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Copernicus GmbH
European Geosciences Union
Atmospheric Physics and Chemistry
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Cologne, July 28, 2026

Answer Letter to Review Comments for the Manuscript "Impacts of mesoscale atmospheric subsidence on cloud glaciation and decoupling in Arctic marine cold air outbreaks"

Dear Ivy,

We sincerely thank the reviewers for their thorough and insightful evaluation of our manuscript. We are pleased to submit the revised version of our manuscript, "Impacts of mesoscale atmospheric subsidence on cloud glaciation and decoupling in Arctic marine cold air outbreaks", which has been carefully revised in response to the reviewers' comments. Detailed point-by-point responses to all comments are provided below.

The reviewers' feedback was highly valuable and helped us improve both the clarity and the overall quality of the manuscript. In addition to addressing the specific comments and suggestions, we used the review process as an opportunity to refine the presentation and interpretation of our results. To better communicate the conceptual implications of our findings, we have added a dedicated discussion section that includes a schematic overview of the key processes identified in our study. We believe that this addition provides a clearer synthesis of the results and strengthens the overall narrative of the manuscript.

We look forward to continuing the discussion and would be happy to address any further comments or questions arising from this revised version.

Sincerely,

Fiona Paulus

Response to Reviewer I

We thank the reviewer for the positive assessment of our manuscript and the recommendation for acceptance with minor revisions. We greatly appreciate the detailed and constructive comments, which helped us to improve the clarity and presentation of the study. We have carefully addressed all comments and revised the manuscript accordingly.

Minor Comments:

Line 18: A cold air mass does not need to move southward to be a MCAO. MCAOs can happen in both hemispheres. Additionally, MCAOs in the Arctic can occur with zonal winds too, for example westerly flow behind a cold front off of the sea ice east of Greenland.

> We thank the reviewer for this comment and have revised the sentence accordingly.
>
> Line 16-18: During marine cold air outbreaks (MCAOs), cold and dry polar air masses are advected over cold sea ice surfaces towards the warmer open ocean, where they heat up and pick up humidity from below.

Line 31: Polar lows also require other mechanisms such as convergence, topography, deeper thermodynamic support. I don't think they should be included in this list

> We agree that the inclusion of polar low formation in this context was misleading and have therefore removed it from the list.
>
> Lines 28-29: These evolving cloud and ABL characteristics modulate cloud-radiative interactions, determine precipitation formation, and increase surface winds (Kolstad, 2017; Terpstra et al., 2021; Mateling et al., 2023).

Line 48: I was under the impression that increased subsidence strengthens the inversion which inhibits entrainment. The wording of this sentence reverses my understanding of the cause and effect.

> We thank the reviewer for pointing this out. We agree that the original wording could be interpreted as reversing the causal relationship. We have therefore revised the text to clarify the role of subsidence in limiting mixed-layer growth through its effect on cloud-top entrainment.
>
> Lines 44-49: ... subsidence in surface-coupled convective layers can counteract mixed-layer growth by reducing the effective contributions of cloud-top entrainment. Increased liquid water paths inside the mixed-layer can enhance cloud-top radiative cooling and, together with precipitation-related diabatic processes, promote convective overturning and precipitation production. In turn, enhanced cloud-top radiative cooling and convective overturning under stronger subsidence can increase precipitation production in both rain and snow. Under surface-warming conditions, stronger subsidence can also suppress cloud-top ascent and help maintain a stable stratiform cloud layer (Young et al., 2018).

Line 67: COMBLE did not observe conditions in the Fram Strait, it sampled downstream conditions at Bear Island and Andenes.

We thank the reviewer for this clarification. The text has been revised accordingly.

> Lines 65-67: ... Cold air Outbreaks in the Marine65 Boundary Layer Experiment (COMBLE) in 2019 and 2020 observed several MCAOs with two permanent observational sites that provided continuous measurements of downstream conditions at Bear Island and Andenes (Geerts et al., 2022) ...

Line 67: CAESAR could also be mentioned

Done.

> Lines 68-70: ... (STABLE) campaign in March 2013 and the Cold Air Outbreak Experiment in the Sub-Arctic Region (CAESAR), who provided airborne observations with research aircraft in the Fram Strait (Michaelis et al., 2022; Zuidema et al., 2026).

Line 86: Current wording "Arctic west of Norway and the Fram Strait" doesn't make clear that the domain of the campaign was only over the ocean and sea ice. Could rephrase to: "Norwegian/Greenland Seas and the Fram Strait".

Done.

> Line 90: The HALO-(AC^3) field campaign took place in the Norwegian/Greenland Seas and the Fram Strait from March to April 2022.

Line 97: Suggest changing to: "Both days were characterized by persistent northerly wind of about ...". "Persistent" and "steady" are redundant. I don't necessarily agree with the classification of the wind speed (reaching 10 m/s or 20 kts) as "slow".

We thank the reviewer for this suggestion. We have revised the sentence accordingly by removing the redundant wording and the characterisation of the wind speed.

> Lines 99-100: Both days were characterised by a persistent northerly wind of about 3 to $10\,\mathrm{m\,s^{-1}}$ measured at $10\,\mathrm{m}$ altitude by dropsonde observations.

Line 99: "MCAO" used 3 times in the same sentence. Sentence should be reworked.

Done.

> Lines 100-101: . These characteristics indicate a weak MCAO event compared with climatological values from the Fram Strait (Slättberg et al., 2025).

Lines 100-102: This sentence is confusing. A weak MCAO between two strong MCAOs? How is that different from a week + of persistent MCAO conditions?

We agree that the original wording was unclear. We have revised the text to clarify that the observed case occurred within a prolonged period of MCAO activity and was situated between two intervals of particularly strong MCAO conditions, rather than between two separate MCAO events.

> Lines 102-104: The observed MCAO case embedded within a prolonged period of extensive MCAO activity from 21 March to 12 April 2022, between two intervals of particularly strong conditions on 21–26 March and 1–2 April 2022 (Walbröl et al., 2024).

Line 110: Clarify that this location in the "high north" is over the sea ice

Done.

> Lines 111-112: The HALO flight plans on these two days aimed at tracking the location of an air mass sampled on the first day at a location in the high north above sea ice using trajectory estimates based on forecast data.

Line 124: Cite ISLAS

A reference to the campaign website was added.

Lines 177-179: CCN concentrations (and their effect on droplet size) are identified as being important for a closed to open-celled transition in Abel et al. 2017. Since your prescribed choice of CCN is held constant throughout your experiments, your experimental design is reasonable for what you are testing, however, I recommend discussing (either here or in the analysis) how not accounting for aerosols could affect your results.

We thank the reviewer for this important remark. We agree that aerosol variability may influence the cloud and precipitation responses identified in this study. To assess the robustness of our findings, we performed additional sensitivity experiments with different CCN concentrations. These tests indicate that CCN concentrations can affect cloud microphysical pathways, but did not change the overall sequence of decoupling. However, a comprehensive analysis of aerosol sensitivities is beyond the scope of the present study. We have therefore added a discussion of the potential implications of CCN variability and the limitations associated with prescribing constant aerosol conditions.

> Lines 656-661: Furthermore, the prescribed aerosol particle number concentrations are held constant during the sensitivity experiments, thereby excluding potential aerosol–cloud–precipitation feedbacks. Variations in cloud condensation nuclei (CCN) concentrations can modify cloud droplet number and size, with consequences for precipitation formation and the transition from closed- to open-cellular convection (e.g. Abel et al., 2017). Changes in aerosol concentrations may, therefore, alter the timing or strength of the precipitation and decoupling responses identified here, and potentially modulate their sensitivity to subsidence.

Line 180: What temperature are those INP concentrations at?

We appreciate this comment. The original description was incomplete, as it did not distinguish between the prescribed maximum available INP concentration and the activated concentration, which depends on temperature and ice supersaturation. We have revised the text accordingly.

> Lines 183-185: The available INP number concentration is prescribed with an upper limit of 1×10^{-3} cm^{-3}, following the setup of Chylik et al. (2023) and Hartmann et al. (2019), while the activated fraction depends on temperature and ice supersaturation through the Reisner et al. (1998) parametrisation.

Line 183: Other SIP mechanisms, such as ice-ice collisions and drizzle freezing fragmentation, are identified as important for reproducing observed ice particle concentrations in Karalis et al 2022, Schafer et al. (2024) and Sotiropoulou et al. (2020). Since SIP is held constant throughout your experiments, your experimental design is reasonable for what you are testing, however, I recommend discussing (either here or in the analysis) how not accounting for other SIP mechanisms could affect your results.

We thank the reviewer for this important comment and for recommending these relevant studies. We agree that discussing the potential implications of neglected secondary ice production (SIP) mechanisms is important in the context of our results. We have therefore expanded the discussion section. The representation of additional SIP processes and their impact on cloud evolution and precipitation formation is an important topic for future work; however, a comprehensive assessment would require a dedicated investigation and is beyond the scope of the present study.

> Lines 661-665: Similarly, secondary ice production is limited to the Hallett–Mossop process, while other mechanisms, such as ice–ice collisional breakup and freezing fragmentation, are not represented in the model. These mechanisms may enhance ice particle concentrations and thereby modify ice growth, graupel formation, precipitation, and the timing of boundary-layer decoupling (e.g. Karalis et al. 2022, Sotiropoulou et al., 2020; Schäfer et al., 2024).

Line 199: How trajectories for flight planning were calculated was not previously discussed. Should remove: "As described in Section 2.1"

Done.

Line 201: ERA5 3-D winds should potentially be added to Table 1

We appreciate this suggestion. The ERA5 three-dimensional winds were used exclusively for the trajectory calculations to determine the air-mass location at each time step and were not used for model initialisation, forcing, or the derivation of advective tendencies. These quantities are instead based on the observed wind profiles. We have therefore chosen not to include the ERA5 winds in Table 1.

Line 202: "matching the circle locations on March 30, 2022" is confusing wording

We appreciate this comment and have revised the sentence to improve clarity.

> Lines 205-207: Air parcels were identified, and their trajectories were calculated starting every 3 km lat/lon within the IC on 29 March 2022 and passed through the circle locations on 30 March 2022 (Kirbus et al., 2024).

Line 229: 0.025 cm/s error sounds unrealistically small. Does this error estimate assume both that dropsondes are all released simultaneously and that dropsondes fall straight downward with no drift? If so, those could be large sources of additional error and the above caveats should be mentioned.

We thank the reviewer for this comment and have clarified that the reported uncertainty reflects only the regression-based estimate and excludes potential representativeness errors from temporal evolution during sequential dropsonde releases and horizontal sonde drift.

> Lines 234-241: Uncertainties associated with the regression-based divergence estimate were calculated following Paulus et al. (2024) and reach up to 0.025 cm s^{-1} (0.002 Pa s^{-1}) below 3 km across all circles, which cannot be resolved in the figure and are therefore not shown. These estimates reflect uncertainties propagated from the regression analysis and do not represent a complete uncertainty budget, as they exclude potential representativeness errors associated with temporal evolution during the sequential dropsonde releases and deformation of the sampling pattern due to horizontal sonde drift. The method assumes approximate stationarity over the sampling period; sensitivity tests by Paulus et al. (2024) indicate that accounting for pattern deformation changes the derived divergence and pressure velocity by less than 1 % and 10 %, respectively.

Line 280: I suggest adding a figure where you show snapshots of outgoing longwave from the simulation with side by side snapshots of satellite imagery. For example, something similar to the VIIRS vs DHARMA pseudo-albedo plot in the COMBLE MIP preprint. (Juliano et al 2026; Figure 6)

We thank the reviewer for this suggestion. We agree that a comparison of simulated outgoing longwave radiation with satellite observations would provide an interesting evaluation of the mesoscale cloud organization. We are fortunate to have a large amount of available observational data for this case and aim to carefully incorporate these observations where they directly support the objectives of this study. However, the focus of the present work is on the time evolution of the boundary layer structure, cloud phase, and precipitation response to perturbed mesoscale subsidence. A spatial evaluation of cloud organization against satellite imagery would require a separate analysis and is therefore beyond the scope of the present study.

Line 308: What do you mean by % agreement? Does this depend on what units (K vs C) are used in that calculation? If this complement of percent error, I'm not sure that is an appropriate metric. For example: if the truth is 273K and that is modeled as 263K, that is only a 4% error (96% agreement), however, being 10K off is significant. Portraying that agreement as 96% is misleading to the reader.

We agree that the percentage agreement may not reflect physically relevant absolute differences and thus be misleading. We have therefore removed this measure throughout the manuscript and now evaluate the simulated thermodynamic profiles based on their vertical structure and absolute differences in virtual potential temperature and specific humidity.

Lines 313-316: These sentences could be consolidated

Done.

Lines 337-339: Line 337 implied that the model underestimates q by 0.3 g/kg in the boundary layer, but line 339 states that the model underestimates q by 0.2 g/kg below the inversion. Why are these numbers different?

We appreciate this comment and have adjusted this sentence to remove the ambiguious statement.

Lines 345-346: The simulated humidity jump is also weaker than observed. Below the temperature inversion, the model underestimates q by $0.2\,\mathrm{g\,kg^{-1}}$.

Line 426: Is 21 W/m2 the simulated value using the new method? This sentence seems to imply that this is the difference between the two methods.

We thank the reviewer for pointing out this ambiguity. We have revised the sentence to clarify the comparison between the two methods.

Lines 432-435: Between C02 and C03, the thermodynamic method yields a simulated value of only $H_{Ctrl,Prof} = 21\,\mathrm{W\,m^{-2}}$, compared to $H_{DS} = 170\,\mathrm{W\,m^{-2}}$, largely underestimating the surface sensible heat flux.

Figure 1: Could add ERA5 MSLP contours from a timestamp of your choosing to help the reader understand the large scale flow. Add lat/lon labels.

We thank the reviewer for this suggestion. We have added latitude and longitude labels to Figure 1 and included an additional figure in the Appendix showing ERA5 mean sea-level pressure and horizontal wind to provide context for the large-scale flow without overcrowding Figure 1.

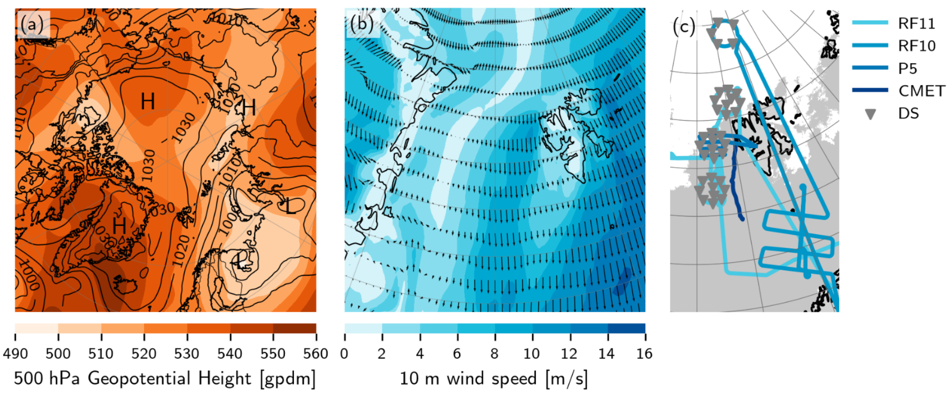


Figure A1: Maps of geopotential height at 500 hPa (shading) and mean sea level pressure (black contour lines; a) and wind speed at 10 m (shading) and wind direction (arrows) on 30 March 2022 at 12:00 UTC during HALO-$(AC)^3$ from ERA5 (b). Flight paths of HALO flights RF10 (29 March 2022) and RF11 (30 March 2022) and P5 on 30 March 2022, together with CMET, Dropsonde (DS) locations () and the sea-ice concentration retrieved from satellite observations Spreen et al., 2008) indicated in grey and white (c).

Figure 2: For temperatures, I suggest using a multispectral colormap. For example, as currently plotted it is impossible to distinguish where SSTs are 1C and where they are 4C.

We thank the reviewer for the suggestion. We prefer to retain the current sequential colormap, as it appropriately represents temperature as a continuous variable and clearly illustrates the broad spatial variations relevant to this figure.

Figure 6: For q, the x-axis should be cut off at zero since negative q is unphysical

This has been adjusted.

Technical Comments:

Line 96 and other locations: The abbreviation "Fig." should be used when it appears in running text and should be followed by a number unless it comes at the beginning of a sentence, e.g.: "The results are depicted in Fig. 5. Figure 9 reveals that...".

Done.

Line 96 and other locations: Date should be in the format: dd month yyyy hh:mm:ss (29 March 2022 and 30 March 2022 respectively)

Done.

Line 100 and other locations: A citation within parentheses should be shown as (Slattberg et al., 2025) not (Slattberg et al., (2025)).

This has been adjusted.

Line 108: foehn effect -> Foehn Effect

Done.

Line 131: air humidity -> relative humidity

Done.

Line 182: 200 L -> 200 L-1

Done.

Line 261: EUREC4A -> EURECA4

We tried to stay consistent with the official website (https://eurec4a.eu/) and logo, where EUREC^{4}A is used.

Line 271: Initialisation -> initialization

We have used a british spelling throught the paper and therefore decided to keep "initialisation".

Line 338: strong humidity -> high humidity

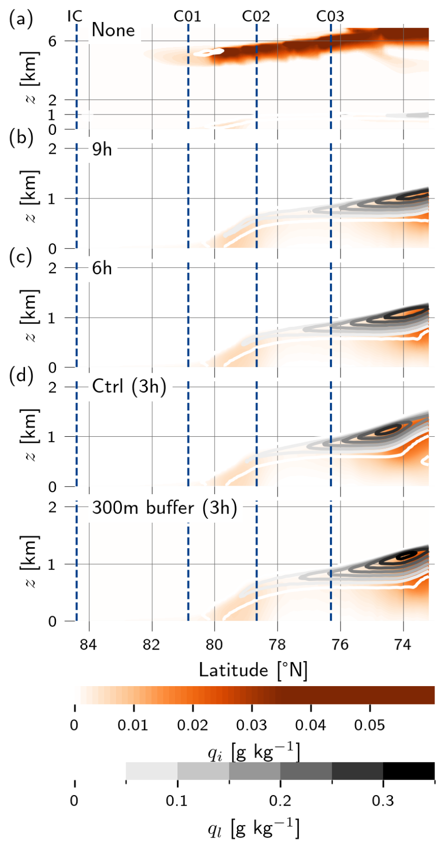
Done.

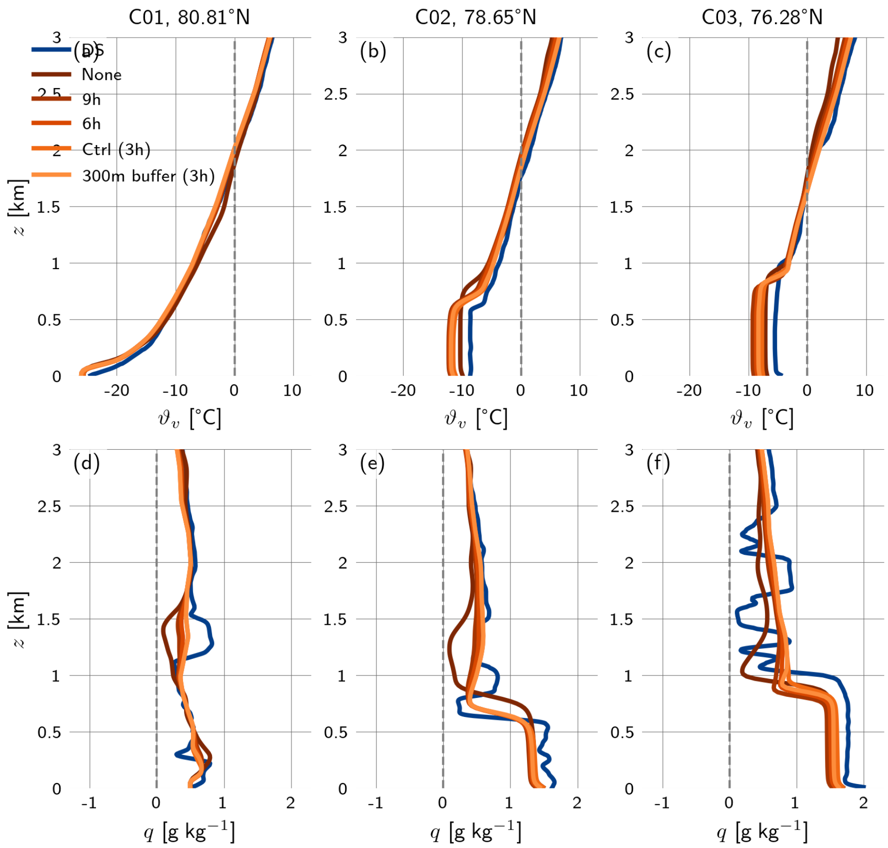
Response to Reviewer II

We thank the reviewer for the positive feedback and recognition of the relevance of our study. We greatly appreciate constructive comments regarding the modelling setup, interpretation of our findings, and role of microphysical processes. In response, we have expanded the analysis and discussion and clarified the corresponding aspects of the manuscript.

Major comments:
Regarding the modeling setup, there is strong nudging (3 hr time scale) imposed above the ABL. The buffer layer is very shallow (only 100 m), which makes me wonder if the evolution of clouds in the model is too heavily constrained by the nudging. Can the authors demonstrate the impact of this nudging on the cloud development by performing one or two sensitivity tests? This can help quantify the dependence of the results on the modeling setup.

> We thank the reviewer for this question. To assess the influence of the large-scale nudging, we performed additional sensitivity experiments for the first analysis period using the same 50 m resolution on a reduced horizontal domain (3.2 × 3.2 km) to limit computational cost. Specifically, we compared (i) the reference setup (3 h nudging, 100 m buffer layer), (ii) a weaker 6 h nudging time scale, (iii) an increased buffer layer depth of 300 m, and (iv) a simulation without any nudging.
> The sensitivity experiments show that the cloud evolution is largely not sensitive to reasonable changes in the nudging configuration. Increasing the relaxation timescale from 3 to 6 h and enlarging the buffer layer from 100 to 300 m produce only negligible differences in the simulated boundary-layer structure of temperature and humidity. In contrast, completely removing the nudging leads to the development of a pronounced upper-level ice cloud. Therefore we conclude that for all reasonable nudging configurations the conclusions of this study are robust.





How do we reconcile the results from this study with the conclusions from Young et al. (2018)? Young et al. (2018) found that in their simulations, LWP and precipitation rates increase with subsidence rate, while IWP is not sensitive to subsidence changes. This is even true when surface warming is included. Here, LWP, IWP, and total precipitation all decrease with increasing subsidence. There is no sign of non-monotonic response to subsidence rates. The range of subsidence rates doesn't seem to explain this difference.

> We thank the reviewer for raising this important point. We argue that the difference reflects the distinct ABL regimes considered rather than the range of imposed subsidence rates. Young et al. (2018) investigated an established, shallow mixed-phase stratocumulus layer, where stronger subsidence strengthened the capping inversion and reduced cloud-top entrainment, increasing LWP and subsequent precipitation production. In contrast, the cloud layer in our study develops within an MCAO and continuously adjusts to surface heating and moistening after leaving the sea ice. Here, subsidence primarily controls ABL deepening and the timing of internal decoupling. Reduced subsidence allows more rapid ABL growth, leading to earlier decoupling, larger LWP, and stronger graupel formation, thereby increasing IWP and precipitation. Stronger subsidence delays this coupled dynamical–microphysical evolution, resulting in the monotonic decreases found here. We have added a discussion section to clarify these processes and place our findings in the context of previous Arctic studies and subtropical stratocumulus-to-cumulus transitions.

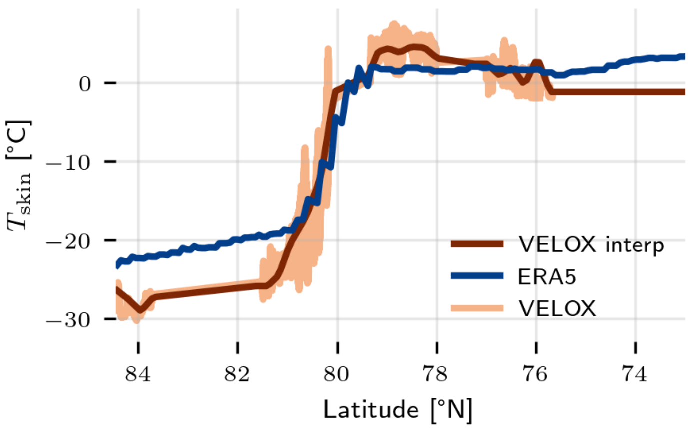
While it may be beyond the scope of this paper to explore the sensitivity of the graupel formation timing to microphysics, it would be helpful to discuss how the microphysical scheme may affect the implications of the results.

> We thank the reviewer for this suggestion. We agree that the precise timing and characteristics of ice-phase precipitation may depend on the representation of cloud microphysical processes. We have therefore added a brief discussion noting that the identified subsidence–decoupling relationship is a dynamical feature of the simulated MCAO, while the timing and partitioning of ice precipitation may vary with the chosen microphysical scheme and aerosol assumptions. A more detailed investigation of microphysical sensitivities is beyond the scope of the present manuscript and represents an interesting topic for future work.
>
> Lines 612-616: Additional sensitivity experiments using the same model setup indicate that the ice-phase pathway is sensitive to CCN concentrations, with higher CCN concentrations shifting ice precipitation from graupel toward snow (not shown). This suggests that the timing and partitioning of precipitating ice depend on the microphysical representation, including processes that control droplet size and riming, as well as on prescribed aerosol concentrations. A more systematic investigation of these sensitivities would be required to assess their impact on the processes identified in this study.

Minoar comments:
L150–152: How well is ERA5 capturing the surface temperature evolution during this period? If surface parameters are from ERA5, it would be helpful to know if ERA5's surface temperature has any biases.

> We thank the reviewer for this comment and clarify that the prescribed surface skin temperature in the LES is not taken from ERA5. Instead, the skin tempera-

ture is constrained by airborne VELOX observations and interpolated along the simulation trajectory. ERA5 is used only for surface parameters that were not available from observations, namely surface albedo and the aerodynamic and thermal roughness lengths.



In a direct comparison of the VELOX observations (orange) with the ERA5 (blue) skin temperature, ERA5 shows a significant warm bias above the sea ice and a bias of about 2 K over open ocean.

L208–211: I'd like to see more details about the bottom boundary conditions. Is the surface temperature prescribed using the observed surface skin temperature? How is atmospheric specific humidity calculated at the marginal ice zone? What is the temporal resolution of these bottom boundary conditions? I assume they are time varying.

We thank the reviewer for this comment. The surface skin temperature is prescribed from the airborne VELOX observations. The VELOX-derived surface skin temperature and sea-ice concentration are averaged longitudinally and interpolated along the Lagrangian simulation trajectory at hourly resolution. The lower-boundary specific humidity follows the setup of Schnierstein et al. (2024) and is calculated from the saturation specific humidity at the prescribed surface skin temperature, weighted by the sea-ice fraction. We have clarified these points in Section 3.3 and added a reference to the description of the lower-boundary formulation.

> Lines 212-216: The lower boundary conditions are prescribed as time-varying fields along the Lagrangian simulation trajectory with hourly resolution. Surface skin temperature T_{skin} and sea-ice concentration sic are derived from VELOX observations (section 2.2), longitudinally averaged along the circular flight legs, and interpolated along the simulation trajectory. As the simulated air mass moves from sea ice through the marginal ice zone to the open ocean, this interpolation provides a temporally evolving surface boundary condition. The lower-boundary specific humidity is calculated following Schnierstein et al. (2024) from the saturation specific humidity at the prescribed surface skin temperature and weighted by the local sea-ice fraction.

L335: Remove parentheses around reference. Why is there a cold bias?

We have removed the parentheses around the reference and added that the cold bias may reflect uncertainties in the prescribed surface skin temperature and limitations of the bulk surface-flux parameterisation, which could lead to an underestimation of sensible heat transfer from the relatively warm ocean to the cold air mass.

> Lines 341-343: Therefore, there is a consistent cold bias of -2K to -3K across all circles, a tendency also found by Wendisch et al. (2025) in the limited-area configuration of the ICON model for all observed MCAO cases of HALO-$(AC)^3$ and may reflect uncertainties in the prescribed surface skin temperature and limitations of the bulk surface-flux parametrisation.

L368–369: "the simulation was slightly overestimated" -> "the simulation slighted overestimated"

Done.

L374: What about the LWP peak of 100 g/m2 near C02 in HAMP? This is an order of magnitude larger than simulated LWP around that location.

We thank the reviewer for pointing this out. The pronounced LWP peak near C02 in the HAMP observations was located in the marginal sea-ice zone, predominantly over sea ice. In this region, the HAMP LWP retrieval may not be reliable because the retrieval is not applicable over sea-ice-covered surfaces. To account for this limitation and avoid including potentially unreliable retrievals affected by sea ice, we have therefore restricted the analysis to observations over open ocean ($sic = 0$).

L390: The fact that the P5 observations do not align with the air mass trajectory makes it questionable to compare them to the simulation.

We agree that the lack of alignment between the P5 observations and the air mass trajectory introduces a limitation in their direct comparison with the simulation results. Nevertheless, we retain the P5 observations because they provide the only available independent observational constraint on IWP and serve as a useful order-of-magnitude reference for the simulated values. Therefore, we use these observations as a qualitative consistency check rather than as a strict point-by-point evaluation of the model performance.

Section 4.4: Is it possible to estimate the surface heat fluxes using the bulk method and compare them to the thermodynamic method? How do we interpret the estimated fluxes at steady state whereas the simulations are evolving drastically?

We thank the reviewer for this suggestion. The thermodynamic method is the more directly relevant diagnostic here, since it is derived from the same vertically integrated fields used to characterise the simulated air-mass transformation. A bulk estimate would require independent near-surface gradients and transfer coefficients and carries its own uncertainties that are difficult to constrain without dedicated near-surface flux measurements. We therefore consider a bulk-method comparison beyond the scope of this study and a worthwhile topic for future work.
Regarding the steady-state assumption: following Hartmann et al. (1997), it is applied to the downstream (spatial) evolution of the profiles, not the tempo-

ral evolution of the simulation. Differences between successive downstream profiles are interpreted as advective changes along the mean flow and linked to the vertically integrated surface heat and moisture input. We have clarified this in the opening paragraph of Section 4.4.

> Lines 403–405: ... and minimal entrainment above the mixed layer. Here, following Hartmann et al. (1997), the steady-state assumption allows thermodynamic differences between successive downstream profiles to be interpreted as along-flow advective changes and linked to the vertically integrated surface heat and moisture input.

Figure 8 caption: the triangle legend is different from what's on the figure, which may cause confusion.

Thank you for the comment, it has been changed.

Figure 12: This figure took some time for me to grasp. What explains the deviation from the 1:1 line? I would appreciate seeing some profiles of buoyancy flux at different stages to better understand the timing of the decoupling.

We thank the reviewer for this helpful suggestion. In response, we have revised Figure 12 to improve its clarity and make the interpretation more intuitive. Furthermore, we have added buoyancy-flux profiles along the MCAO evolution trajectory in the Appendix to provide additional insight into the development of boundary-layer decoupling.