Response to reviewer 2

We would like to thank reviewer 2 for a very thoughtful and detailed review of our manuscript that has greatly contributed to the improvement of the paper.helped to improve 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. *Title/Abstract*

   In the title and abstract, the discussion of high-concentration water vapor content appears, but the main text is more about the relationship between the formation of low-concentration water vapor content above the CPT and ice clouds. It would be better to change the title/abstract along the main subject or vice versa. If you want to investigate the behavior of high-concentration water vapor above the CPT, why not analyze cases where water vapor is more than 7ppmv shown in Fig.1a? It is also not clear the reason why Type B and M were used for analysis. Please describe the reason to select those cases.

   We initially started with all the local flight data and then applied specific conditions, including a water vapor concentration larger than 7 ppm and ozone concentration larger than 100 ppb, while focusing on data observed above the cold point tropopause (CPT). This selection process resulted in three distinct types of data: A, B, and M. Type A data primarily represents air with signatures of fresh convection, while type B data consists of air with aged convection, and type M data represents mixed properties. All these 2315 data points are relatively moist (>7 ppm) and in the stratopshere (above CPT with $O_3 > 100$ ppm.
   Our main focus in the paper is to demonstrate the potential for significant dehydration effects along the forward trajectories, despite the initially high water vapor concentrations observed above the CPT. We emphasize the importance of the "Lagrangian view," which highlights

the possibility of moisture depletion along these trajectories. This challenges the common assumption of strong and irreversible moistening of the stratosphere based solely on the "Eulerian view." We have also changed the title of our paper.

2. *Structure of manuscript*

   Many explanations and interpretations (and it is important!) of the figures were written on the appendix, and it was very difficult to read. Implying that it may be a difficult task to summarize in a short report. For example, "fresh convection signature" in Figure 1 was explained in the caption of Figure A1 of Appendix A, and the explanation of the trajectory analysis and the definition of CPT were in Appendix A those are important matter for this study.

   Sorry for that. We included now some changes of figures, captions and text to improve the readability of the paper (see above). Both, definition of the CPT and LDP are in the main text.

3. *Representativeness of selected data*

   Regarding the effect on water vapor in the lower stratosphere, which is the purpose, there is a gap because the trajectory analysis is performed with only a limited number of observations (Type A,B and M). Further, related with my major comment (1), it is unclear whether the selected cases are actually representative ones. It is recommended that you add the reason for the selection and add an evaluation of its validity.

   Of course, the avaiable data can be only interpreted as a case study. We state this point very clearly in the "Discussion and conclusions" part. See L132-141

4. *Quality of trajectory model*

   The trajectory model used for the analysis may be the latest, but what is the guarantee of the reliability of the expression of supersaturation and vertical flow (here, diabatic heating is substituted), which exist in many cases? Shouldn't those differences (between observation and simulation) also be included in the discussion section?

   Yes we agree. In the revised version of the manuscript, we included a section related to the quality of the ERA5 temperatures (Fig. A1b) and to the dependence of our results on the length of the trajectories (Fig. B1c) as the quality of the trajectory decreases with its lengths.

Minor comments:

1. *Line numbers in odd pare are missing from page 3.*

   was corrected

2. *p.2, l.26:*
   How scales of "the large-scale moisture budget" for temporal and horizontal? How scales of "the large-scale moisture budget" for temporal and horizontal?

This is probably difficult to derive from only a few statistically non-representative in situ observations. However, a well-validated model could provide insights into the scaling properties of the large-scale moisture budget in terms of temporal and horizontal scales. This could be an area of investigation for future studies.

3. *p.2, l.28:*
   the subtitle should be changed, for example in situ and behavior of selected data etc..

   was changed

4. *p.2, l.30:*
   need the explanation of definition of "the local CPT" here and the "local" means unclear.

   was done

5. *p.3, l.1:*
   as a function of "vertical" distance to the CPT. Let me confirm that this distance was defined in either direction (up/down or above/below).

   good point!, was done

6. *p.3, l.4:*
   Figure 1c: "fresh convection signatures" In the case of "fresh", the CO concentration is likely to be high due to the air from troposphere, but this is inconsistent with the fact that the CO concentration has a range from 30 to 100 ppb. How can this be explained? In relation to this, does the distribution of LDP age from 60 days before in Fig1c mean that it has been present in the lower stratosphere since 60 days before? If so, can you say "fresh"? This reviewer confuses this point.

   Sorry for the confusion. Following the recommendation of another reviewer, we have revised the paper to consistently classify data types A and B as signatures of fresh and aged convection, respectively, while data type M represents a mixture of these properties. It is important to note that these classifications are more statistical in nature. For example, data set A may include air masses with properties indicative of aged convection, such as Lagrangian Dry Point (LDP) values from 3 weeks in the past. This variability may be attributed to errors in the ERA5 data, which generally reproduces the observed properties of data set A but may not for every individual data point within this data set.

7. *p.3, Fig1:*
   To understand the positional relationship in the vertical direction, it is useful to have the potential temperature distribution.

   good point, was done, see Fig. A1a

8. *p.4, l.48:*
   Considering supersaturation, should it be removed?

   The formulation was slightly changed

9. *p.4, l.63-64:*
   "where..." Is it not fully represented by FDM, or does it look like it has not been removed because the fluctuation of FDM is small?

GW are not included in FDM and this is not the reason for the differences to CLaMS-Ice. Much more, in FDM, ice is removed insantanously while in CLaMS-Ice ice removal occurs through the interplay of condensation, evaporation, and sedimentation. The respective text was reformulated

10. *p.4, last paragraph:*
On Type A, the temperatures are rising in the first three days (Fig 2c), but there is a lot of ice (Fig2d). In Fig 3, is it due to the sedimentation of the ice that the decrease in water vapor is remarkable after 7 hours and 2 days?

    At the beginning, the temperature is around 192K with some small-scale fluctuations. During the first two days, the temperature remains relatively stable, and sedimentation becomes the dominant process that significantly affects the water vapor distribution. By day 3, the temperature increases to 195K. During this period, sublimation of ice crystals becomes increasingly important but does not significantly influence the distribution. This is why the distribution between 2 and 5 days does not change dramatically.

11. *p.5, bottom of Fig 2c:*
It is better that the y-axis direction is opposite. It is easy to understand to express that the temperature is low at the top, that is, the altitude is high.

    To simplify our story and to reduce the number of figures, this panel was completely removed as Fig 2b explains most of this behaviour

12. *p.5, l.2 in caption of Fig 2.:*
in Figure 1 (a). Three representative... (need comma)

    Comma was included

13. *p.5, l.5 in caption of Fig 2.:*
replace to slash, 60/43/54

    was done

14. *p.6, last sentence of caption in Fig3.:*
back (grey) and forward (black)

    was done

15. *p.7, l.9 in caption of Fig 4:*
light grey -> white? In general, the 2-3PV is defined as dynamical tropopause. The value of 5.8-6.2 PVU is higher than general.

    As ashown in Kunz et al. JGR, 2011, at lower θ-levels around 350 K, dynamical tropopopause around 2-3 PVU is a good definition. However, at θ=380 K, like in this figure, higher values between 5 and 7 PVU are more appropriate. This was the motivation for our choice. The respective citation is now included

16. *p.8, l.88:*
on "spread" for Type B; Can't you visualize how much the trajectory is "spread"? Is it possible to add an example of spreading to Fig2a?

    was done, see Fig. B1a

17. *p.8, l.112:*
Is it correct (good correlation) to focus only on the south side of ASM in the case of Type B?

Now we discuss in Fig B1a the spread of the type B air, in particular the part detached from the ASM anticylone and transported into the lowermost stratosphere. This part is also analysed in Fig B1d and the related text

18. *p.9, l.121:*
Figure 5(c) -> Figure 4(c)

was done

19. *p.9, Fig5:*
The horizontal and vertical lines for supplementation in the figure are too thin to be seen.

the lines are now thicker