

Response to Referee 1

We thank the reviewer for the careful reading of our manuscript and the constructive feedback. In revising the paper, we went through the entire manuscript to improve the wording, consistency, and overall clarity. Below, we address each of the reviewer's comments in detail, indicating the changes made in the revised version.

General Comments

The authors have satisfactorily addressed my previous comments. This work is a valuable contribution to the community's understanding of water vapor anomalies above Northern Hemisphere summer monsoons. I have a few minor comments that the authors should address prior to publication.

Minor comments

- Line 9: "water vapor is predominantly controlled by local temperatures near the tropopause in the Asian Monsoon." Does this refer to water vapor across the tropics, or just water vapor near the Asian Monsoon?

Thank you for pointing this out. We have changed the sentence to "... water vapor in the Asian Monsoon is predominantly controlled by local tropopause temperatures" (L8-9).

- Line 14: "an underestimation of moistening due to convective ice injection may play a role in this region." Is there a reason to suggest that the link between convection and the reconstruction dry bias is specifically driven by ice injection? I would suggest making this statement more general if not.

We removed the specific "ice injection" (L14).

- Line 43: "Our goal is to evaluate the role of the freeze-drying mechanism in the large-scale temperature and wind fields for the enhancement of stratospheric water vapor over the ASM and NAM regions from a Lagrangian perspective." This study explores water vapor anomalies

throughout the UTLS, not just the stratosphere. I think the region of the study (i.e., the UTLS) should be made clear here to prevent readers from focusing only on results in the stratosphere. We have changed the stratospheric water vapor to UTLS water vapor for this sentence (L42-43), and we also modified other texts that have similar issue.

- Line 55 and throughout the text: The term “deep tropics” is misused. This refers to the region within the tropics closest to the equator (e.g., 10S to 10N). It would be sufficient to just refer to the study region (35S to 35N) as the tropics.

Thanks for clarifying this. We have modified the corresponding texts at L44.

- Line 65: “How well can stratospheric water vapor mixing ratios in the ASM and NAM as observed by SAGE III/ISS and MLS be reconstructed using a simplified Lagrangian modelling method. . . .” Similar to a previous comment — does this study aim to focus on the stratosphere or the UTLS? This distinction matters for the analysis, especially given the emphasis on reconstructions at 16.5 km in Figures 1, 5, 6, and 8. (16.5 km is below the cold point tropopause across the tropics.) If the study is intended to focus on the stratosphere, then the analysis needs to be done above the cold point tropopause.

Our focus is on the UTLS region. We thank the reviewer for noting the confusing formulation and have revised the first research question to read: “How well can UTLS water vapor mixing ratios in the ASM and NAM be reconstructed . . .” (L44–45).

- Line 205: The term “tropical tropopause layer” is used incorrectly here and in the following discussion. The TTL spans 150 hPa to 70 hPa, or 14 km to 18.5 km. It is not the layer between the lapse rate and cold point tropopauses.

We avoided using this term wrongly (L178-179).

- Line 236: “whereas small-scale mixing appears to be a more dominant contributor.” This is not part of the analysis and is speculative. Why the focus on small-scale mixing here? What about, e.g., ice injection?

Sorry for the confusion. Here, we mentioned that the small-scale mixing was based on Plaza

et al. (2021). To improve clarity and flow, we have removed this sentence in the revised manuscript (L189-191).

- Lines 347–348: “The locations of LCPs for the tropics (Fig. 5a–b) resemble an ensemble of those found in the ASM and NAM, suggesting that dehydration predominantly occurs near the monsoon regions.” Figure 5 shows that the LCPs are concentrated around southeast Asia for each ensemble. It does not show LCPs for the tropical ensemble centered around the monsoons. Instead, Fig. 5 shows that cold trap dehydration mechanism (Holton and Gettelman, 2001) dominates the tropical trajectories (as is correctly noted in the following sentence).

We agree with this and have revised the text to read: “For the ASM (Fig. ??c), the LCPs cluster near the monsoon region and exhibit high reconstructed mixing ratios consistent with elevated cold-point temperatures. For the NAM (Fig. ??e), although some LCPs are located in its vicinity, a considerable portion is concentrated over southern Asia.” (L236–238)

- Lines 365–366: “ $OLR \geq 1.5$ standard deviations” and “ $OLR \leq -1.5$ standard deviations” would be more clear if stated as “ $OLR \geq 1.5$ standard deviations above the mean” and “ $OLR \leq 1.5$ standard deviations below the mean.” (Assuming that the mean is used.)

Modified (L261-262).

- Lines 438–442: This paragraph implies that the Lagrangian method is effective across the tropics. This needs to be qualified to acknowledge the method’s ineffectiveness in capturing the NAM water vapor anomalies.

Sorry that we didn’t express the information clearly enough. Now the paragraph has been re-organized (L320-322).

- Line 465–466: “Hence, it is likely the underestimated moistening effect of ice injection of convection in the western region of the Asian monsoon which controls the dry bias of Lagrangian reconstructions in the ASM.” Convective ice injection is not part of the analysis, so this statement is speculative. Why the focus on ice injection here? Is it known that convection over the western sector brings ice particles to the UTLS?

We avoid using this speculative statement. Now the sentence is modified to read: "These findings suggest that underestimated moistening from convection in the western part of the ASM is a key driver of the dry bias in the reconstructions." (L337-338)

- There are a handful of typographical errors that need to be addressed:
 - Line 8: "The main dehydration, region ..." should be "The main dehydration region, ..."
 - Line 13: "dry bias in reconstruction ..." would read better as "the dry bias in reconstructions ..."
 - Lines 59–60: Citations need to be in parentheses.
 - Line 260: comma before "thus" should be a semicolon.
 - Line 407: "simulates the conversion of excess water vapor to ice, and setting parcels to saturation within convection zones" should be "simulates the conversion of excess water vapor to ice by setting parcels to saturation within convection zones."

We thank the reviewer for identifying these typographical issues. They have been corrected accordingly.

Response to Referee 3

We thank the reviewer for the careful reading and the thoughtful and detailed feedback. We agree that some of the wording and formulations may have caused unnecessary confusion and could still be improved. Following the overall recommendation of the reviewer, we thoroughly worked through the manuscript text again to enhance clarity and conciseness. A few examples of changes which apply to the overall manuscript are summarized here:

- In the revised version of the manuscript, we follow the recommendation of the reviewer and consistently use the term “*advection-condensation paradigm*” to describe the methodology applied in this study. In this sense, we avoid the usage of different terms to describe the method, as was done before (e.g., freeze-drying, advection-condensation, etc.).
- Similarly, the use of the term “*anomaly*,” which may have been unclear before, has been changed. In the revised version, we clearly define the type of anomaly before using the term, especially distinguishing between spatial and temporal anomalies, as this distinction is essential for interpreting the results correctly.
- Overall, we tried to shorten sentences, removed parentheses, etc, to simplify the wording.

More detailed replies to the specific points are given further below. We emphasize, that we not only considered these specific points provided by the reviewer but worked over the entire manuscript to improve the text consistently. We are confident that these changes significantly improved the manuscript such that it is ready for publication now.

Specific comments

- L21: Suggested rewording: “... where air masses undergo slow diabatic ascent ...” “...into the stratosphere over timescales of weeks to months.”

Revised (L20-21).

- L36: What exactly is the tropical stratospheric water vapor anomaly? An anomaly with respect to what? Likewise for the summertime NH extratropical water vapor maximum.

We appreciate the reviewer's comment and acknowledge that the wording was imprecise. The ASM contribution to the tropical stratospheric tape-recorder moist signal has been quantified as 25% by Bannister et al. (2004) and 14% by Nützel et al. (2019). However, since we have restructured the Introduction, this discussion has now been removed.

- L41: Replace *predict* with *assess*.

Revised (L34).

- L42: The sentence here is confusing. The large-scale temperature and wind fields are no more than a representation of the atmosphere that the reanalysis provides. They do not have their own set of processes. The freeze-drying occurring in the actual atmosphere can of course be estimated with reanalysis wind and temperature fields—that's the methodology here after all. But as I read the sentence, it implies that there is a freeze-drying mechanism for the large-scale fields and another at finer resolution.

We thank the reviewer for pointing this out. To avoid confusion, we have revised the sentence to read: "... we aim at evaluating dehydration processes in the UTLS over the ASM and NAM regions from a Lagrangian perspective. " (L42-43)

- L50: The phrase beginning with *to reconstruct* ... and ending with ... *Version 5.0 Lambert et al. (2017)* is a nightmare of parentheses. Since this is only the introductory section, there is no immediate need to specify version numbers of datasets or their specific references. This can and should be the business of Section 2. In any case, by eliminating all those version numbers and references, the profusion of parentheses goes away. Furthermore, identifying the SAGE III and MLS versions here implies that the updates they represent were somehow critical to the outcome of the study, which I am pretty sure is not what you are saying.

Thank you the specific suggestion. We have revised the text accordingly (L49-55).

- L59: This sentence amounts to an awkward juxtaposition, inasmuch as you have just pointed out the advantage of the SAGE III data over MLS. The advantage of the MLS data, of course, is their unmatched temporal and spatial coverage. In this regard, SAGE III/ISS is a very poor

cousin indeed.

We agree that MLS provides much greater temporal and spatial coverage. SAGE III/ISS offers finer vertical resolution (about 2 km, due to the 1–2–1 smoothing, L92–94), which permits more vertical structure to be depicted on a 0.5 km grid. To remove the juxtaposition, we have revised the text to state the MLS coverage advantage and the SAGE III/ISS vertical-resolution separately, without suggesting equivalence (L50–52).

- L60: Improper use of *furthermore*. Use another, more appropriate, conjunctive adverb.

We have re-organized the paragraph and now use “Finally” as a short transition (L57).

- L68: ... are most critical. Critical for what, exactly? Presumably remote control, but that is not stated.

Sorry for causing confusion. The sentence has been changed to: “Are the moisture enhancements observed within the ASM and NAM anticyclones locally or remotely controlled and which regions contribute most strongly to these enhancements?” (L46–47)

- L83: It would be helpful here to describe the particular sampling challenge presented by the MLS data. A 10° latitude $\times$ 20° longitude grid box is a big piece of real estate, but nonetheless at the latitudes of interest in this study, on most days it would be traversed by only a couple of MLS overpasses, one ascending (afternoon) and one descending (nighttime). The limb-viewing geometry also comes into play here, in particular the 200 km swath length along the orbit path and at the lower MLS levels (100 hPa and below) where spatial variability rears its ugly head. In short, there are a number of temporal and spatial sampling considerations that almost certainly play a role in a three-dimensional gridding of MLS data, and the same can be said for SAGE III/ISS. Given their very different times at which they sample alone, MLS and SAGE III/ISS “see the world,” as it were, in different ways. Are these different perspectives significant? I don’t know—I didn’t do the work! But I think it’s a fair question and should be addressed here.

We thank the reviewer for the detailed comment. We acknowledge that MLS and SAGE III/ISS differ in sampling characteristics, including spatial coverage and observation times, and we appreciate the concern. To clarify, we have revised the text (L71–72, L94–95), that horizontal

gridding was applied only in Fig. 1 to illustrate the horizontal distributions. For the vertical profiles in Fig. 2 and Fig. 7, data were averaged within the defined horizontal boxes. For all other analyses, no horizontal averaging was performed. In particular, for the CLaMS trajectory simulations, air parcels were released at the exact observing location and time of each profile, and correlation coefficients were calculated using individual points. Therefore, despite their different sampling strategies, we consider the reconstruction performance of MLS and SAGE III/ISS to be comparable. For experiment LAG, we added 50 more trajectories around each observation point to enlarge the ensembles vertically, covering $H \pm 0.25$ km (H is the height of the observation point) for both SAGE III/ISS and MLS. We recognize, however, a remaining incomparability, as we did not account for the large vertical kernel used for MLS. We will keep this in mind and consider a better approach in future work.

- L88: Fix reference.

Corrected (L75).

- L90: Missing terminal parenthesis, period, and double space.

Corrected (L77).

- L97: The latitude range 35°S – 35°N is incorrectly referred to as the subtropics.

We corrected the "subtropics" to "tropics" (L84).

- L97-L100: The previous subsection on MLS does not mention a specific period, so presumably the study period mentioned here, the months of August from 2017 to 2022, applies to both MLS and SAGE III/ISS. But perhaps not, as the second sentence in the paragraph says that data for the years 2020 -2022 were added in order to get more spatial coverage. I find it hard to make sense of what's going on here unless there are actually two periods of study here: a 2017 -2019 period for MLS and SAGE -III, and a supplemental 2020-2002 period just for SAGE III/ISS. If this is in fact the case, then the trustworthiness of the foregoing comparisons between the MLS and SAGE III/ISS reconstructions is seriously undermined. That it was the case that MLS data were not used in the latter period is strongly implied by the statement that there is

no significant difference between the SAGE reconstruction results from the two periods. This of course begs the question of why MLS data weren't used over the entire 2017-2022 period in the first place. If there is a technical reason for this, one isn't mentioned.

NOTE: The question of the two study periods is answered at L131. That this is referred to only obliquely here muddies the narrative unacceptably.

We thank the reviewer for this helpful comment. We agree that the distinction between the study periods was not sufficiently clear in the original version. In the revised manuscript, we now explicitly state at the beginning of the subsection that MLS data are analyzed for 2017–2019, while SAGE III/ISS data are extended to 2017–2022 to ensure sufficient spatial coverage (L84-86). We also clarify the technical reason for using the shorter MLS period: the large volume of MLS data makes it computationally prohibitive to perform all trajectory calculations over a longer time span, particularly given that we launch large ensembles for each measurement point (the specific trajectory numbers are provided in Section 2.3.1). To address the reviewer's concern regarding the robustness of the comparison, we now include a direct comparison of SAGE III/ISS reconstructions using only 2017–2019 versus the full 2017–2022 period (Fig. S1). The results show no significant differences, demonstrating that the extended SAGE III/ISS period does not affect our conclusions and thus justifies our choice to continue with the 2017–2022 period for SAGE III/ISS.

- L115: In this paper, the term anomaly is used primarily for the difference between a derived value at a given geographical location from its long-term mean. However, here it refers instead to the difference between an (OLR) instantaneous value and its temporal mean. The latter usage is the more natural one as I see it, but in any case the dual usage here leads to some confusion in the text. In particular, back at L36 the phrase tropical stratospheric water vapor anomaly is in the same sentence as the summertime NH extratropical water vapor maximum. Is this maximum a spatial or temporal one? It's not clear. I mention this since the spatial features in the water vapor fields that are associated with the ASM and NAM could easily be referred to as local maxima without any ambiguity.

We thank the reviewer for pointing this out. We now explicitly state in the manuscript whether a anomaly or maxima is spatial or temporal (e.g., at L102–104 we specify temporal OLR anomalies). The issue raised at L36 is addressed in our earlier response.

- L150: Suggest replacing along with in. The word “along” is more suitable for horizontal spans or temporal stretches.

Revised (L136).

- L151: The SAGE III/ISS CPTs used in LOC are from the MERRA-2 reanalysis while the trajectory reconstructions use ERA5 reanalysis temperatures. This necessarily introduces a complication into any comparison between the LOC and two Lagrangian experiments.

We thank the reviewer for pointing this out. In the original version, we used MERRA-2 temperature profiles because SAGE III/ISS water vapor data are reported as number densities on altitude and converted to mixing ratios using MERRA-2 temperature and pressure profiles (Park et al., 2021). We acknowledge that this introduced an inconsistency with the ERA5-based trajectory reconstructions. Upon checking, we found that CPTs from MERRA-2 and ERA5 are highly consistent; nevertheless, to avoid unnecessary complexity we now use ERA5 temperatures throughout and have updated the corresponding text (L136) and figures (Fig. 3 and Fig. 4) accordingly.

- L172: Here is the first reference in the manuscript to the direction of crossing the tropopause. If it’s not significant enough consideration to merit mention in the Introduction, why does it figure into the methodology?

The explanation for TST in the methods section is mainly to distinguish air parcels that are transported from the troposphere into the stratosphere from those circulating within the stratosphere. Since our results used both TST and non-TST trajectories and they are reconstructed differently, we consider this needs to be explicitly described in the methods. In the Introduction, we have now stated the direction of tropopause crossing when introducing the Lagrangian perspective (L36-38).

- Figure 1 (from L180):

- The caption should identify the level of the water vapor fields in the panels. The reader should not need to refer to the text.
- The lower four panels are labeled as either Observation anom and Reconstruction anom.

I would simply identify them as maps of the differences between the reconstructions and the satellite observations.

- The concluding clause in the caption is awkward English, the use of the word strings in particular.
- The lower panels are labeled as “Observation anom,” which suggests that identifying them as maps of the differences between the reconstructions and the satellite data would be preferable. Thus change the portions of TST shown with upper right strings of c–d and g–h to “TST fractions noted in panels c–d and g–h.”

We thank the reviewer for the suggestions. The anomalies in panels (e–h) are spatial anomalies relative to the tropical mean of the corresponding distributions in panels (a–d). Instead of using the difference between the reconstructions and observations, we use the anomalies to highlight that the reconstruction can reproduce the anomalies in the ASM, though with a common dry bias. If using the difference, the dry bias will diminish the presentation of the good performance of the reconstruction.

We now have revised the caption to: Horizontal distributions of water vapor concentrations and anomalies in August. Panels (a–b) show observed H₂O concentrations, (c–d) reconstructed concentrations from Experiment LAG, and (e–h) the corresponding spatial anomalies, based on SAGE III/ISS at 16.5 km (left) and MLS at ~16.3 km (right). The anomalies are calculated relative to the tropical mean (35°S–35°N)...

- L181-L192: In keeping with my preceding comment, the spatial features in the eight panels in Figure 1 are variously referred to as “enhanced”, “elevated”, “high”, “maxima” and “anomalies”. If, as I think is the case, all these terms essentially refer to the same thing, a single term should be used. Otherwise the text is ambiguous.

We thank the reviewer for noting this inconsistency. In the revised manuscript, we use the terminology more consistently: “maxima” to describe the high values over the monsoon regions in Fig. 1a–d, and “moisture enhancement” for the positive anomalies in Fig. 1e–h (L165–173).

- L195: A simpler way to put this is that the reconstruction only faintly reproduces the observed NAM water vapor pattern.

Revised (L170).

- L196: Change increase to elevation. The word increase implies a temporal change.

We modified the sentence according to the suggestion above and thus avoided using the word “increase.” (L170)

- Comment on the discussion of Figure 1: The spatial patterns displayed by the positive-valued reconstruction “anomalies” are a nice result, in particular their similarity to the maximum in the field of SAGE III observation in the vicinity of the ASM. While the text at L188 does note the overall dry bias of the reconstructions for each satellite dataset, one might expect that outside regions like the ASM, where the stratospheric entry values of water vapor are controlled locally, the reconstruction anomaly field would be more or less flat. Instead, the negative-valued anomalies more or less mimic the patterns in the observations, in much the same way as the positive-valued anomalies. I do not know if there is a simple explanation for this or not, but it seems that whatever is causing the coherence of the positive-valued reconstruction anomalies with the observed fields may also be contributing to a coherence of the negative-valued anomalies.

We are very pleased that the Lagrangian reconstruction captures the positive anomalies well, especially within the ASM region. This can be explained by temperature-driven condensation processes that are well represented in the ERA5 temperature fields. As pointed out by the reviewer, this also appears to be the case for the negative anomalies. The explanation is essentially the same: the reconstruction reflects the temperature history along the trajectories. Even though the backward trajectories are generally longer for this more stratospheric part of the domain, the coherence with the observed patterns remains. In summary, this highlights the strength of the Lagrangian reconstruction—already noted in many earlier studies—in its ability to reproduce stratospheric water vapor anomalies based on the Lagrangian temperature history.

- Figure 2 (from L197):

- (caption) Replace concentrations with mixing ratio throughout.

- (caption) Replace “For each subplot, it shows” with “Each panel shows.”
- (caption) Properly, biases are not “between” fields/variables, but the difference of a particular estimate of a variable/field from a “reference” or “true” value of the same.
- (caption) The phrase “reconstructed values subtract observed values” needs to be corrected. Suggest “reconstructed minus observed.”

We have applied the wording changes as suggested in the revised manuscript.

- L202–206: It is not surprising that the reconstructions do so poorly below the tropopause, as this is where remoistening by clouds is a factor.

We agree. Now we have added a short sentence to discuss this (L176-178).

- L221, L237: What is the basis of the statement that in the ASM region the tropopause layer is higher and thinner than elsewhere in the tropics? Likewise, that the NAM is also thinner but lower?

Sorry for causing the confusion. We meant the ‘tropopause layer’ as the layer between the lapse rate tropopause and the cold point tropopause, which are indicated by the yellow and cyan lines. Now we consider such description was not appropriate and helpful, so we have removed them (L182, L192).

- L224: It is not clear what the phrase vertical performance means.

We rephrased the sentence to read: “The contrast between the coral and grey bars in the right sub-panels illustrates how well the moisture enhancements relative to the tropical mean are captured.” (L183–184)

- L225–L229: As written, neither of these sentences makes sense. What do “one-third of the observed anomalies” and “over two-thirds of the observed values” mean? I assume these refer to the magnitudes of the reconstructions, but the phrasing is poorly chosen.

We have revised this to read:” At 15.5 km, reconstructions based on SAGE III/ISS reproduce about one-third of the observed enhancement magnitude. Agreement improves with altitude,

exceeding two-thirds at 16.5 km and approaching close consistency above this altitude.” (L184-186)

- L230–L236: How is the “consistent behavior of the reconstruction in the ASM compared to the tropics” evidence for the water vapor above the ASM being affected by something called “mechanisms-freeze-drying in the large-scale temperature”? This is its first mention in the paper, and if it is indeed the “advection–condensation” paradigm of Liu et al. (2010), is there a reason it needs to be given a new name—and a grammatically unpleasant one at that? In any case, what exactly is this “mechanisms-freeze-drying in the large-scale temperature” process and how is it related to the “consistent behavior” noted above?

We thank the reviewer for pointing out this issue. The phrase “mechanisms–freeze-drying in the large-scale temperature” was an inappropriate wording. In the revised manuscript, we first introduce the advection–condensation paradigm at L35 and consistently refer to this established term throughout (e.g., at L187–188).

References

Park, M., Randel, W. J., Damadeo, R. P., Flittner, D. E., Davis, S. M., Rosenlof, K. H., Livesey, N., Lambert, A., and Read, W. (2021). Near-global variability of stratospheric water vapor observed by sage iii/iss. *Journal of Geophysical Research: Atmospheres*, 126(7). Cited by: 6; All Open Access, Green Open Access.