# Review for "The Arctic low-level mixed-phase haze regime and its microphysical differences to mixed-phase clouds" by Moser et al., ACPD

## Summary

The study presents arguments for distinguishing between Arctic mixed-phase clouds and Arctic mixed-phase haze. The latter occurs at water subsaturated conditions and consists of non-activated haze droplets and ice crystals. Clear cloud microphysical differences are identified and support the presence of the Arctic mixed-phase haze. The authors combine several measurements from the HALO-(AC)$^3$ campaign, which also allows them to characterize the vertical structure of these clouds in the Arctic boundary layer as well as to assess the chemical composition of the measured aerosol particles. The authors did a nice job on combining all these data and draw new insights on Arctic mixed-phase haze. I recommend this study for publication after addressing my comments below.

## 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?

## 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.
- 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.
- 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.
- Line 36: replace "including those" by (e.g., ....).
- Line 44: using a threshold of LWC = 0.01 g m$^{-3}$
- Line 48: mixed-phase temperature regime needs to be defined (in numbers)
- Line 49: the Wegener-Bergeron-Findeisen process should be at least explain in 1-2 sentences, because it appears throughout the manuscript.
- Line 50: One-sentence paragraphs should be avoided.
- 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.
- Line 66: what does "partially close proximity" mean? Can you just quantify it as you do it later in the text?
- Line 72: "had" to has

- Line 74: reference to Figure 1 when you introduce the location
- 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.
- Line 93: "N" needs to be defined
- Line 121: Why are legs from horizontal flights less significant?
- Line 126: Suggestion for reformulation: Moser et al. (2023b) introduced a method for determining ...
- Line 131: The sentence regarding the thresholds is somewhat confusing, and should be rephrased, such that word repetition is avoided.
- Table 1: the abbreviation were not introduced.
- Line 142: given that CAO and WAI are only used twice, consider not using the abbreviations and just write it out.
- 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.
- Line 165: which data were now used?
- Line 168: the information in the brackets for SIC can be omitted as it was defined before
- Line 170: Suggestion for reformulation: ..., at which the minimum temperature occurs (i.e., minimum of temperature inversion).
- 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.
- 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.
- Line 177: The sentence "Consequently, ..." is a repetition of what was said in the sentence before. Can this be consolidated and maybe combined?
- 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.
- 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.
- Line 189: The definition of the normalized altitude should be introduced when it is actually used in Section 3.2.
- 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.
- Line 220: The introduced regimes and abbreviations should be used explicitly and not converted back to full text (whole paragraph).
- Figure 4: add in the caption that 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.
- 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.

- 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.
- Line 260: What do you mean by greater sensitivity of the liquid phase to the enviroment?
- Line 262: higher supersaturation than what?
- Line 265: Isn't it the defitnio of MPH and ice that RH $< 100$ %?
- Line 274: Are these really stable thermodynamic conditions? Wouldn't the existence of ice crystals with the haze droplets mean some metastable state?
- Line 314: Can you explain the process how GCCN are impacting ice sublmation and cloud droplet growth?
- Line 323: Why did you choose 300 m?
- Line 327: dotted should be dashed
- Line 336: I thought the MPH have water subsaturated conditions, how can there be liquid water present?
- 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.
- 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.
- Figure 9: What is the white shading in a, c, and e?
- Line 376: Why is the glaciation efficiency higher in more stable conditions?
- Line 385: Here an assessment of the lifetime of MPH would be great.
- Line 399: Does not the mixed-phase state imply that we have liquid and ice crystals? So this is circular?
- Conclusion point 2 and 4: These are the same to me and could be combined.
- Line 406: The optical thickness was only assessed visually, no? Can you quantify it?
- Line 421: Are MPH really persistent? You did not show anything regarding lifetime?