Response to RC4

We thank the reviewer for their comments and extremely positive feedback on the quality of our work, which we greatly appreciate.

In our revised manuscript, we have:
(i) added more context to the introduction to address the additional studies suggested by the reviewer;
(ii) pointed out the novelty of the PPE and our inability to compare it to other results;
(iii) changed the INP fits to work with logarithmically spaced values and clearly stated the significance of each fit using more precise language and listing Spearman correlation coefficients; and
(iv) updated our figures to implement the reviewer's suggestions.

More changes than listed have been implemented to accommodate the suggestions by the other three reviewers. Also, figure numbers are shifted by one due to the incorporation of a new figure depicting a synoptic overview (now Fig. 1). Below we list our answers to the specific comments.

Responses to specific comments

Lines 42-48 ("While most of the aforementioned...contributor to extratropical UTLS moisture."): I would like to see more literature on UTLS moisture transport in the extratropics cited, with explanations of how the authors' study relates to this literature. Examples of articles the authors may want to cite include Weigel et al. 2016 ("UTLS water vapour from SCIAMACHY limb measurements V3.01 (2002-2012)"), Heller et al. 2017 ("Mountain waves modulate the water vapor distribution in the UTLS"), and Sun et al. 2017 ("Characteristics of water vapor in the UTLS over the Tibetan Plateau based on AURA/MLS observations").

Thank you for bringing these studies to our attention. We have revised the introduction to provide a more accurate summary of the extent to which studies have investigated moisture transport in the extratropical UTLS. (Lines 46-59)

Table 1: It would help to show in the table whether perturbations for each parameter between the min and max are spaced linearly (e.g., -2, -1, 0, 1, 2 for SST), logarithmically (e.g., 0.01, 0.02, 0.04, 0.08, ..., 5, 10, 20 for INP), or otherwise, as well as the number of distinct values tested for each parameter.

We have added this information to Table 1.

Lines 339-345 ("We interpret this inability...with parameter perturbations."): The authors should try to compare their results to related findings in at least one study besides Oertel et al. (2025) and see whether any explanation for their results can be found in past literature, and if so, whether the explanation given in past literature is plausible or whether a new explanation is needed. If the authors' results contradict past literature, or if no past literature exists for comparision, that too would be important to mention.

The study done by Oertel et al. (2025) is, to our knowledge, the only perturbed parameter ensemble study ever conducted on warm conveyor belts and there is little other systematic information on the impact of microphysical choices for warm conveyor belt moisture transport. Therefore, our findings cannot be compared to previous studies. However, we now point out the novelty and uniqueness of the study more clearly in the introduction.

Lines 384-393 ("We interpret these findings...ice mass mixing ratio qi."): See comment for lines 339-345.

See comment above.

Lines 406-417 ("We interpret...(heavier ice particles fall out more quickly).): See comment for lines 339-345.

See comment above.

Lines 422-432 ("This clear dependancy...dominate the ice-phase cloud microphysics."): The authors should check to see whether any related findings exist in any past literature. If so, the authors should cite the literature and relate their analysis to the analysis done in the literature. If not, the authors should clarify that their findings and analysis are original.

See comment above.

Lines 460-462 ("The same is true the other way around...(red dots in Fig. 7 b)."): Please see my comment about figures 7b and 7e. I am not convinced that any linear fit between INP and RH (tao_sat) is statistically significant. Once a relationship between INP and RH (tao_sat) is decided upon, the statement in lines 460-462 should be updated.

Figures 7b and 7e: I am not convinced that the linear fits plotted are worthy of publication. As the authors mention, the INP scaling axis is logarithmic, yet the relative humidity and timescale for supersaturation over ice are fit to linear functions of INP

scaling. This creates a situation where only a small fraction of the data has statistical significance in estimating the slope of the fits. Further fuelling my skepticism, the random variability in the median relative humidity and timescale appears to be at least as significant as the total variance captured by the linear fits. If the authors choose to fit relative humidity and timescale for supersaturation over ice to INP scaling, I urge them to consider non-linear fits and test whether any best fit, particularly for any non-linear relationships supported by previous literature, has a significant Spearman correlation coefficient.

This is a very good suggestion. For INP, we have now changed the fit instead to work on the logarithm of the INP scaling factor. In the updated Figure, the line is now straight and more clear.
We revise the text to additionally note the spearman correlation coefficients for these fits, and relativize the significance of the trends (i.e., point out weak correlations), such that the reader is clearly informed on the significance of the correlation and the trends we describe.

Technical comments:

Figure 1: The text in the legend above the subplots should be made larger.

Figure 1c: The text on each axis and on the colorbar should be made larger.

Figure 8: The text in the legend above the subplots and the numbers on each subplot's axes should be made larger.

Figures 9-10: The numbers and text in colorbars and the numbers along axes should be made larger

We have implemented all of these changes concerning the Figures.