

Community comment on “Long-term evolution of stratospheric water vapour from the Hunga eruption”, by Kimberlee Dube et al. (<http://doi.org/10.5194/egusphere-2026-4022>)

I am making here a community comment on this study, mainly in relation to what I consider to be a substantial change this study is reporting, in relation to the picture that is emerging from this new analysis of the latest satellite measurements (MLS with ACE-FTS).

I make 3 main comments in this comment, and do so entirely as an individual, whilst noting my recent international leadership role as one of the four co-ordinating scientists for the World Climate Research Program’s Hunga atmospheric impacts report (APARC, 2025).

The 3 comments each highlight a separate point, but the 1st sets the over-arching theme towards the study’s finding of an increased longevity for the ~10% anomaly in global stratospheric water vapour that resulted from the January 2022 Hunga eruption.

The aim here is towards asking the authors to comment on these over-arching issues, with each comment also stemming from specific findings/Figures from the submitted manuscript.

- 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”.

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.

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.

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 stronger than represented in the simulations (on average), and this could potentially be part of the difference seen in Figures 7c and 7d.

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.

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.

- 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.

- 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.

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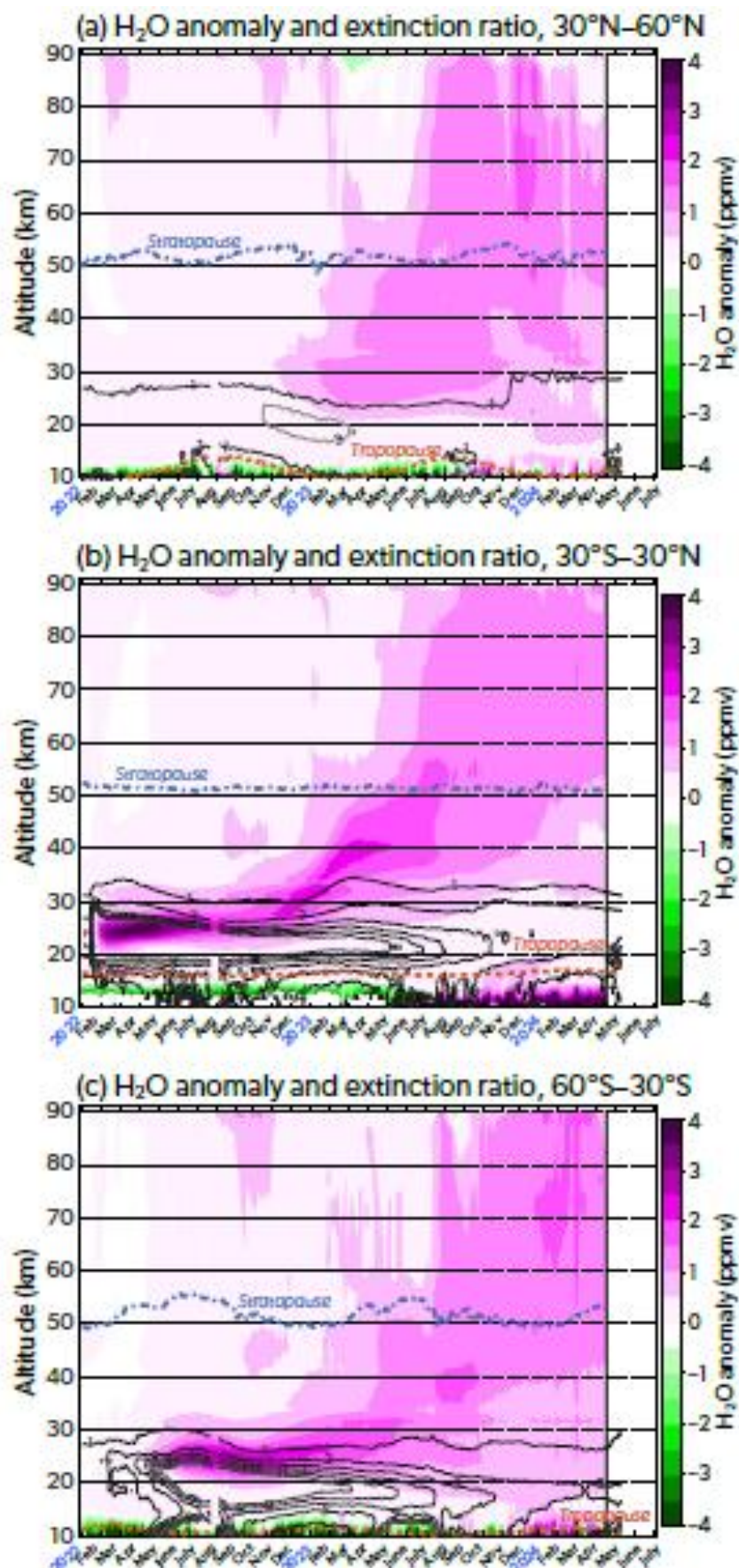


Figure 3.2: Vertical evolution of the water vapour absolute anomaly and aerosol perturbation from MLS and OMPS-LP. Water vapour anomaly (shades) and aerosol extinction ratio (contours) as a function of time and altitude at different latitude bands: (a) 30°N – 60°N , (b) 30°S – 30°N , and (c) 60°S – 30°S .