

Response to comments of referee #2 on egusphere-2025-2047, “Interhemispheric Anti-Phase Variability in Mesospheric Climate Driven by Summer Polar Upwelling During Solstice Months”

I would like to thank the authors for thorough responses to the referee comments and for the revised manuscript. I am generally happy with the revisions and I find the manuscript almost ready for publication.

We sincerely thank Referee #2 for the positive feedback on our manuscript. We appreciate the constructive comments, which are invaluable in improving the quality of this work.

I am still uneasy regarding the authors' distinction between freeze-drying (involving vertical transport of PMC particles) and cold trap (not involving vertical transport of PMC particles). I understand that there is a “companion manuscript” by Zhang et al. under review at ACP. It seems that that review process is almost completed, and I suggest to refer to the companion manuscript (discussion paper) in the present manuscript. Once the companion paper will be published, I assume that future studies will need to address the suggested distinction and the relative importance of the PMC freeze drying and PMC cold trap mechanisms. In any case, the focus of the present manuscript is on the role of upwelling for the mesospheric chemistry, and I would argue that the conclusions of the present manuscript are actually not critically dependent on the distinction between freeze-drying and cold trap. Maybe the authors want to comment on that. So, as of now, I am satisfied with the description of both processes in the current manuscript.

We thank the referee for raising this important point regarding the distinction between the freeze-drying and cold-trap effects.

We have now cited our companion manuscript (Zhang et al., 2025) *in lines 124 and 268*, which provides detailed microphysical foundations supporting the cold-trap effect.

We fully agree that the conclusions of this paper—specifically concerning the role of upwelling in mesospheric chemistry—do not critically depend on the precise distinction between these two effects. Both mechanisms result in to similar patterns of dehydration above and hydration below PMCs, which adequately explain the chemical responses presented in this study.

However, there is one related issue that must be clarified in order to avoid potential confusion for readers not familiar with water vapour in the middle atmosphere:

The main reason why the upper mesosphere and thermosphere are dry (“dehydrated”) is the photolysis of water vapour by solar UV radiation. This is particularly prominent in the polar summer mesosphere because of the permanent solar irradiation. This basic fact should be clearly stated in the manuscript when mesospheric water vapour is discussed (sections 1.1, 2.2 (point 1), and 4.1).

In the polar summer, PMC add to this dehydration of the upper mesosphere and thermosphere, but they are not the major cause. The manuscript uses wordings like “ice particle growth that blocks upward H₂O transport, causing de-hydration above PMC in the summer hemisphere” (line 259-260). Such wordings are potentially confusing as a reader may understand that PMC are the major cause of low water concentrations at higher altitudes.

An instructive reference is e.g. Siskind et al. (2018) “Understanding the effects of polar mesospheric clouds on the environment of the upper mesosphere and lower thermosphere”. This paper is already cited in the present manuscript, albeit only as an example of PMC effecting odd hydrogen chemistry.

The more important message from that paper is how much PMC actually affect the global water vapour budget water in the mesosphere (based on WACCM model simulations). See in particular their Figure 1, comparing the global distribution of water vapour in a world with PMC and without PMC. I suggest to add this to the discussion with an explicitly reference to this paper.

We sincerely thank the referee for this important clarification.

We agree that it is essential to clearly emphasize the dominant role of solar UV photolysis in dehydrating the upper mesosphere, particularly in the polar summer region under continuous illumination. This fundamental mechanism has now been explicitly highlighted *in lines 32-33* of the revised manuscript.

As noted by the referee, although PMCs contribute to local dehydration above the cloud layer, they are a secondary factor compared to photolytic loss. To avoid potential misinterpretation, we have clarified *in line 261* that the PMC-related dehydration/hydration primarily concern the interannual variability in water vapor, rather than the background water vapor profile which is dominated by solar UV photolysis.

We also fully agree on the relevance of Siskind et al. (2018) and thank the referee for underscoring its importance. Given that most WACCM simulations do not include an interactive PCM module, the results presented by Siskind et al. (2018) offer particularly valuable model-based insight into PMC-induced water vapor redistribution—especially through the comparative analysis (Figure 1 therein) of simulations with and without PMC feedback. *In lines 39-40*, we have expanded our discussion to explicitly reference their findings.