

Response to Reviews

Thank you for taking the time to provide feedback on our manuscript! Responses to all suggestions are provided here in blue text.

Please note that we have also added additional co-authors to the revised manuscript. These authors contributed to setting up and running the WACCM simulations but were missing from the initial submission due to a regrettable oversight. In addition to the changes suggested by the Reviewers, we have also made a few changes suggested by the coauthors. The figures were also updated to include MLS and ACE-FTS observations up to August 2026.

RC1

General comments

This paper considers the fate of the water vapour deposited in the stratosphere by the 2022 Hunga eruption. This topic has been considered in a number of earlier papers; the paper reviewed here makes further progress by more detailed modelling studies and by using a more sophisticated technique to separate the Hunga water from the background.

The main scientific suggestion that I would make would be to re-work the plots using the newly-released version 6 of the MLS data. Version 6 is very similar to version 5, but the artefact at ~33km altitude has been eliminated, so the process to remove the artefact recommended by Millán et al. (2024) is not required. While the authors would probably regard version 6 as more satisfactory I understand that it may be too much work to re-do all of the analysis, so the paper could be published using version 5 if the authors consider that this is the most practical way forward.

The paper appears generally well written and illustrated. I consider that it should be accepted subject to some minor corrections.

Thank you for the suggestion. We did not originally use MLS version 6 because the manuscript was largely complete by the time it was publicly released. However, we were able to redo the analysis, and all figures in the revised manuscript use MLS Version 6- the effect of switching the data version on our results is negligible.

Specific comments

Line 77: "Version 5 of the MLS retrieval is used here." As noted in the general comments I would recommend re-working the paper using version 6 of the data.

We are now using MLS version 6.

Line 157: I found figure A1 to be rather illuminating; it seems a shame to relegate it to an appendix.

This figure is in the appendix due to its similarity to Figure 10 in Randel and Park (2016), who developed the water vapor reconstruction method.

Randel, W. and Park, M.: Diagnosing Observed Stratospheric Water Vapor Relationships to the Cold Point Tropical Tropopause, *Journal of Geophysical Research: Atmospheres*, 124, 7018–7033, <https://doi.org/10.1029/2019JD030648>, 2019

Lines 163--64: I think that the first paper to report the water vapour injected by the ANY event was Schwartz et al. (2020) (<https://doi.org/10.1029/2020GL090831>) At any rate it was published ahead of the papers currently cited here.

This additional citation has been added.

Some of the multi-panel figures (especially figures~4 and 6) contain a lot of small detail. The authors should ensure that they end up printed large enough. Journal layout staff sometimes seem to prefer huge white margins to sufficiently large detail on two-column figures; the authors should resist this tendency.

Yes, larger figures are preferred! We will do what we can within the constraints of the journal.

Technical corrections

Line 84--85 "(Millán et al., 2024)" should be "Millán et al. (2024)" i.e. the citation should be a `\citet{}` and not a `\citep{}`. (If the authors follow my suggestion to use version 6 this sentence will be removed anyway.)

Yes, this sentence has been removed given that we no longer use MLS v5.

Line 96 and elsewhere: Units should appear in the same font as the running text. The fact that they do not is a long-standing bug in the LaTeX package supplied by the journal; the authors should not waste their time trying to fix it as it will be fixed anyway when the final version of the paper is typeset.

As you point it, this is a latex problem that can be fixed by the copyeditor.

Figure 1 The legend items for "ACE" in (a) and "Residual ACE" in (b) do not match the plot in that the plot has only dots and the legend items have a line as well.

Thank you for pointing this out, the legend now only shows a dot for the ACE-FTS entries.

Line 174: km is a unit and should appear in a non-italic font, as is already the case for ms-1 and gmol-1. (As noted above, the journal's LaTeX package will use the wrong non-italic font; the authors should not worry about that.)

Yes.

Lines 216, 322, 377: Replace "likely" with "probably". Likely should not be used as an adverb in formal writing; it is an adjective synonymous with "probable". (Yes, this is a personal beef with all of North America which I have no hope of winning.)

This change has been made in all cases, despite most of the authors being North American.

Line 229: "(shown in Figure 7)". Journals usually expect the first mentions of figures to be in order, in that the first reference to figure n should come after the first mention of figure n-1 and before the first mention of figure n+1. There are a variety of ways to address this. One possibility is to put the current figure~3 and the current figure~7(b) into an extended figure~3; this would make the paper clearer and easier to follow.

Rather than changing the figures, we have removed the reference to figure 7 from this section of the paper.

RC2

This paper investigates the changes in the distribution of stratospheric water vapor, in the years following the large direct injection of steam by the Hunga volcano in Jan 2022, from contemporary satellite observations and comparisons with model simulations. The Hunga excess H₂O lingers on in the stratosphere, transported around by the BDC, until various removal mechanisms take control, such as photolysis following ascent into the mesosphere, the seasonal dehydration caused by sedimentation of ice PSCs (overwhelmingly in the SH) and the stratosphere/troposphere exchange of air masses; the latter two must await for the advection of the excess H₂O into their domains of influence. The 2022 Antarctic vortex was already in place before the H₂O excess was transported far enough poleward so the dehydration removal process did not come into effect until the following year. Similarly, strat/trop exchange is not effective at removing the H₂O excess until it has descended to low enough altitudes.

In the study, interannual changes in the water vapor are modeled using CPT temperature vs H₂O lag regressions trained on the 2005-2021 tropical time series and extended to the post-Hunga time frame using the observed RO CPT temperature time series to track the vertical and horizontal propagation of the resulting correlated H₂O anomalies.

The conclusion from the paper is that the model simulations agree reasonably well with the observations on the redistribution of water vapor above 20 km, but do not represent an accurate depiction of the removal of stratospheric water vapor from the lowermost regions of the stratosphere. Evidence of excess tropical H₂O during 2023/2024 seen in the observations is interpreted as a recirculation of water from the southern hemisphere mid-latitudes back to the tropics rather than increased transport upwards through the tropopause.

This interpretation seems reasonable, but reporting of the results of earlier studies of the Hunga H₂O excess could be more appropriately worded e.g. L224-L233 and elsewhere seems unusually harsh since the LMS region was omitted or could not even have been considered relevant in some of the works cited given their earlier publication dates.

[We apologize if some of the wording was harsh! We only intended to point out the differences between our results and those from earlier studies.](#)

I recommend that the paper be accepted subject to some minor corrections.

Typos, Suggestions and Other Comments:

/xxx/ means remove xxx

[xxx] means add xxx

L14: suggest /increases/ [prolongs]

[Done](#)

L76: No MLS [H₂O] observations

[Done](#)

L78: /suggested/ [standard]

[Done](#)

L80-81: In May 2024 and onwards the MLS H2O measurement runs for only a few days per month to preserve the longevity of the remaining measurement record. The H2O status flag is set to an odd value for profiles when its measurement system is off and it is the primary indicator of bad or unavailable data regardless of the data quality flag i.e. the status flag must be an even value otherwise the data should not be used.

Yes, we only use H2O profiles that have an even status flag throughout the whole MLS record. The standard values for each flag are now specified in the manuscript.

L87-88: What is the procedure used for conversion from geopotential to geometric heights?

The equation that we use has been added: “The v6 MLS geopotential height (GPH) profiles that are retrieved along with each H2O profile are therefore used to convert the h2O profiles from a pressure grid to a geometric altitude grid using the equation $H = 9.80616 \text{ m/s}^2 \times z_{R_e} / (g_{R_e} - z)$ where H is the geometric altitude in m, z is the geopotential height in m, g is the acceleration due to gravity as a function of latitude in m/s^2 , and R_e is the WGS84 ellipsoid radius as a function of latitude in m.”

L89: The [v5 MLS H2O] artifact correction

The manuscript was updated to use MLS v6 at the suggestion of Reviewer 1, which does not require the artifact correction, so this sentence has been removed from the manuscript.

L107: monthly CPT [RO] temperature

We are not sure that this addition makes sense. Removing the acronyms it would be “cold point tropopause radio occultation temperature”.

L113-118: define the acronyms MOZART-TS/TSMLT, MAM4, CMIP6 SSP2-4.5, MERRA2

All definitions have been added.

L133: define ERA5

Done

L137: don't start a sentence with a numerical value... altitude. 300000

The sentence has been restructured.

L178: observed [RO] CPT temperature

We do not feel that it is necessary to mention the data source each time the observed CPT temperature is discussed, given that it is the same every time.

L227: re-arrange the figures to be referenced in numerical order, this page jumps from Fig3 to Fig7 and skips the ones in between

We have instead removed the reference to Fig. 7 from this section.

L237: subsequent /F/[f]igures

Done

L248: evolution /is/ [are]

Done

L304: curve/d/

Done

L309: suggest /increases/ [prolongs]

We have instead changed “increases” to “extends”

L336: Tracer particles were released in the FLEXPART simulations in December 2023. It does not appear that uncertainties in the back-trajectories were examined. What are the effects of accumulated errors in advection over many months of following these particles? This is in contrast to the treatment of the WACCM simulations, where an ensemble of free-running simulations and a specified dynamics simulation were examined (by the way, why doesn't an SD run deserve the same skepticism and get sampled with probable dynamics errors?).

We use FLEXPART to demonstrate that mixing from midlatitudes to the tropics is a reasonable explanation for the enhanced H₂O, by considering the statistics of the transport for a large number of trajectories, as opposed to using FLEXPART to map specific individual trajectories. In this case, the uncertainties in the individual trajectories are expected to at least partially cancel out, and we are left with reasonable statistics on the large-scale motion. Nonetheless, we have chosen to remove the attempt to relate the FLEXPART statistics to the water vapor mass anomaly, as there are further uncertainties contributing to that calculation.

The WACCM-SD simulation uses assimilated meteorological fields (from MERRA-2), and there is not an ensemble of reanalysis winds available to test sensitivity. Hence there is only one WACCM-SD simulation to analyze.

Community Comment – Graham Mann

Thank you for taking the time to comment on our manuscript! We have taken the suggestions into consideration when preparing our revised submission.

1) Re: the finding from last sentence of the Abstract (line 14): this new satellite measurements analysis “increases the decay time scale of the Hunga stratospheric water vapour by 1-2 years”

The authors state categorically the cause of this change (compared to other recent studies, e.g. Schoeberl et al, 2024; Zhou et al., 2024; 2026) is their methods include the lower stratosphere in the calculations, aligned to the statement on lines 10-11 “In the lower stratosphere observations show substantially more H₂O compared to the model during 2024 and 2025”.

While updating our results during the review period we observed that the water vapor decay rate in the first half of 2026 above 20 km was slower than in prior years. The result is that the decay time is now similar, whether or not the lowermost stratosphere is considered in the calculation. Based on this, the last line of the abstract has been changed to simply say “Based on the MLS observations, the stratospheric water vapor anomaly will return to pre-Hunga levels around the year 2031.”

That model-obs difference is apparent from comparing Figures 7a (where 60S-60N mass burdens are calculated upwards from 2km above the tropopause) with Figure 7b (same 60S-60N mass burdens, but calculated only above 20km).

The Figures 7c and 7d are clear that both MLS and ACE-FTS are seeing the emergence of an extra 25-30 Tg of water vapour in the Southern Hemisphere lower stratosphere (30-80S), not represented in the model simulations.

The above I agree fully with, but the main comment I make here, is to query the interpretation the authors have given to (fairly definitively) identify this difference as evidence of recirculation from SH mid-latitudes back to the tropics.

We think there may be some confusion in this comment regarding our attribution of recirculation. Our arguments regarding recirculation focus on the appearance of anomalous, positive residual H₂O *in the tropical lower stratosphere* in 2023 and beyond, together with transport calculations from WACCM-FR and FLEXPART. This recirculation argument does not strongly impact our interpretation of the lower stratospheric Hunga H₂O budget results and large differences between satellite data and model simulations.

Firstly, the Figure 7c includes latitudes into the polar regions, through to 80oS, and it is quite possible that a good portion of that emerging water vapour could be simply the arrival of Hunga excess water vapour in the deep branch Brewer Dobson circulation. The timing to emerge in 2024 and 2025 would be consistent with timescales associated with deep branch BDC southward meridional transport from the tropics in the upper stratosphere, and subsequent descent in the wintertime polar vortex.

We agree that the major source of Hunga H₂O in the SH lower stratosphere is downward transport in the BDC. We have tried to make this more explicitly clear in Section 3.3.

The counter-argument might be that we should see a discrepancy earlier, if that were the case, but with the units being shown here being mass burden, any potential model-observation discrepancy in H₂O vapour in the upper stratosphere would be less apparent, with the thinner air in the upper stratosphere and lower mesosphere.

When the air descends however, any discrepancy propagating within the downwards transport into the Antarctic polar vortex would then emerge as a difference in mass, with the increasing pressure.

This point relates also to the 2nd point I make below, that all Figures of the vertical profile (Figures 2, 4, 6 and 9) all show the Hunga-excess water vapour in mass-burden terms.

That choice of unit implicitly weights towards the lower stratosphere, and any model-observation differences in the upper stratosphere would not be apparent when plotted with this unit. Please see the 2nd point where I discuss this further

This 1st point however I note also another possible cause of the lower stratosphere model-observation discrepancy, in relation to this timing of emergence aligning with when the El Nino Southern Oscillation progressed into the warm El Nino phase.

Neither the coupled-model or specified dynamics WACCM simulations described in section 2.4 will be representing this transition into El Nino, and then the transport of water vapour within the tropical upwelling would be expected to be stronger than represented in the simulations (on average), and this could potentially be part of the difference seen in Figures 7c and 7d.

The WACCM-SD calculations use observed meteorology and hence the effects of the (relatively small) warm ENSO event is 2023-2024 are presumably captured in the results. This is not the case for the WACCM-FR results (with random ENSO timing), which do show evidence of midlatitude-tropical transport.

The timing of emergence is potentially consistent also with when the Hunga aerosol descended to the tropopause, for a tropopause heating of the tropical lowermost stratosphere alleviating the freeze-drying effect, allowing extra water vapour into the lower stratosphere (e.g. as demonstrated for Pinatubo in Zhou et al., 2023). However, the magnitude of the aerosol absorptive heating would be less here, and this seems unlikely to be a significant cause of the observed post-2024 increase.

While Hunga aerosols could potentially contribute to tropical tropopause heating, this effect would presumably be captured by the observed CPT variability and then be removed in the H₂O residual calculations.

In summary, this 1st point is to ask the authors to adjust the certainty of their attributing causative statements, when interpreting the differences seen between the observations, and these particular model simulations.

I'd argue this difference is certainly motivating additional model studies to understand the cause of this emerging discrepancy, but please can the authors make the interpretation statements more open as to other potential causes.

2) Mass burden units shown in vertical profile Figures (2, 4, 6, 9) weighted to lower stratosphere

In this 2nd point, I am further noting that considering model-observation discrepancies only in mass burden terms, is implicitly weighting differences to those in the lower stratosphere.

I realise the study is focused to understand the progression of the mass of water vapour in the stratosphere, and I agree totally that mass burden is definitely the variable to consider also.

However, when considering vertical gradients in the stratosphere, the 10-fold difference in pressure between the upper stratosphere and the lower stratosphere, and the deep branch of the Brewer Dobson circulation transporting air upwards into much lower pressure air, requires to consider the vertical gradients in mass mixing ratio terms.

I have attached here a screen-shot PNG of Figure 3.2 from the APARC Hunga atmospheric impacts report (Bourassa, Khaykin et al., 2025), which illustrates how the Hunga-excess water vapour was transported upwards into the upper stratosphere during 2023, consistent with the timing of transition into El Nino (see point 1 above), and the absence of aerosol-absorptive heating from the Hunga water vapour layer separating from the Hunga volcanic aerosol.

What's clear from the Figure, is the transport of the Hunga-excess water vapour into the upper stratosphere and mesosphere, where the air is much thinner.

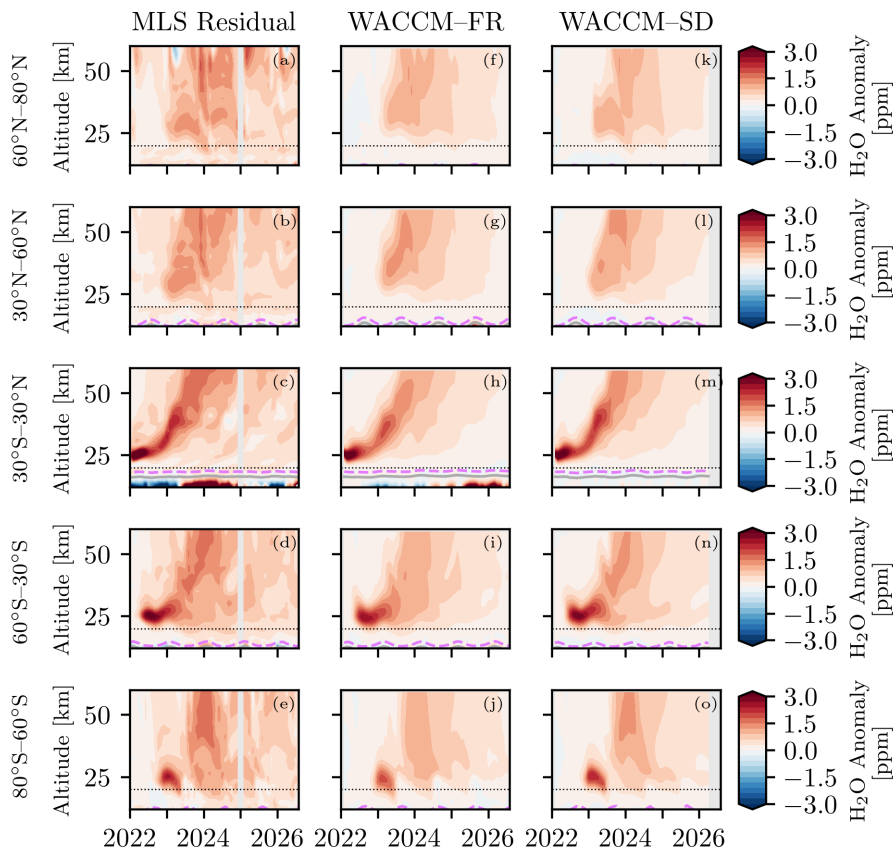
Atmosphere and climate scientists less familiar with stratospheric composition and circulation, may not realise this transition, and indeed for volcanic impacts, for all other large eruptions seen in the satellite era, impacts have tended to be from volcanic species being transported in the shallow branch of the stratospheric circulation, where there is no significant change in pressure.

This 2nd comment is to ask the authors to cross-check for any anomaly between the model and observations in mass mixing ratio terms, specifically perhaps an extra Figure in the same format as Figure 4, but with the 4 columns showing 4 selected latitudes, perhaps 50N, 20N, 20S and 50N, to check the northern and southern tropics, and the northern and southern mid-latitudes?

For these view-graphs, the vertical co-ordinate should extend up to 60km, to be able to see the transport up into the upper stratosphere and lowermost mesosphere.

Yes, it is true that our choice to use mass units rather than mixing ratio units accentuates the water vapor anomaly in the lower stratosphere, compared to the upper stratosphere. A version of Figure 6 in vmr units, rather than mass units, and extending up to 60 km, has been added to the Appendix. In this case, the transport to the upper stratosphere in 2023 is clear. The figure also shows that the smaller water vapor anomaly in WACCM in the lower stratosphere compared to MLS is present at all levels, beginning in 2023. The following discussion has been added to the manuscript:

“The choice to use mass anomaly units rather than mixing ratio units results in larger anomalies in the lower stratosphere compared to the upper stratosphere, as the air is more dense. We therefore also include a version of figure 6 showing the volume mixing ratio (vmr) anomaly up to 60 km, instead of the mass anomaly in the lower stratosphere, in the Appendix (Figure A5). In this case the transport of water vapor to the upper stratosphere in 2023 is clear, along with descent at higher latitudes in 2023 and 2024. WACCM and MLS have similar mixing ratio anomalies throughout the whole stratosphere in 2022. By 2025, the WACCM simulations have a smaller water vapor anomaly than the MLS observations throughout the whole stratosphere. The tape-recorder signal at tropical latitudes in the MLS observations is still clear in the MLS vmr anomaly, but it does not stand out in the WACCM simulations.”



3) The final point I would like to make, in this community comment, is to note a close analogue situation to the current post-Hunga period, from the vertical profiles of radiocarbon emitted into the stratosphere from thermonuclear testing in the 1950s and early 1960s.

Specifically I refer the authors (and other readers of this journal) to the 1989 JGR study by Harold Johnston (Johnston, 1989), analysing the progression stratospheric circulation of excess concentrations of long-lived radionuclides in the quiescent periods of the initial testing moratorium, in 1958-1960, and the years after the subsequent partial test ban in October 1963.

The very extensive programs of in-situ balloon-borne and aircraft stratospheric monitoring of these radionuclides in the 1950s and 1960s (e.g. Friend, 1966, Telegadas et al. 1969) in these two periods, provide useful observational constraints for the circulation of stratospheric aerosol, and in the case of radio-carbon, tracing specifically air circulation relevant to this study's analysis of the Hunga excess water vapour progression in the last 4 years.

Radio-carbon (specifically carbon-14) was monitored extensively, and whereas long-lived fission products such as radio-strontium and radio-tungsten were transported within aerosol particles, the excess carbon-14 followed as essentially a passive tracer of stratospheric air circulation.

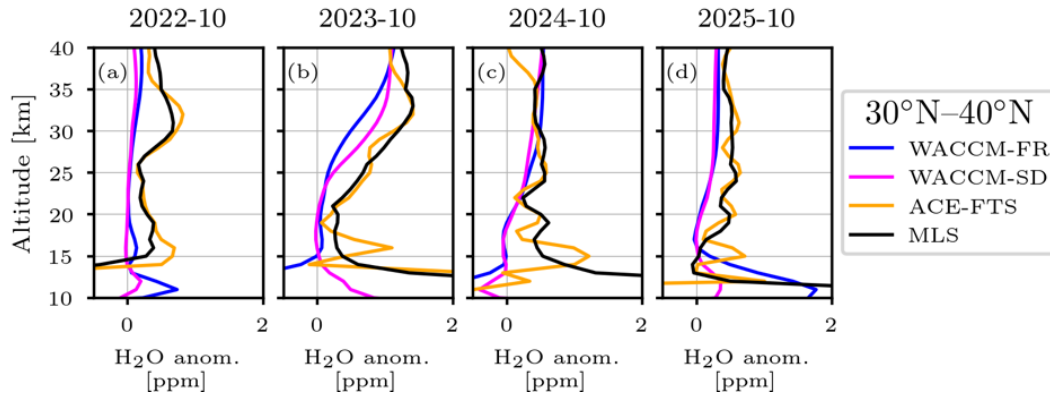
Specifically, I refer the authors to the discussion on pages 18,488 and 18,489 of the Johnston (1989) study, and the summary sentence in the Abstract viz-a-viz the statement explaining how the initial skewed-Gaussian shape of vertical profile layers of excess carbon-14 in the stratosphere (e.g. shown in Figure 5 and Figure 11c, for 31 N balloon soundings) progressed in the 2-3 years after the partial test ban to a “step-staired” profile, with essentially constant mass mixing ratio above 20km.

It is quite striking the similarity between the Oct 1963, 1964, 1965 and 1966 excess radiocarbon profiles at 31N from Figures 5 and 11c of Johnston (1989) with the Hunga-excess water vapour mass mixing ratio profiles in Figure 3.2 from the Hunga report. Specifically, considering the vertical profiles for Oct 2022, 2023, 2024 and 2025 as equivalent timings seem to illustrate very similar progression, for the Hunga excess water vapour.

Since all previous volcanic cases so high into the stratosphere were considering aerosol transport, which (from sedimentation) can never follow the deep branch stratospheric circulation, this analogue situation from radio-carbon monitoring in the years after the partial test ban could be an illustrative and well-observed analogue situation.

Specifically, I ask whether the authors would be willing to plot-up for these months (Oct 2022, Oct 2023, Oct 2024 and Oct 2025) the 31N profiles of the water vapour anomaly from MLS and ACE-FTS, and in the model simulations, which could potentially make an interesting equivalent comparison test case for interactive stratospheric CCMs.

Thank you for pointing out the similarity to the case of thermonuclear testing. This is an interesting point of comparison, however we feel it is out of scope to discuss in the paper. The requested figure is included here instead. Note that the profiles are for the latitude bin from 30N-40N rather than directly at 31 N and we use the residual H₂O anomaly for ACE and MLS.



4) A final note I make, within this extra 4th comment, is to ask the authors whether as a community we should expect a progression into a longer timescale residence time, with this vertical-shift of the Hunga excess water vapour into the upper-half of the stratosphere and lower mesosphere.

This now appears to be what the MLS and ACE-FTS measurements are showing, that initial expectations of the 5-10 year timescale (e.g. from Millan et al., 2022) have lengthened initially to a 2031 expected return date (from Zhou et al., 2026), and suggested here in this new study to be a further 1-2 years later, i.e. 2032 or 2033.

The timescales for removal from the stratosphere must tend to be longer for air masses at this altitude, and then should we implicitly have expected any initial decay timescale, seen through 2022 to 2024, progress later to a slower decay-rate more representative of upper stratospheric air masses, or so. Please can the authors make some comment about this point.

Yes, it appears that the residence time of the Hunga water vapor has increased somewhat since some of the initial estimates were performed. Our findings suggest a return time of 2030-2031 relative to the pre-Hunga 2sigma background variability. When compared to the 1sigma background, as was done in some earlier studies, we find a return time of 2032-2033. Please note that these numbers have changed slightly since the initial submission of the manuscript, as we updated the results to include MLS observations up to August 2026.