case study, it provides valuable observational support for theoretical and modeling studies that propose wind shear as a critical factor in maintaining cloud street organization."

We also want to mention here that we do not rule out the impact of changes of mesoscale factors. Indeed, the observed change of windspeed and stability parameter did change for some reason. This can be one of the given parameters (wind speed, surface flux, subsidence). However, all these factors finally change the stability parameter H/L that we analyzed. As we argue, that the stability is the major indication for changing the cloud horizontal structure, there is no urgent need to investigate the primary cause of the change. Of course, it would be highly interesting to identify the major mesoscale driver, but this is beyond the scope of our manuscript.

Minor comments:

line 29-30: It is unclear whether "this study" refers to this manuscript or Brown's 1972 study.

This line and the lines before are in the context of the simulation of Etling and Raasch (1987). We did the following changes to make that more clear:

Line 23: "Etling and Raasch (1987) investigated the development of cloud streets by boundary layer rolls in CAOs. They showed that the inflection point instability, often discussed as a reason for boundary layer rolls in theoretical studies does not explain the typical cloud structure.…"

line 47-48: Since both the strength of the CAO and the surface heat flux play important roles in cloud structure and its transition, why doesn't this study explain the exclusion of the analysis of CAO strength and surface heat flux?

You are right, heat flux is already included in "strength" of the CAO. We removed that.

line 121: What does this mean "quantified by the cloud top altitude". Are cloud top heights in Figure 4b retrieved from AMALi?

Thank you, that really wasn't clear. In this sentence, "quantified by the cloud top altitude" simply means that the cloud top altitude is measured. The cloud top heights presented in Figure 4b are indeed retrieved from AMALi (Airborne Mobile Aerosol Lidar) measurements. To avoid potential confusion, we have revised the sentence for clarity, line 98:

"The data are used to determine the vertical structure of cloud layers, with cloud top altitude as a key measured parameter, having an accuracy of roughly 7 m (Mech et al., 2022; Schirmacher et al., 2023)."

line 171-172: Why can the ratio of the red and blue channels be used to determine the cloud fraction? How is the 0.8 threshold determined?

Thanks for this question. The identification of cloud fraction using the red/blue channel ratio is based on the difference in spectral reflectance between clouds and the ocean surface. Clouds reflect more in the red and near-infrared spectrum compared to the open ocean, where absorption is stronger in the red and near-infrared bands. This contrast allows us to separate

between cloud-covered and cloud-free areas.
The threshold value of 0.8 was selected empirically by visually inspecting multiple images and identifying a reasonable separation between cloud-covered and cloud-free areas. To clarify this in the manuscript, we have revised the relevant section as follows, line 151:

"Cloud pixels were identified based on the ratio of the red and blue channels, as clouds generally exhibit higher reflectance in the red spectrum compared to the blue. A threshold of 0.8 was chosen based on visual inspection of multiple images to ensure accurate cloud detection."

line 182: It appears that the cloud fraction significantly depends on the size of the camera images. If two camera images overlap, could this impact the cloud fraction results?

Thanks for your question. The camera images were taken every 10 seconds, and we have verified that they do overlap. This means that some cloud structures may appear in multiple images. However, since the aircraft is continuously moving forward, each image still captures a new portion of the cloud field. While overlap could introduce some redundancy, the cloud fraction is calculated independently for each image, so the overall trend remains valid.

To clarify this in the manuscript, we have added the following statement, line 159:

"In addition, because the images are taken every 10 s, they partially overlap, meaning that some cloud structures may appear in multiple images. However, since the aircraft is continuously moving forward, each image still captures a new portion of the cloud field, and the overlap does not significantly impact the cloud fraction determination."

line 212-213: Are passes 2 and 4 from Polar 6 or Polar 5?

Thank you for your question. Passes 2 and 4 correspond to Polar 6, which conducted in situ cloud measurements. Polar 5, on the other hand, performed remote sensing observations and launched dropsondes. To clarify this, we have updated the text in Section 4.2 to explicitly state that the microphysical measurements discussed in this section originate from Polar 6, line 195:

"In situ cloud measurements of microphysical properties from Polar 6 are available for passes 2 and 4 of the case study, providing detailed observations of cloud particle distributions at low altitudes."

Line 221-222: What airborne measurements are used in the calculation of M?

Thanks for mentioning that. The other reviewer had the same question. We changed the text to, line 210:

"To derive M, we used the in situ method from Maherndl et al. (2024), which is based on in situ observations of particle shape. Only a subset of particles can be used to derive M (for

CIP, particle diameters must be larger 210 µm; for PIP, larger 1400 µm), because small particles have round shapes due to imager resolution. For a detailed description of the method and its limitations, we refer the reader to Maherndl et al. (2024). We consider particles with M < 0.01 to be unrimed due to their nearly identical scattering properties to particles with M = 0 (Maherndl et al., 2023)."

line 226: "…lack 4…". Did you mean pass 4 here?

Yes, we changed it. Thanks!

line 228-229: More evidence is needed to support this hypothesis.

Thanks, we agree. We acknowledge that this is not stated in the paper and revised the sentence in line 221 to reflect this more cautiously:

"Cloud streets are typically associated with stronger vertical shear at cloud top, while the transition to isotropic cloud patterns coincides with a reduction in wind shear, suggesting a shift toward buoyancy-driven convection."

line 238-240: Based on Figure 1, there is a thick cloud band on the right side of the cloud streets. Are these clouds included in the analysis as well? Are they treated as isotropic cloud regimes?

Thank you for your question. The thick cloud band seen in Figure 1, located around 5.5°E longitude, is not included in our analysis. We restricted our evaluation of cloud properties and organization to data collected west of 5.0°E, which excludes this cloud band. We have clarified this selection criterion in the manuscript to avoid potential confusion, line 165:

"For the analysis of cloud properties and organization, only data west of 5.0°E were considered to exclude unrelated cloud structures outside the main region of interest."

line 266-267: How do authors calculate the SST using the values of H, theta_0, and delta_theta_as?

Thank you for your question. The sea surface temperature (SST) was not derived, but measured directly using the KT19 infrared pyrometer onboard Polar 6, which flew at an altitude of about 60 m. The KT19 provides thermal infrared brightness temperatures in the 9.6–11.5 µm range, which were used to estimate the SST. It is mentioned in the sentence before.

line 294-295: It is unclear how the authors reached this conclusion. Is this hypothesis based solely on Table 1? The sample size is too small to support this conclusion.

Thanks for this comment. We acknowledge that the sample size is small, as it is based on a limited number of observations during one research flight. For that reason we explicitly mention that our study is a case study. However, to our knowledge, these are the first airborne measurements that directly confirm the theoretical framework linking the reduction in wind speed to the transition from cloud streets to isotropic cloud patterns. Previous studies (e.g., Gryschka and Raasch, 2005) have primarily relied on modeling and reanalysis data, whereas our study provides direct observational evidence supporting this mechanism.

We added the following part to the manuscript to make that more clear, line 292:

"Although based on a small sample size, these airborne observations provide the first direct in situ confirmation, to our knowledge, of the theoretical link between decreasing wind speed and the transition from cloud streets to isotropic cloud patterns, as previously suggested by modeling studies (e.g. Gryschka and Raasch, 2005)."

line 308: How do authors determine the strength of inversion?

Thanks for your question. Upon reviewing the manuscript, we realized that the mention of the inversion strength was not essential to our analysis, as it was neither compared to other cases nor used in further discussions. Therefore, we have removed it from the manuscript to maintain focus on the key findings.
For completeness, the inversion strength was determined from the dropsonde temperature profiles as the temperature difference between the base and top of the inversion layer

Figure 1: The manuscript does not justify the selection of WP1 and WP2. Has any analysis been conducted using dropsonde measurements from these locations?

Thank you for your question. WP1 and WP2 were selected based on the flight track, as they define the section where Polar 5 and Polar 6 repeatedly flew back and forth to capture the cloud evolution over time. These waypoints serve as reference points for the flight leg under investigation. While some dropsondes were deployed near WP1 and WP2, they were not included in our analysis, as our study focuses on the dropsondes released at the DS location, which was positioned within this flight segment and provided the most relevant measurements for our case study. To clarify this, we have added the following note, line 121:

"Waypoint 1 (WP1) and Waypoint 2 (WP2) in Fig. 1 were selected based on the flight track to define the section where Polar 5 and Polar 6 repeatedly flew back and forth to observe the temporal evolution of cloud properties. While some dropsondes were deployed near WP1 and WP2 (as marked in Fig. 1 these were not included in our analysis. Instead, we focus on the dropsondes released at the DS location, where the cloud transitions were most clearly observed."

Figure 3c: If Pass 1 was the earliest flight and Pass 4 occurred 120 minutes later, then Pass 4 is expected to have a smaller cloud street index compared to Pass 1 at the same location, as discussed later. However, this figure shows that Pass 1 and Pass 4 have similar cloud street index values when the distance to the sea ice edge is between 25 and 35 km, which is confusing. Additionally, it would be helpful to add a vertical line to Figure 3c to indicate the DS location.

Thank you for this helpful observation. The reason for the seemingly similar cloud street index (I_{CS}) values between Pass 1 and Pass 4 in the 25–35 km range is that the analysis shown in Figure 3c is not limited to the DS location. Instead, we calculate I_{CS} along the entire flight leg for each pass (Passes 1–4). Since the cloud street structure varies spatially along each leg, the I_{CS} values can overlap—even if the cloud field at the DS location evolves significantly over time.

To better illustrate this point, we show in the figure below the spatial variation of I_{CS} along each flight leg, which highlights how cloud organization changes within each pass. Additionally, we appreciate your suggestion and have added a vertical line in Figure 3c to mark the DS location for clearer reference.

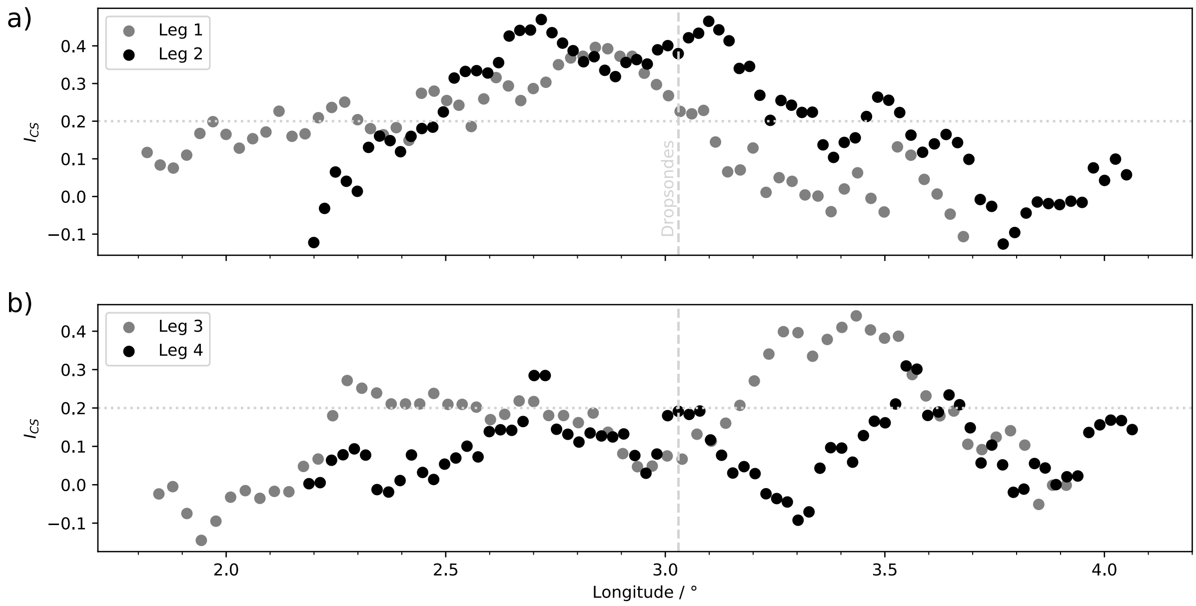

Figure 1: Spatial variation of ICS along each flight leg

Table 1: Why were the dropsonde results from HALO excluded from this table?

Thank you for your comment. Initially, we considered the HALO dropsonde data as additional information and did not include it in Table 1. However, after reviewing your suggestion, we agree that it should be mentioned for completeness. We have now added the HALO dropsonde results to Table 1, and we find that these data are consistent with our findings, further supporting our conclusions.