Response to reviewer 1

We would like to thank reviewer 1 for a very thoughtful and detailed review of our manuscript that has greatly contributed to the improvement of the paper. To address the recommendations of both reviewers regarding the readability of the graphics and the length of the captions, we have made the following changes:

1. Figures 1 and 2 have been simplified and cleaned up.

2. Figures 3 and 4 have been slightly enlarged, and redundant notation has been removed.

3. Both figures in the appendix have been revised to incorporate the additional requests made by the reviewers (see below)

4. The captions have been significantly shortened. Relevant portions have been moved to the main text or the appendix.

5. Most of the changes to the text have been made in the appendix. We would like to note that we are still striving to meet the requirements of ACP Letters, which include a limit of 2500 words for the main text and 200 words for the abstract.

6. The title has been changed to: "The dehydration carousel of stratospheric water vapor in the Asian Summer Monsoon Anticyclone"

In the following, we address all the points (marked in blue) that were raised in the review (denoted by italic letters).
Major comments:

1. *This is an important paper, and it provides good evidence that "convective moistening of the stratosphere" over monsoons is far more complicated than some of the earlier proponents have envisioned. Previous discussions of the convective moistening process have assumed that Convection reaching above the tropopause simply resets the relative humidity to saturation. In this study, it is evident that the air parcels may continue to dehydrate due to the elevated cold points as they move around in the Asian monsoon anticyclone. In my own mind, the higher the convection, the colder the air due to adiabatic expansion and the more complete the dehydration. This paper shows that parcels launched at the top of other convective events, can transit through colder air undergoing later dehydration.*

   Thanks a lot for these nice words

2. *I found the abstract quite descriptive and useful. I recommend that a longer version of the abstract be repeated in the summary section which could be expanded.*

   Thank you for your positive feedback on the abstract. We understand your suggestion to include a longer version of the abstract in the summary section. However, we are constrained by the word limit imposed by ACP Letters, which restricts us to 2500 words in the main text. Nonetheless, we have made some efforts to provide a comprehensive overview of the study within the given constraints.

3. *In general, the figures are hard to read, and the captions are too long. The author might consider breaking up the figures into smaller groups and edit the captions. I would delete Fig. 5, not very helpful.*

   See our explanation above. We understand your point regarding Fig. 5. While it may seem trivial to those familiar with the topic, it serves an important purpose in addressing a common misconception prevalent in the experimental community. There is a widespread opinion that moistening above the cold point tropopause (CPT) can be considered as "irreversible" moistening of the stratosphere, completely neglecting the Lagrangian view. Given this context, I believe it is valuable to include Fig. 5 in the manuscript. In fact, it played a crucial role in motivating the writing of this paper.

Minor comments:

1. *I would reference Brewer (1949) as the originator of the CP regulation of water vapor theory.*

   was done

2. *The introduction is too brief to cover this complex and important scientific field. For example, you might also expand on some of the previous publications mentioned. The Randel & Park (2019) paper is a particularly important prelude to these conclusions. Additionally, there are additional trajectory model simulations by Ueyama & Schoeberl and collaborators are relevant - these papers also used convection and ice formation models. The Avery paper focusses on El Nino, not the monsoon. Lumping the regular monsoon convection system with El Nino seems like a stretch to me.*

   We appreciate the reviewer's feedback. The introduction has been expanded accordingly. The citation for Ueyama et al. (2023) has been included, and the publications have been categorized into regular convection and monsoon convection for better clarity.

3. *Table 1 is confusing. 10.08 flights? Is this the date? Why is this relevant? I would put a comment in the Table on the difference between Type A and Type B. Perhaps a comment line "recent convective influence" and "aged convective influence" for A and B - something that the reader can immediately grasp*

   great recommendation. we followed this idea and changed the table and the text around it

4. *I would put the references to the instruments in Figure 1 caption into the text. All the references make the caption difficult to read. "time distance?" you mean time since encountering an LDP.*

   The references were moved to the appendix. and the formulation "time distance" was replaced by the "LDP age" and explained both in the main text and in the caption of Fig. 1

5. *The exact LDP is a little uncertain since gravity waves could create an LDP even after the temperature along the path has warmed up a little. I assume you observed temperature fluctuation measurements as part of the aircraft flights. You could translate this into an uncertainty in the LDP time using Delta-T and the temperature along the path. These fluctuations could be important. It wasn't clear from the text that Podglajen et al. (2016)*

gravity wave parameterization is included, or if it is included, does it match observations over mountainous Himalayas?

Yes, we agree that the exact definition of the LDP is a little uncertain since gravity waves may influence it. Our definition is based solely on the temperature fields resolved in ERA5 data. However, CLaMS-Ice applied in this paper uses the Podglajen et al. (2016) parametrization of gravitiy waves, which follows a statisical approach to represent the missing temperature fluctuations in ERA5. Note, as we discuss it (caption of Fig. 3), the influence on our results is small.

6. *You might add some additional references on CO photolysis beyond von Hobe (2021). CO is measured by MLS. Minschwaner et al., (2010) is the classic paper on CO lifetime, also see Liang et al. (2023) and references therein.*

   Minschwaner et al., 2010 reference is now included.

7. *Clearly type B is "aged air" with higher ozone, lower CO whereas type A is "younger air". So it was a little surprising to see the LDP age for type A all over the map (Fig.1 C). This confusing point was straightened out in Fig. 1d so maybe 1c could be eliminated or make the symbols smaller.*

   great recommendation, we removed figure 1c and simplified in this way our story following general recommendations of both reviewers

8. *FIG. 2 - it might be useful to locate where the Part b Lagrangian dry point is located on the map shown in Part a. I would have shown the type A trajectory in 2c - makes your point better - and put the Type A label inside 2d. Remove the not-needed information from caption of Fig. 2*

   we included now two panels in Fig 2 with the full age of air derived from the back-and-forwar trajectories

9. *Line 80. CALIPSO does not detect ice mixing ratios. It detects particles and then using a model the ice mixing ratios are inferred... maybe "..which can be used to infer ice mixing ratios (Avery et al., 2012)".*

   was included, see L86: "We compare the ice distribution calculated by CLaMS-Ice during the dehydration periods along the forward trajectories with CALIPSO observations, which detect ice mixing ratios larger than $\sim$0.1 ppm (Avery et al., 2012)."

10. *Fig. 4 caption, although way too long, was actually readable.*

    ...one sentence was removed, a new citation was added following the recommendation of the other reviewer

11. *How does the aircraft temperatures compare with ERA5. The type B trajectories will encounter ERA5 temperatures, if these temperatures are too warm and you are downstream from the coldest temperature, then you might see a bias. Can you validate these temperatures against GNSS-RO?*

The comparison between the in-situ temperature measured onboard of the Geophysica with the ERA5 data interpolated at these points is now discussed in Fig. A1b. We did not includ any other validation.

12. *How do you account for the vertical averaging kernel in the MLS measurements?*

We do not account for the MLS vertical averaging kernel. We are aware in the huge differences in the vertical and horizontal resolution between MLS (3 km/200 km, respectively) and the spatialy higly resolved in-situ observations. The missing agreement between these two data sets, especially the wet bias of the in-situ observations is interpreted in this study as a non-repesentativeness of our in-situ observations for the large-scale distributions sampled by MLS. We discuss now this point in much more details in Fig B1b and B1c, see also the related text.

13. *Line 100 Schoeberl and Dessler used forward trajectories.*

was corrected

14. *I think some explanation on what is done with full trajectories is needed. Does the full start at the measurement point and go backward X days, or - like Ueyama et al. (2023) does it terminate at convection?*

Thanks for this remark. We use the full back-and-forward, in total 120 days trajectory to determine the LDP and do not terminate the backward trajectories above the convective towers. This explanation is now included in L164-5

15. *Line 111 "highest ice concentration found mainly at southern edge." Where the temperatures are coldest according to Fig. 4.. might want to point that out.*

good idea...the respective sentence was completed

16. *Line 117 ...vertical sampling resolution than CALIPSO*

The word "vertical" is now included

17. *Line 124 "are not able to freeze out the excess water" ...assuming the temperatures from ERA5 are correct and there are no gravity waves. How much colder would the temperatures have to be to get the right water vapor? I suspect only a couple degrees...*

We compare now temperatures observed onboard of the Geophysica with the ERA5 temperatures. See Fig. 1Bb and the related text.

18. *Line 126.. I am confused about the backward trajectories. Presumably you start with the aircraft measurement of water and you go backward in time to get a temperature field.*

Our approach is indeed simpler. We utilize the Lagrangian dry point of the backward trajectory to calculate the minimum saturation mixing ratio, which we refer to as FDM-full. This information is described in the caption of Figure B1c in the revised form of the manuscript.

19. *Then starting with a saturated parcel at the furthest back time where it has encountered convection, you dehydrate and arrive at the predicted measurement. Do the two values of water agree? I am wondering if the instrument measured air might be wet biased. Do they*

agree with MLS? I think that this weird Type B bias needs more discussion as to possible sources of error.

Based on the reviewer's ideas and questions, we have included a new Figure B1c to address these points. Our findings consistently demonstrate that regardless of how we approach the analysis, in situ data of type B are consistently nearly twice as large as MLS observations. While there is still potential for factors such as "missing microphysics," gravity waves, trajectory errors, or match errors to contribute to this discrepancy, we believe that the non-representativeness of these small-scale structures appears to be the most probable explanation.

20. *I would delete Fig. 5. I found it confusing and not helpful.*

In response to the reviewer's comment, we understand the confusion and concerns regarding Figure 5. However, we believe that including this figure is important for pedagogical reasons. One of the key motivations behind our paper was to address the misconception, often held by experimental researchers, that being above the Cold Point Tropopause (CPT) automatically implies being in the stratosphere. The figure helps to illustrate and clarify that rather a Lagrangian than an Eulerian view matters, especially in relation to water vapor. We are open to suggestions on how we can improve the figure to make it clearer and more useful for the readers.