Review of "*The Arctic Low-Level Mixed-Phase Haze Regime and its Microphysical Differences to Mixed-Phase Clouds*", by Manuel Moser, Christiane Voigt, Oliver Eppers, Johannes Lucke, Elena De La Torre Castro, Johanna Mayer, Regis Dupuy, Guillaume Mioche, Olivier Jourdan, Hans-Christian Clemen, Johannes Schneider, Philipp Joppe, Stephan Mertes, Bruno Wetzel, Stephan Borrmann, Marcus Klingebiel, Mario Mech, Christof Lüpkes, Susanne Crewell, André Ehrlich, Andreas Herber, and Manfred Wendisch, egusphere-2025-3876.

Response to reviewer 1

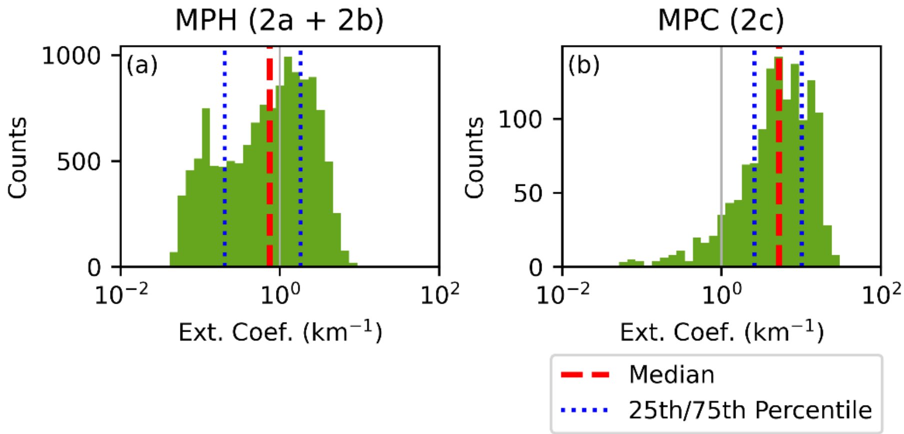
Dear reviewer,
We are very grateful for your valuable feedback and suggestions which helped us to improve the manuscript. The manuscript has been thoroughly revised and point-by-point responses have been prepared. Please find below our replies, highlighted in blue, along with changes made in the manuscript, highlighted in orange. The revised manuscript is also provided with tracked-changes for clarity.

Major comments
These are comments regarding the comprehension of the manuscript and include questions to help the clarification of the findings. The authors should consider addressing them.

- Line 45: The authors hint at the potential of MPH for the atmospheric radiation budget. However, throughout the manuscript no evidence is shown for that, and in the end, the authors state that MPH are optically thin (Line 394). I am wondering what is their importance? This also connects to how often this regime was identified, and especially with no liquid layer at cloud top. Because for MPC the liquid layer is the driver for the cloud top cooling and has the biggest impact of the radiative budget. Can you please elaborate on that more?

We thank the reviewer for this valuable comment. Indeed, this study does not include a quantitative calculation or direct measurement of the optical thickness. The statement that mixed-phase haze (MPH) is optically thinner than mixed-phase clouds (MPC) is based on visual impressions from synchronized onboard photographs and video footage, which were correlated with the respective cloud regimes.
In the introduction, we assume that the MPH regime is optically thin. Therefore, we highlight the general relevance of optically thin clouds in the context of sensitivity, as even small changes in their microphysical properties can lead to substantial differences in their radiative effects, e.g. downward thermal-infrared emission warming the surface. However, we do not claim that the MPH regime itself has a large impact on the radiative budget.
To provide a more objective comparison, we now omit the optical thickness statement from the manuscript and instead use the extinction coefficient to quantify and compare the optical properties of MPH and MPC. Since the data from the Polar Nephelometer (PN) are not available in sufficient quality for the HALO-(AC)^3 campaign, we refer to PN data from the AFLUX campaign presented in Fig. 8 in Moser et al. (2023b). In this figure extinction coefficients for the individual cloud regimes are shown. For the specific comparison between MPH and MPC, these data are summarized in R_Fig. 1. From these data, the median [25th/75th percentile] extinction coefficient is 0.7 km^{-1} [0.2 / 1.8 km^{-1}] for the MPH regime (regime 2a + 2b) and 5.2 km^{-1} [2.6 / 10.1 km^{-1}] for the MPC regime

(regime 2c). This confirms that the extinction coefficient of MPH is significantly lower than that of MPC, reflecting its optically thin nature.



R_Figure 1: Frequency distribution of the extinction coefficient (Ext. Coef.) measured with the Polar Nephelometer for the (a) mixed-phase haze (MPH; 2a +2b) and (b) mixed-phase cloud (MPC; 2c) regimes. The data were obtained during the Arctic field campaign AFLUX in spring 2019. Original figure is presented in Moser et al. (2023), Figure 8.

Changes made in the manuscript:

- We have changed the sentence in line 391 to: "Consistent with the visual observations, the extinction coefficient of MPH is significantly lower than that of MPC, reflecting their optically thinner property. Values for the extinction coefficients are provided in Fig. 8 of Moser et al. (2023b). Based on the measurements during spring 2019, the median [25th/75th percentile] extinction coefficient is 0.7 km^{-1} [0.2 km^{-1} / 1.8 km^{-1}] for the MPH regime (2a + 2b) and 5.2 km^{-1} [2.6 km^{-1} / 10.1 km^{-1}] for the MPC regime (2c)."
- Sentence in line 405 changed to: "This difference is also reflected in the substantially reduced extinction coefficient (Ext. Coef.) observed for MPH."
- Figure 8: Optical thickness (τ) was changes to the extinction coefficient (Ext. Coef.)

Minor comments

These are editorial comments helping to improve the formatting and readability of the manuscript. The authors should consider adding them.

- Line 13: While reading I was wondering if the MPH is a state instead of MPCs, but it appears that it is most often found beneath the MPC, which is stated here. However, there are cases of MPH without a MPC identified. I think the abstract would benefit from a clear statement on the occurrence and persistence of MPH.
  Reply: We thank the reviewer for the suggestion to emphasize the relevance of MPH by highlighting its frequent occurrence. We have extended the sentence in Line 4 of the abstract with: „, with MPH observed about eight times more frequently than MPC.". This statement is based on the results shown in Fig. 8, indicating a 4 % fraction of MPC and a 34 % fraction of MPH.

- Line 21: "Many factors influencing Arctic amplification are discussed (Wendisch et al., 2023a)", is an incomplete sentence. Either you say "discussed in" or you rephrase it in such a way that it is clear that a list of possible reasons is following.

Reply: We have changed the sentence to: "Many factors influencing Arctic amplification are discussed in the literature (Wendisch et al., 2023a)"

- Line 28: That MPCs are in a quasi-steady state is true, however, the reason is not only dynamical processes but rather a superposition of dynamics, turbulence, radiation, and cloud microphysics. This should be clearly stated, as it is still a challenge to fully understand how these clouds remain persistent given their metastable thermodynamic state.
  Reply: We have changed the sentence to: "These clouds persist in a quasi-steady state due to a complex interplay of dynamics, thermodynamic structure, radiation, and microphysics, which interact to maintain the liquid phase in spite of its metastable thermodynamic state (Morrison et al., 2011)."

- Line 36: replace "including those" by (e.g., ...).
  Adopted

- Line 44: using a threshold of LWC = 0.01 g m→3
  Adopted

- Line 48: mixed-phase temperature regime needs to be defined (in numbers)
  Reply: We have changed the sentence to: "Costa et al. (2017) observed a low number concentration (N < 1 cm−3) in some cloud types when analysing a large in-situ data set of clouds in the mixed-phase temperature range between 0 °C and -38 °C."

- Line 49: the Wegener-Bergeron-Findeisen process should be at least explain in 1-2 sentences, because it appears throughout the manuscript.
  Reply: We assume that the Wegener-Bergeron-Findeisen process is familiar to readers working in this field and therefore do not provide a detailed explanation in the manuscript. However, we have added two relevant references (Pruppacher and Klett, 2010; Storelvmo and Tan, 2015) at the first occurrence of the term. Please also see our response to Reviewer 2 regarding the comment "Line 49: Add citation for the Wegener–Bergeron–Findeisen (WBF) process."

- Line 50: One-sentence paragraphs should be avoided.
  Reply: Thank you for the comment. Following a suggestion from Reviewer 2, this paragraph has been extended and is no longer a one-sentence paragraph.

- Introduction: While the abbreviations MPC (also it should be MPCs if it is used in the plural form) and MPH are introduced in the abstract, they are only defined later in the manuscript text, even though the regimes are discussed already in the introduction. Please double-check that you introduce the abbreviations at the earliest appearance and then use them throughout the text. This goes for several abbreviations, such as LWC and IWC as well.

  Reply:
  We have carefully reviewed the use and introduction of all abbreviations throughout the manuscript. The terms mixed-phase cloud (MPC) and mixed-phase haze (MPH) are now introduced at their first occurrence in the main text and consistently used

thereafter. In the abstract, both terms remain written out in full to ensure that it can be read independently from the manuscript.
The abbreviations MPC and MPH are only used where the respective regime is explicitly meant.
In addition, the abbreviations LWC, IWC, CWC, N, D_{eff}, ABL, MIZ, RH_w, RH_{ice}, CCN, INPs, PSD, and SIC are now correctly defined and used consistently throughout the paper.

- Line 66: what does "partially close proximity" mean? Can you just quantify it as you do it later in the text?
  Reply: We cannot provide a more precise quantification here, as the flight activities conducted with the FAAM and ATR aircraft were not part of this study. They are mentioned explicitly to acknowledge their coordinated operation within the HALO-$(AC)^3$ framework and to draw attention to related measurement activities.

- Line 72: "had" to has
  Adopted

- Line 74: reference to Figure 1 when you introduce the location
  Adopted, we have added "(see Fig. 1)" at the end of the sentence.

- Line 84: CWC needs to be introduced, especially since I assumed you meant LWC. It should be clear that here the total water content (TWC) is meant. Also a short sentence on the threshold should be done, and not only referring to a previous study.
  Reply: We have changed the sentence in line 84 to: "The microphysical low-level cloud dataset from Polar 6 consists of a total of 19.4 h (< 1000 m, and cloud threshold CWC > 2×10−4 g m−3 according to Moser et al., 2023b; CWC = LWC + IWC), collected during 13 flights in March and April 2022." And deleted the sentence in line 243: "~~The CWC is calculated as the sum of LWC and IWC.~~"

- Line 93: "N" needs to be defined
  Reply: N is now defined before that line

- Line 121: Why are legs from horizontal flights less significant?
  Reply: We assume that the reviewer's question is "Why are legs from vertical flights less significant?". Vertical flight legs are less statistically significant than horizontal ones because environmental and thus microphysical properties typically vary more rapidly in the vertical than in the horizontal direction. During horizontal legs, data can be averaged over longer distances, which increases the statistical robustness of the measurements. In contrast, vertical profiles are evaluated at 1 Hz resolution, whereas in studies focusing on horizontal in-situ cloud data, averaging is often performed over several minutes.
  In addition, the airflow conditions around the in-situ cloud probes can change during ascents and descents, which are less well characterized than during steady horizontal flight legs.

- Line 126: Suggestion for reformulation: Moser et al. (2023b) introduced a method for determining ...
  Adopted

- Line 131: The sentence regarding the thresholds is somewhat confusing, and should be rephrased, such that word repetition is avoided.
  Sentence changed to: Consequently, when applying the same procedure as in Moser et al. (2023), the resulting threshold values separating the individual regimes differ slightly.

- Table 1: the abbreviation were not introduced.
  Reply: This is now corrected in the revised version.

- Line 142: given that CAO and WAI are only used twice, consider not using the abbreviations and just write it out.
  Adopted

- Line 152: The classification ocean/land I find confusing as it should indicate that the air mass was mostly influenced by the ocean. In Line 182 the phrasing is also slightly different for the ocean/land mask, which makes it more confusing.
  Reply: We agree with the reviewer. One option would be to name the mask simply "ocean" and mention in one sentence that a minor influence from land cannot be excluded. However, we prefer to remain transparent and scientifically precise. Therefore, we chose to describe the mask as "ocean/land" to accurately reflect the possible influence of both surface types.

- Line 165: which data were now used?
  Reply: Only the meteorological data from the dropsondes were used, as the nose boom data provided reliable measurements on only a few days. For the days when both datasets were available, the correlation approach between the Polar 6 in-situ cloud measurements and the dropsonde data was verified. Since this validation is not part of the manuscript, the corresponding sentences were removed to avoid confusion:
  ", which limited the availability of reliable data to only a few days. However, these data were used to validate the correlation approach between Polar~6 and the dropsonde data."

- Line 168: the information in the brackets for SIC can be omitted as it was defined before
  Adopted

- Line 170: Suggestion for reformulation: ..., at which the minimum temperature occurs (i.e., minimum of temperature inversion).
  Adopted

- Figure 2: For all figures the colors should be revised such that they are color-vision-deficiency friendly (especially avoiding red and green lines). I was wondering, if the

grey dropsonde profiles can be stratified by the origin as it was done for the mean value. You could use just a lighter color of the mean value to indicate that. Moreover, asterisks could be used to mark the mean ABL height.
Reply: All figures have been rechecked and, where necessary, adjusted to be color-vision-deficiency friendly. In Fig. 2, the dropsondes are now categorized according to their release location, over the sea ice, over ocean, and in the MIZ. In addition, the color scheme (consistent with Fig. 3) now distinguishes the air mass origin.

- Line 172: First the figure should be introduced and then the interpretation should be done. Move the first sentence to after the sentence ending on Fig. 3.
  Adopted

- Line 177: The sentence “Consequently, ...” is a repetition of what was said in the sentence before. Can this be consolidated and maybe combined?
  Reply: We acknowledge that the sentence partly repeats the previous statement; however, it also provides a simplified explanation that improves readability and emphasizes the key message. Therefore, we decided to keep it as it is.

- Line 184: I do not see how Fig. 3 informs us on the vertical structure of the ABL? Do you mean the vertical extent? You only investigate the structure later in the manuscript.
  Reply: We have changed “vertical structure” to “vertical extend”

- Line 187: Is it a surprise that the large-scale conditions define the temperature, but local processes determine the boundary layer dynamics? If so, this should be emphasized. If not, I would suggest to add references to contextualize your statement.
  This statement specifically refers to the air masses crossing the sea ice edge, e.g., warm air masses moving northwards form ocean to sea ice and cold air masses moving southwards from sea ice to open ocean. In these cases, the present air mass is mostly driven by synoptic processes. However, air mass transformation is triggered by the changes surface and this mostly affects the ABL. E.g., in cold air outbreaks, the temperature at top of the ABL almost remains constant, while the strong surface fluxes over the ocean lead to a mixing of the lower atmosphere that causes an increase of the ABL height (Wendisch et al., 2025).
  The citation (Wendisch et al., 2025) was added in line 188.

- Line 189: The definition of the normalized altitude should be introduced when it is actually used in Section 3.2.
  Reply: We understand the reviewer’s point. However, the definition is placed in this section because it is part of the methodology and therefore fits best within Section 2: Methods.

- Line 203: “as” is missing and the explanation for combining the regimes for MPH could be supported by an appendix figure, if the authors want to.
  Reply: The word “as” has been added. We appreciate the suggestion to include an additional figure in the Appendix. However, we believe that the description in the text

is sufficiently clear, and further details on the microphysical properties of the individual regimes 2a and 2b are already presented in Moser et al. (2023).

- Line 220: The introduced regimes and abbreviations should be used explicitly and not converted back to full text (whole paragraph).
  Adopted

- Figure 4: add in the caption that the percentiles were calculated based on bootstrapping.
  We have added to the figure caption: "The percentiles were calculated based on bootstrapping."

- Line 244: Why the reference to Moser et al., 2023b: It is just the sum of IWC and LWC, no? Here the CWC defintion needs to come earlier in the manuscript.
  Reply: This issue has been resolved during the revision process in response to a previous comment.

- Line 255: The Wegener-Bergeron-Findeisen process describes in principle the evaporation of cloud droplets and the growth of ice crystals. However, for cloud droplets to evaporate, water subsaturated conditions must exist, otherwise they are not evaporating. Then both cloud droplets and ice crystals would grow. So, the WBF process by itself does not imply water saturation, but rather the available cloud droplets. So one needs to be careful when arguing with the WBF process. Also I would argue that the WBF process is not persistent.
  Reply: We agree with the reviewer that the original sentence was not scientifically accurate. We have therefore revised the sentence in Line 255 as follows: "In a persistent mixed-phase cloud state, a quasi-steady balance exists between the ongoing Wegener-Bergeron-Findeisen process, which transfers water mass from liquid droplets to ice crystals, and dynamical processes that compensate the resulting mass transfer."

- Line 257: the word cloud is missing, but anyway the abbreviation MPC should be used. I agree that there is water saturation, but the WBF process is definitely not the only process helping to sustain that. This is too-short handed argued.
  Reply: We have added the term mixed-phase state at this point. We intentionally avoid using the abbreviation MPC here, as throughout the manuscript this abbreviation is only used when explicitly referring to the in-situ identified mixed-phase cloud regime.

- Line 260: What do you mean by greater sensitivity of the liquid phase to the enviroment?
  Reply: By this statement we refer to the higher sensitivity of liquid particles to environmental relative humidity compared to ice particles. Because of the short relaxation time of liquid droplets (only a few seconds; Korolev et al., 2017), the relative humidity distribution within mixed-phase clouds is mainly determined by the liquid phase. In contrast, ice crystals can persist in subsaturated air masses due to a much longer phase relaxation time, which is comparable to the lifetime of an entire ice cloud (Krämer et al., 2009; Rollins et al., 2016; Korolev et al., 2017).

- Line 262: higher supersaturation than what?
  We have adopted this sentence: "However, liquid clouds are observed more frequently at colder temperatures, resulting in slightly higher levels of supersaturation with respect to ice than is observed within MPCs."

- Line 265: Isn't it the defitnio of MPH and ice that RH < 100 %?
  Reply: This refers to an observation in our dataset, which we attribute to the mixed-phase haze (MPH) regime.

- Line 274: Are these really stable thermodynamic conditions? Wouldn't the existence of ice crystals with the haze droplets mean some metastable state?
  Reply: We thank the reviewer for this comment. Given the frequent occurrence of these cloud regimes, we assume that they represent a relatively stable or metastable condition. However, we cannot demonstrate that this stability is of thermodynamic origin. To avoid overinterpretation, we have therefore removed the word thermodynamic from the sentence: "…, suggesting the presence of a stable condition."

- Line 314: Can you explain the process how GCCN are impacting ice sublmation and cloud droplet growth?
  Reply: The study by Ji et al. (2025) shows that giant CCN can reduce the water vapor pressure over large solution droplets, which may shift the vapor equilibrium between liquid and ice. This can suppress ice growth or enhance droplet growth, thereby affecting the balance between ice sublimation and droplet condensation. We have added the sentence: "Large CCN can lower the equilibrium vapor pressure over the droplet surface, potentially inhibiting ice growth and extending the mixed-phase lifetime."

- Line 323: Why did you choose 300 m?
  Reply: Since this analysis also investigates the influence of the surface on the haze particles, it was important to consider only data within the ABL. The altitude of 300 m was chosen as a compromise: low enough to ensure that most data points are within the ABL, but high enough to include a sufficiently large statistical sample.

- Line 327: dotted should be dashed
  Adopted

- Line 336: I thought the MPH have water subsaturated conditions, how can there be liquid water present?
  Reply: Haze droplets consist of a mixture of water and dissolved salts. These particles are not activated cloud droplets but exist in equilibrium with the subsaturated environment. Although they are much smaller than typical cloud droplets, they still contain a substantial fraction of liquid water.

Figure 7: The colors could be in agreement with Figure 3 and instead of using the same linestyle for separation the total, use maybe dotted.
Adopted

- Figure 8: Can you turn the pie chart in such a way that “0 %” starts at the top of the circle? This way it is more intuititve.
  Adopted

- Figure 9: What is the white shading in a, c, and e?
  Reply: We have added the following sentence in the figure caption: “Insufficient data due to low statistics are grayed out.”

- Line 376: Why is the glaciation efficiency higher in more stable conditions?
  Reply: We thank the reviewer for pointing this out. We agree that the original sentence was misleading, and we have removed it in the revised manuscript.

- Line 385: Here an assessment of the lifetime of MPH would be great.
  Reply: We agree with the reviewer that an assessment of the lifetime of MPH would be highly interesting. However, with the available in-situ aircraft data, it is only possible to estimate this to a limited extent. A detailed analysis would require dedicated flight patterns specifically designed to follow the temporal evolution of a mixed-phase haze layer.
  Nevertheless, one goal of this study is to motivate future measurements and complementary observations of the MPH regime using other techniques. For example, such investigations could be carried out using airborne or ground-based lidar observations to identify potential MPH signatures, observe their evolution, and estimate their lifetime. Moreover, existing lidar or remote-sensing datasets might already contain indications of mixed-phase haze layers that were previously not classified as clouds due to their low extinction coefficients. Revisiting such datasets with the findings from this study in mind could provide valuable insights before new dedicated measurements are conducted. Such an analysis would go beyond the scope of this paper. Our conclusion regarding the potentially long lifetime of the MPH regime is therefore based on its frequent occurrence and on the finding that the MPH regime represents a equilibrium state with the environment due to its microphysical composition.

- Line 399: Does not the mixed-phase state imply that we have liquid and ice crystals? So this is circular?
  Reply: We would like to clarify the wording. The term mixed-phase state refers to the regimes 2a, 2b, and 2c, all of which contain both liquid water and ice. However, in regimes 2a and 2b the liquid phase is present in haze particles, whereas in regime 2c it is found in cloud droplets.

- Conclusion point 2 and 4: These are the same to me and could be combined.
  Reply: In conclusion point 2, the microphysical composition of the two regimes is described, whereas conclusion point 4 focuses on the environmental conditions under which the two regimes are observed and the corresponding physical explanation. To improve readability, we have swapped points 3 and 4 in the revised version.

- Line 406: The optical thickness was only assessed visually, no? Can you quantify it?

Reply: This comment has been addressed during the revision process as part of the major comment. We thank the reviewer for this helpful remark.

- Line 421: Are MPH really persistent? You did not show anything regarding lifetime?
  Reply: See response to the comment on Line 385.

References:

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