

**Replies to reviews for:**

**Was the strengthening of the Northern polar vortex in 2024/2025 associated with the Hunga Tonga eruption?**

ACP, Ales Kuchar et al.

## Reply to Ref #1

Dear anonymous reviewer #1,

We appreciate your very helpful comments! We revised the manuscript according to your suggestions and hope the revised version obtains your approval for publication in ACP. Please find our point-by-point answers to the comments below.

Best wishes

Ales Kuchar et al

This study examines the impact of the water vapor anomaly from the January 2022 Hunga Tonga (HT) volcanic eruption on the Northern Hemisphere polar vortex, specifically focusing on the winter of 2024/2025. The paper compares the observed responses with simulations from the SOCOLv4 model with and without the HT forcing, using 10 ensemble members each. The authors show that the Northern polar stratosphere in winter 2024/2025 was characterized by the descent of a significant HT-induced water vapor anomaly (up to 2 ppmv) in MLS observations. The SOCOLv4 simulations, along with an offline radiative transfer model, are used to calculate the direct H<sub>2</sub>O radiative cooling and the indirect cooling caused by reduced shortwave ozone heating due to chemically induced ozone loss. The authors argue that this cooling then strengthened the meridional temperature gradient and the stratospheric polar vortex, which then inhibited wave forcing of the vortex, leading to a stronger than usual polar vortex throughout most of the 2024/2025 winter. The authors also suggest that the winter of 2025/2026 may have an increased probability for sudden stratospheric warmings as the HT water vapor anomaly diminishes.

### General comments:

The paper presents some interesting results which are relevant within the scope of ACP. The possible link between the HT water vapor anomaly and the stronger than normal Northern polar vortex during the 2024/2025 winter ( 3 years after the eruption) is of considerable interest. The SOCOLv4 model results qualitatively illustrate the possible mechanism(s) involved. However, I have some concerns about the interpretation and presentation of results where corrections and/or clarifications are necessary, as discussed below. I would characterize some of these as major revisions which should be addressed for the paper to be published.

### Specific comments:

Abstract, L11-12: "...shift toward sudden stratospheric warmings in the winter 2025/2026 as the WV forcing declines." Do the authors mean more SSWs here? As WV declines back towards pre-Hunga levels, one would expect stratospheric dynamics would also tend toward more "normal" pre-Hunga behavior, i.e., a tendency towards more frequent SSWs and a weaker SPV compared to 2024/2025. Please clarify.

We formulated the sentence as follows: "The observed 2024/2025 winter thus represents a plausible manifestation of HT-induced vortex variability. Our simulations also predicted the observed shift towards more favorable conditions for sudden

stratospheric warmings in the winter 2025/2026 as the WV forcing declines." We chose this formulation because SSW actually happened in March 2026.

35 L20: "WV inputs of HT were distributed. . ." – should specify what locations and time periods are being referred to here, e.g., Southern hemisphere subtropics in the days/weeks following the eruption (see the Hunga APARC report, section 2.3). Similar comment for the following sentence (L20-21), i.e., when were the positive residuals of WV over high latitudes.

We specified the locations and timing according to your suggestion.

L24: Return of stratospheric air to the troposphere at mid-high latitudes by the BDC is also likely an important sink of the H<sub>2</sub>O anomaly.

We reformulated the sentence as follows: "The Antarctic dehydration likely remains the dominant WV removal pathway (Zhou et al., 2024), together with transport of stratospheric air to the troposphere at mid-high latitudes by the Brewer-Dobson circulation (BDC)."

L36: Suggest changing to "linked to a stratospheric WV increase." not "linked to the stratospheric WV increase." since you're not specifically talking about HT here.

We revised it according to your suggestion.

L59: "... illustrate the enormous strength of the SPV." Please be more specific here, i.e., I assume the authors are referring to the fact that although there is large variability, the SPV is generally stronger than average during December-February (2024/2025).

We reformulated the sentence as follows: "...illustrate that the SPV strength exceeds the average strength in DJF 2024/2025."

50 L59-74: The authors show MLS observations of water vapor, ozone, and temperature anomalies for the winters of 2024/2025 (Figure 2) and 2025/2026 (Figure A3). However, there is no discussion of the statistical significance of these results, for example, how these anomalies compare to the range of interannual variability (as done in Figure 1) seen over the MLS time period (2004-2025). The H<sub>2</sub>O anomaly from HT is quite large in 2024/2025, and is likely outside the range of variability and is therefore more easily detectable. But what about temperature and ozone? MERRA-2 temperature anomalies in the Northern polar region during winter 2015/2016 and 2021/2022 are not impacted by HT but show patterns similar to Figure 2. So are the patterns in 2024/2025 uniquely due to HT? Perhaps the HT forcing contributed to the observed responses in 2024/2025, with some partial attribution to natural variability. But this should be discussed and clarified.

We agree that the observational anomalies alone do not allow unique attribution to HT. We therefore added empirical two-sided significance estimates to Fig. 2 and Fig. A3, based on the MLS climatological distribution, and revised the text to distinguish the robust WV anomaly from the less robust ozone and temperature responses. The WV tongue is statistically significant over much of its descent, whereas the negative ozone and temperature anomalies are significant only locally, consistent with a substantial contribution from internal dynamical variability. We now state explicitly that 2024/2025 is not interpreted as a uniquely forced HT event, but as a winter in which HT-related composition anomalies plausibly contributed to the observed vortex state.

65 We also checked using our methodology other strong-vortex winters such as 2015/2016 and 2021/2022 (see Figs. R1 and R2).

While these winters do not reveal comparable amount of increased WV such as in 2024/2025, you can also collocate significant cold anomalies with anomalies of enhanced WV, especially in 2021/2022.

Also, observations for the 2023/2024 winter are not shown. If the HT H2O had not yet reached the Northern polar region (or was too weak to drive a temperature/ozone response) in the 2023/2024 winter, it would be helpful to at least briefly mention  
70 this point.

We checked the 2023/2024 winter (see Fig. R3) and found increased high-latitude WV anomalies comparable to OW3 in terms of magnitude. These were however not accompanied by robust collocated negative anomalies in ozone and temperature. We attribute this to higher meridional gradient between lower and higher latitudes (compare upper and lower panels in Fig. R4) and to slowly descending WV in the case of 2024/2025 (attributable to anomalous residual velocities from MERRA2 (not shown)  
75 or to our new Fig. A10 from SOCOL). This dynamical preconditioning positively feedbacks on radiative cooling implied in the manuscript and strengthens the vortex. We now mention the difference between 2024/2025 and other winters briefly in the text to remain within the requirements of the Letter format.

L65: “negative ozone (< 0.5 ppmv) anomaly” is confusing, since this implies that ozone anomalies of 0 to + 0.5 ppmv are also negative.

80 We removed "< 0.5 ppmv" for simplicity.

L66-67: “descend downward” is redundant.

We removed it accordingly.

L68-70: “In March, we observe a significant increase in both temperature and ozone...”. However, the stratospheric ozone anomalies have both alternating positive and negative bands. Also, the temperature anomaly is positive only in the stratosphere,  
85 with a strong negative anomaly in the mesosphere. These points should be corrected/clarified.

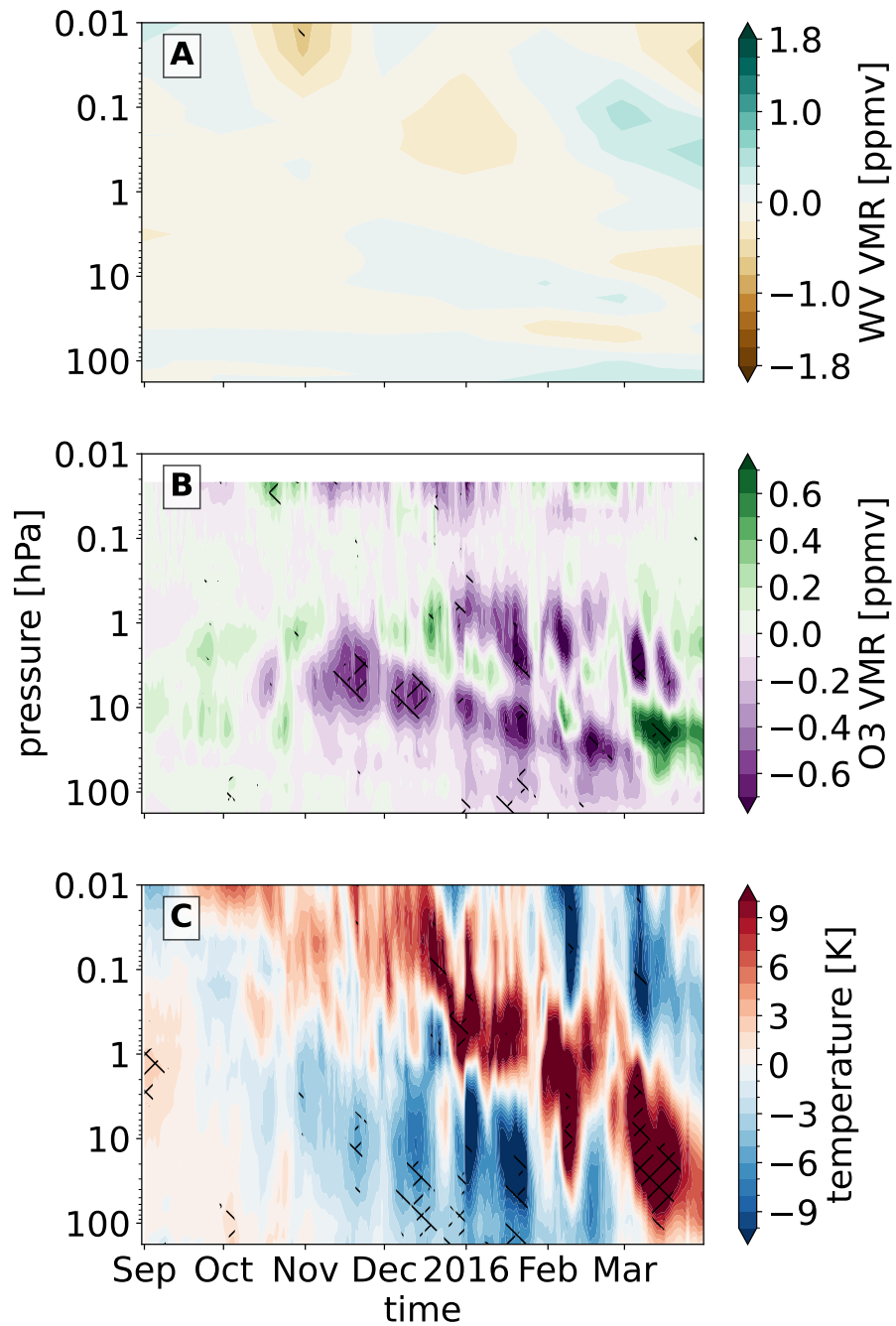
We revised this part according to your comment.

L71: “The by then . . . .” Wording should be corrected.

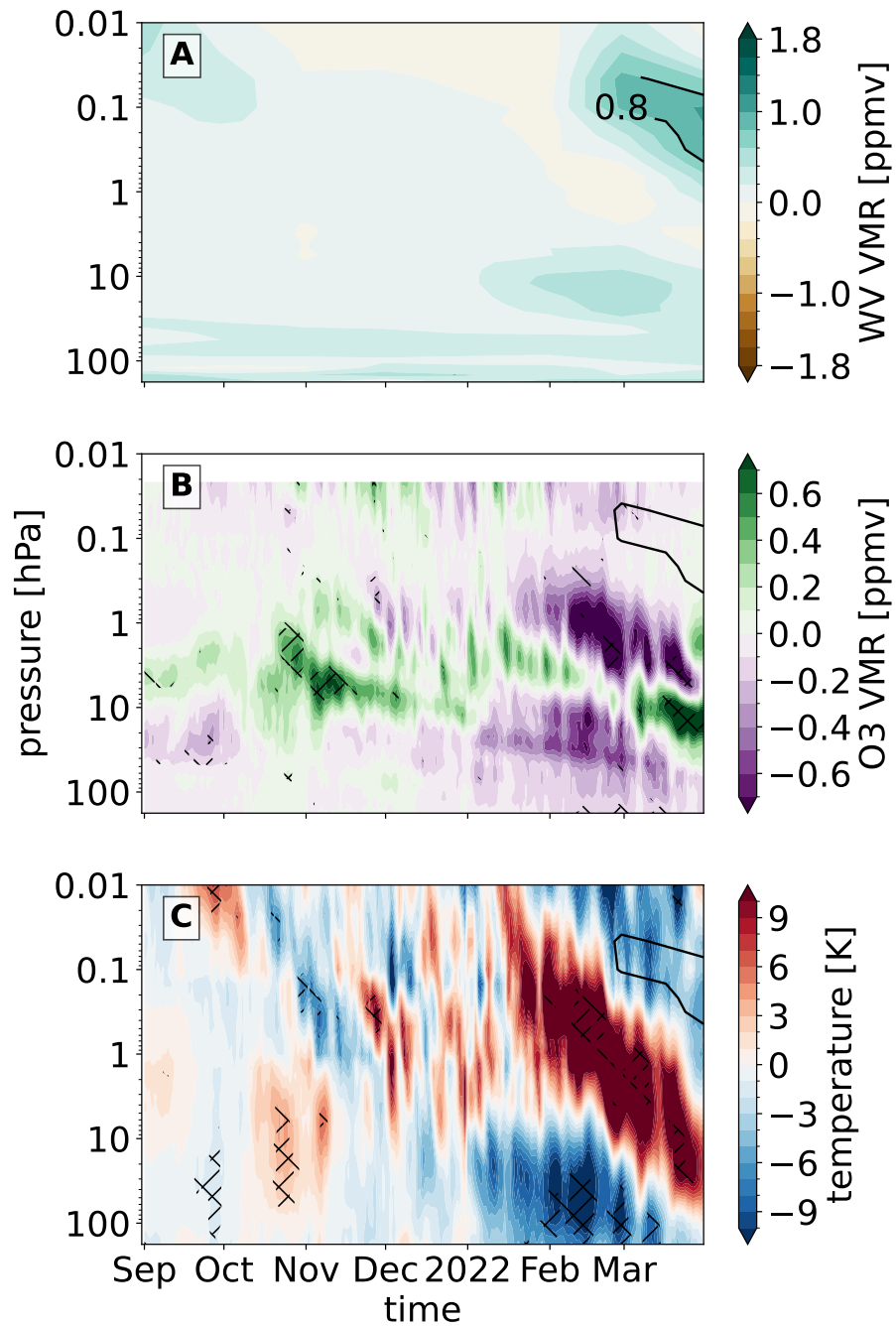
We removed "by then" accordingly.

L72-74: “The close correspondence between the WV and ozone anomalies and the diagnosed temperature response suggests  
90 that they are the primary drivers of the initial cooling observed in OW3.” This point is not clear as written. The large WV anomaly in the stratosphere (> +1 ppmv) corresponds to both a positive ozone anomaly in early winter (as discussed on L67-68), and a negative ozone anomaly in Dec-Feb. And what is the “initial cooling observed in OW3”? Is this the cooling at the beginning of the winter? This cooling is rather weak in the plots shown. There is a correlation between the negative ozone and temperature anomalies in the middle and lower stratosphere during Dec-Feb. This appears to be due to weaker than normal  
95 wave driving as shown in Figure 1B. If so, this should be clearly discussed as a dynamical process, not radiative as is currently implied (L71-74).

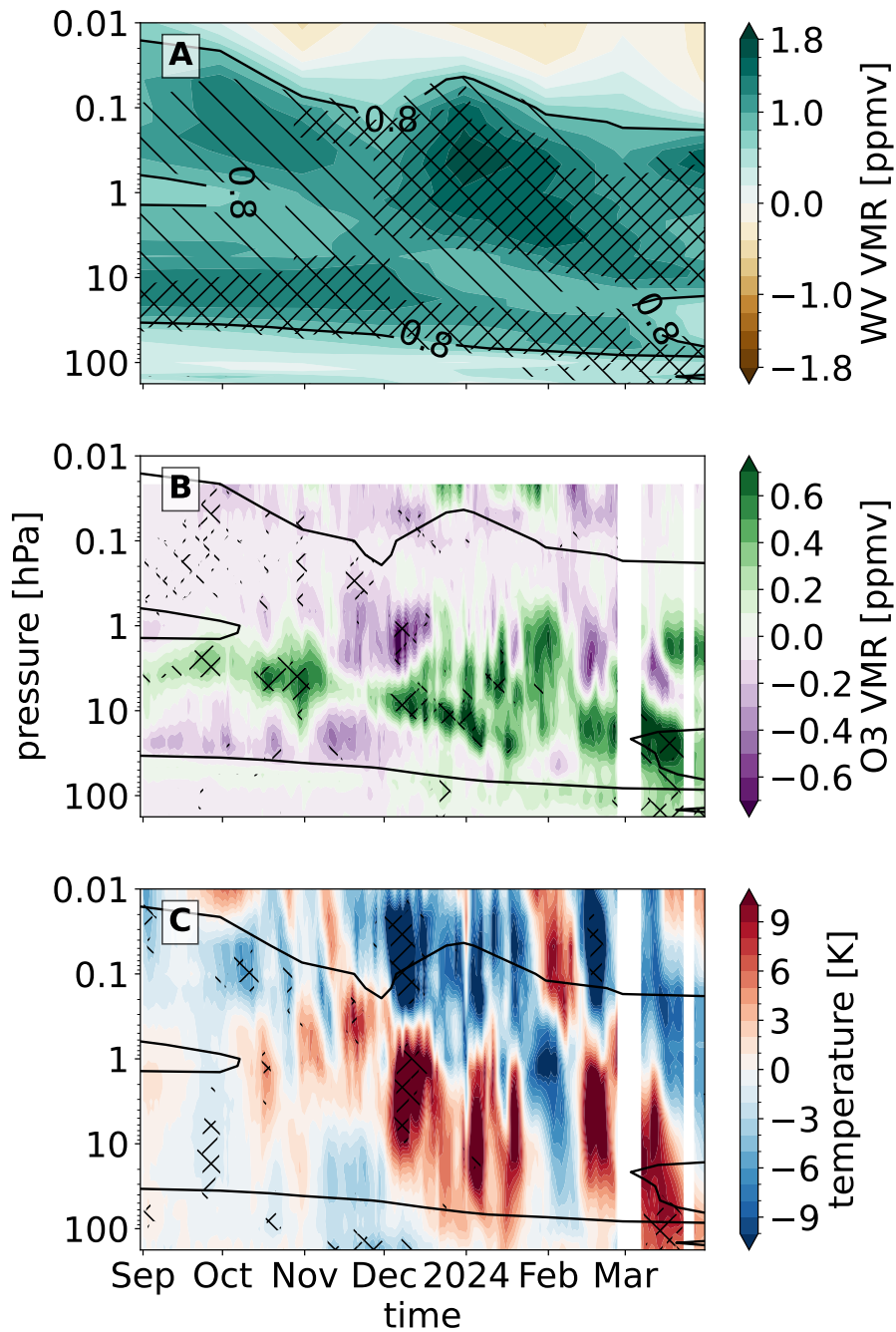
We agree and revised this statement. The observed ozone and temperature anomalies cannot by themselves be interpreted as a purely radiative response. In DJF the reduced EHF and weaker poleward ozone transport indicate a dynamical contribution. We



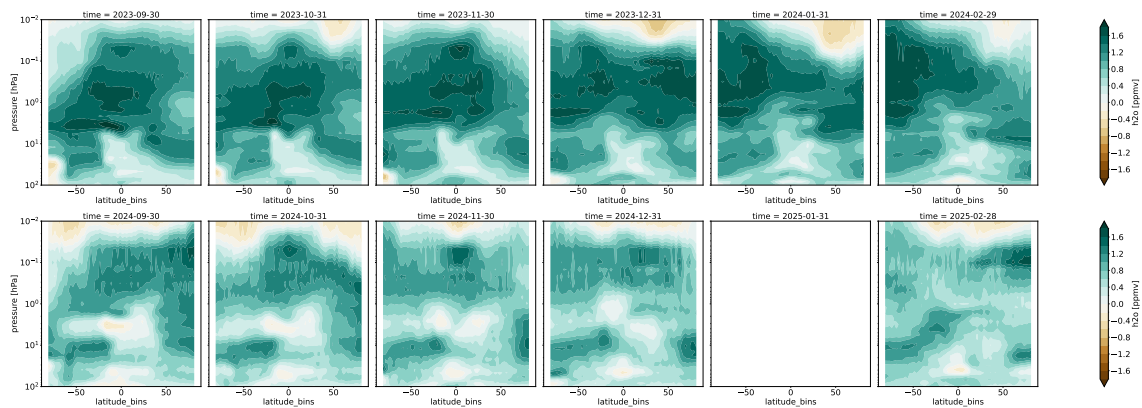
**Figure R1.** As Fig. 2 but for the extended winter 2015/2016.



**Figure R2.** As Fig. 2 but for the extended winter 2021/2022.



**Figure R3.** As Fig. 2 but for the extended winter 2023/2024 (OW2).



**Figure R4.** Water vapour anomalies for the extended winters 2023/2024 and 2024/2025 from MLS.

now describe the observed WV, ozone and temperature collocation as consistent with a radiative contribution preconditioned by dynamics and further assessed with the offline RT calculations and the SOCOL sensitivity simulations.

L76-116: While the model H<sub>2</sub>O anomaly in Figure 3 is statistically significant throughout the winter, the model ozone, temperature, and zonal wind responses are significant only in small areas (Fig.3). This would imply that the HT-induced responses are not necessarily detectable above the background variability. This is not mentioned (as far as I could tell), but should be discussed.

105 We agree and added this limitation. In SOCOLv4, the WV perturbation is robust, but the ozone, temperature, and ZW responses do not exceed ensemble variability throughout the whole winter. We therefore interpret the simulations as evidence for a forced tendency rather than deterministic grid-point attribution. The mechanism is supported by the coherent sign and vertical structure of the response and the offline RT calculations, while it remains embedded in substantial internal variability.

L82: "... we understand MW3 as the upcoming OW4." Wording is not clear. Perhaps: "we assume MW3 matches with the upcoming OW4.?" Or something to that effect?

We revised the sentence accordingly.

L86-88: should clarify that the positive ozone anomaly is only for Sept.-Nov. and over a very narrow vertical extent. And this feature is shown by a very faint green color in Figure 3B and is hard to see. It would be good to make it a darker or more visible green if possible.

115 We added the following: "...limited in Sep-Nov and rather in lower to mid stratosphere similar to MLS...". Furthermore, we modified the extent of colorbars in Figs. 2-4 and A3 so that all colors are more visible.

L88-89: should clarify that the weaker poleward transport is reflected by the EHF in Fig. 3D. But the negative ozone and temperature anomalies are mostly not statistically significant.

We modified the sentence as follows: "While negative ozone and negative temperature anomalies are not statistically significant<sup>1</sup> along the entire WV tongue similarly as in observations (see Fig. 2), they are associated with a stronger SPV and correspondingly weaker poleward transport of ozone from lower latitudes reflected by the EHF (see red line Fig. 3D and Fig. A9.)"

L94: "more muted" - suggest using "weaker" as this is more precise wording.

We changed to "weaker" as suggested.

125 L128: need to define "EPP"

We revised to "effects driven by energetic particle precipitation".

L190-191: "We report the Holton-Tan mechanism. . ." What is meant by this? Do you mean the model simulates the Holton-Tan mechanism qualitatively? Please clarify.

130 We reformulated it as follows: "We can demonstrate a link between the equatorial ZW at 10 hPa (QBO phase) and the polar vortex in SOCOLv4, i.e. so called Holton-Tan mechanism (Holton and Tan, 1980)."

Figure A1: SWOOSH appears to show a H<sub>2</sub>O anomaly of 0.5 ppmv at 10-20 hPa in early 2022, whereas MLS shows a maximum of up to 2 ppmv in the global average at this time (APARC, 2025, Fig. S3.14). This is a big difference and should be noted/discussed and/or the SWOOSH data should be checked for accuracy here. Also, the contours overlaid on the shading are difficult to interpret. Suggest putting the SWOOSH data in a second panel with shading/contours consistent with the model.

135 We modified Fig. A1 so that it allows comparison of SOCOLv4 with the MLS and SWOOSH datasets. We also updated MLS from version 5 to 6 correcting the 10–8 hPa artefact in WV (Millán et al., 2024). The artefact is still apparent in SWOOSH since it contains data from MLSv5.0. Except for this artefact, SWOOSH and MLS are comparable, especially for the main WV plume.

L209: "in consequence of" should be "as a consequence of"

140 We reformulated it accordingly.

Figure A4: This needs more explanation. It took me some time to understand that the bottom, middle, and top rows are o<sub>3</sub> only, h<sub>2</sub>o only, and o<sub>3</sub>+h<sub>2</sub>o, respectively. This needs to be explained clearly in the caption. Panels F and I are probably not necessary. I think it's better just to clearly label what's being input. And it needs to be stated that the x-axis scales are all different. This was also confusing. I assume the input o<sub>3</sub> and h<sub>2</sub>o for 2024-10-10 are held constant throughout the 200-day integration? And is integrating for 200 days needed for convergence? And why was this specific date chosen, as opposed to sometime in mid-winter? This all needs to be clearly stated/discussed.

150 Based on your and Ref. #2 comments, we replaced the original Fig. A4. with Fig. R6 since RCE does not hold in the polar regions. To diagnose the radiative heating rates (HR) due to ozone and WV perturbations as in original Fig. A4, we decided to use Konrad's interface to RRTMG. We no longer infer an equilibrium temperature response. Instead, we diagnose instantaneous longwave and shortwave heating-rate perturbations from prescribed WV and ozone anomalies using RRTMG. We leave the

---

<sup>1</sup>at respective p-values for given figure

analysis using RAE as suggested by Ref. #2 for future work. We analyse HR for both MLS and SOCOL but we replace only the MLS HR in Fig. A4. Fig. R6 and Fig. R7 show longwave cooling following the WV plume from Fig. 2 in the manuscript throughout the stratosphere and lower mesosphere, especially pronounced at the beginning of winter. Shortwave cooling and longwave warming due to negative ozone anomalies are pronounced especially above 0.1 hPa and around 1 hPa, respectively.

155 HR in SOCOLv4 are lower in magnitude since anomalies in SOCOLv4 from Fig. 3 in the manuscript reach lower magnitudes as well. These offline radiative transfer calculations reveal that perturbations in both WV and ozone abundances can induce cooling rates up to 1 K/day, with ozone and WV dominating above and below 0.1 hPa, respectively. We modified the discussion of the figure in the manuscript accordingly.

Figure A6: With the current colors, it is very hard to distinguish between the blue and green for easterly/westerly winds, especially since the dots are small. Suggest using red colors for the westerlies, or something with greater contrast for easterly/westerly. Also, the dots should be made larger. Should also mention/clarify in the caption that the dots correspond to each of the 10 ensemble members. And what time period does the 10hPa equatorial ZW correspond to? Is it the average over the winter? This should be clearly stated in the caption.

160

We revised the figure so that it is easier to distinguish between easterly and westerly winds. Furthermore, we revised the caption according to the suggestions.

165

L246-249: The sentence: “A similar configuration occurred in OW3, when the QBO was also in its westerly phase” should be checked/corrected or clarified, since OW3 as I understand it (winter 2024/2025 according to Fig. 2) shows QBO easterlies in Figure A6.

The confusion arose because Fig. A6 refers to model winters MW1-MW3, whereas the text referred to the observed OW3. We clarified this in the caption and text (see above).

170

And L250-251: It’s also unclear that “. . . the forced SPV response in MW2 arises from the HT-induced WV”. MW2 (2023/2024) has equatorial winds in the westerly QBO phase (Fig. A6) which create conditions that favor a stronger SPV as stated at the beginning of the paragraph. Perhaps both mechanisms contributed to the stronger SPV, but this needs to be clearly discussed. We added the following "compared to OW3 where perhaps both QBO and the HT forcing contributed to the stronger SPV" to differentiate between the forced response in MW2 where both (with and without the HT forcing) simulations were in westerly QBO and reality in OW3 where perhaps both QBO and the HT forcing contributed to the stronger SPV.

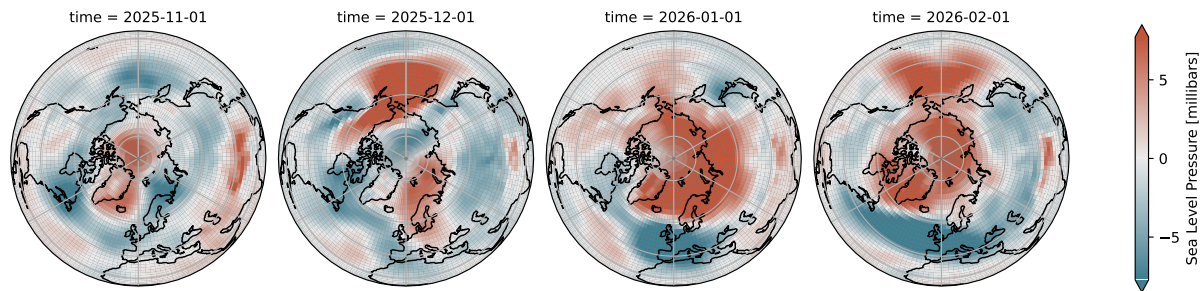
175

L252-255: It would be interesting to know if reanalysis SLP in February 2026 show a pattern similar to the model in Figure A8.

Fig. R5 shows monthly anomalies of SLP for the extended winter 2025–2026. Interestingly, January and February reveal similar patterns as in Fig. A8 for SOCOLv4. While these negative AO-like patterns likely emerged for different reasons, rather than reconditioning SSW at the beginning of March, the predicted stratospheric conditions from our chemistry-climate model simulations are quite striking and would require further investigation to understand the underlying mechanism.

180

Figure A8: To be clear, suggest adding “SOCOLv4” at the beginning of the caption right before “Monthly anomaly”. We decided to keep the description as is so that it is compatible with other figures.



**Figure R5.** Monthly anomaly of sea level pressure (SLP in hPa) for the extended winter 2025–2026 (OW4) from the NCEP/NCAR reanalysis. Anomalies are expressed as differences with respect to the climatology.

185 **Reply to Ref #2**

Dear anonymous reviewer #2,

We appreciate your very helpful comments! We revised the manuscript according to your suggestions and hope the revised version obtains your approval for publication in ACP. Please find our point-by-point answers to the comments below.

190 Best wishes

Ales Kuchar et al

The paper poses the question of whether the Hunga Tonga eruption led to a strengthening of the Northern hemisphere polar vortex in winter 2024/2025. They argue that water vapor injected from the Hunga Tonga eruption makes its way into the polar vortex and starts a chain of processes that lead to a stronger polar vortex. They make use of satellite, reanalysis and model (SOCOLv4) datasets to support their mechanism. Whilst this paper is certainly addressing an important and timely problem and the topic could make a good contribution to the literature, there are some major methodological flaws that we would like to see addressed.

**Major comments:**

200 - Line 65 and Figure 2: There needs to be more evidence presented to link the water vapor anomaly to the ozone and temperature anomalies. We are not convinced that the ozone and temperature anomalies are actually collocated with the water vapor. Are these ozone and T anomalies large compared to the usual climate anomalies during strong vortex years?

See our response to comments of Ref. #1 above discussing Figs. R1 and R2. A systematic comparison with all strong-vortex winters would be valuable but is beyond the scope of this Letter. We now avoid wording discussing not statistically significant

205 (added as suggested by Ref. #1) temperature and ozone anomalies.

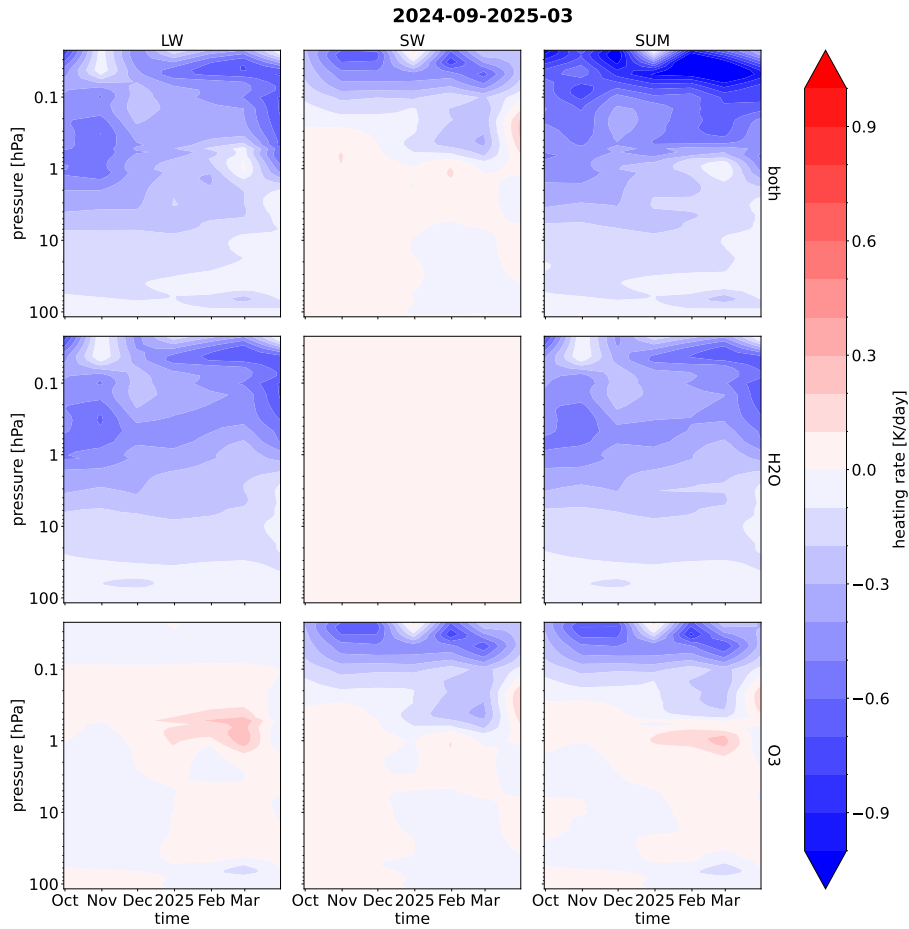
- Radiative transfer calculations: Radiative–convective equilibrium (which is valid in the tropics) does not hold in the polar regions and is the correct calculation to use to diagnose the radiative heating rates. The relevant balance is radiative–advective equilibrium (Caballero and Merlis, 2025; Cronin and Jansen, 2016).

210 We agree with the reviewer that RCE does not hold in the polar regions. To diagnose the radiative heating rates (HR) due to ozone and WV perturbations as in original Fig. A4, we decided to use Konrad’s interface to RRTMG. We no longer infer an equilibrium temperature response. Instead, we diagnose instantaneous longwave and shortwave heating-rate perturbations from prescribed WV and ozone anomalies using RRTMG. We leave the analysis using RAE for future work. We analyse HR for both MLS and SOCOL but we replace only the MLS HR in Fig. A4. Figs. R6 and R7 show longwave cooling following the WV plume from Fig. 2 in the manuscript throughout the stratosphere and lower mesosphere, especially pronounced at the beginning of winter. Shortwave cooling and longwave warming due to negative ozone anomalies are pronounced especially above 0.1 hPa and around 1 hPa, respectively. HR in SOCOLv4 are lower in magnitude since anomalies in SOCOLv4 from Fig. 3 in the manuscript reach lower magnitudes as well. These offline radiative transfer calculations reveal that perturbations in both WV and ozone abundances can induce cooling rates up to 1 K/day, with ozone and WV dominating above and below 0.1 hPa, respectively. We modified the discussion of the figure in the manuscript accordingly.

220 - Mechanistic explanation: the paper presents a variety of coincident observed & simulated variables to demonstrate influence on the SPV from WV anomalies. The mechanistic explanation is plausible but at present the paper does not clearly show a causal link. The response in simulation runs with a strong WV anomaly should be compared with runs with a weak WV anomaly to diagnose a different response. Other strong SPV years should be considered; is the response distinct from these years as a result of the increased WV?

225 We agree that the original wording implied a stronger causal attribution than can be supported from collocation alone. We revised the manuscript to frame the mechanism as a forced tendency rather than deterministic attribution of the observed winter. Specifically, we now combine three lines of evidence: (i) empirical significance estimates for MLS WV, ozone, and temperature anomalies, showing robust WV anomalies but only locally significant ozone and temperature anomalies; (ii) comparison with OW2 and with previous strong-vortex winters (see discussion of Figs. R1 and R2 above), showing that similar ozone/temperature anomalies can occur dynamically without a comparable HT-related but still collocated with an enhanced WV signal; and (iii) paired SOCOLv4 simulations with and without HT forcing, where the stronger WV perturbation in MW2 is accompanied by clearer radiative cooling and a stronger-westerly tendency, while the weaker WV perturbation in MW3 yields a weaker, more variability-dominated response. We therefore avoid claiming a deterministic causal chain and instead conclude that HT-related WV anomalies plausibly preconditioned the vortex under favourable dynamical conditions throughout the manuscript and especially in the discussion section.

235 - Line 129: “The key elements are the slow Brewer-Dobson descent of the HT WV anomaly, enhanced HOx and ClOx chemistry leading to . . . ozone loss” It is not clear to us where these statements are drawn from. It has not been clearly demonstrated that 1) the descent of the WV anomaly is slow and 2) HOx and ClOx chemistry are responsible for the ozone loss. Which months and at what height levels are these chemical cycles causing ozone loss? Is the heterogeneous chemistry adequately capturing

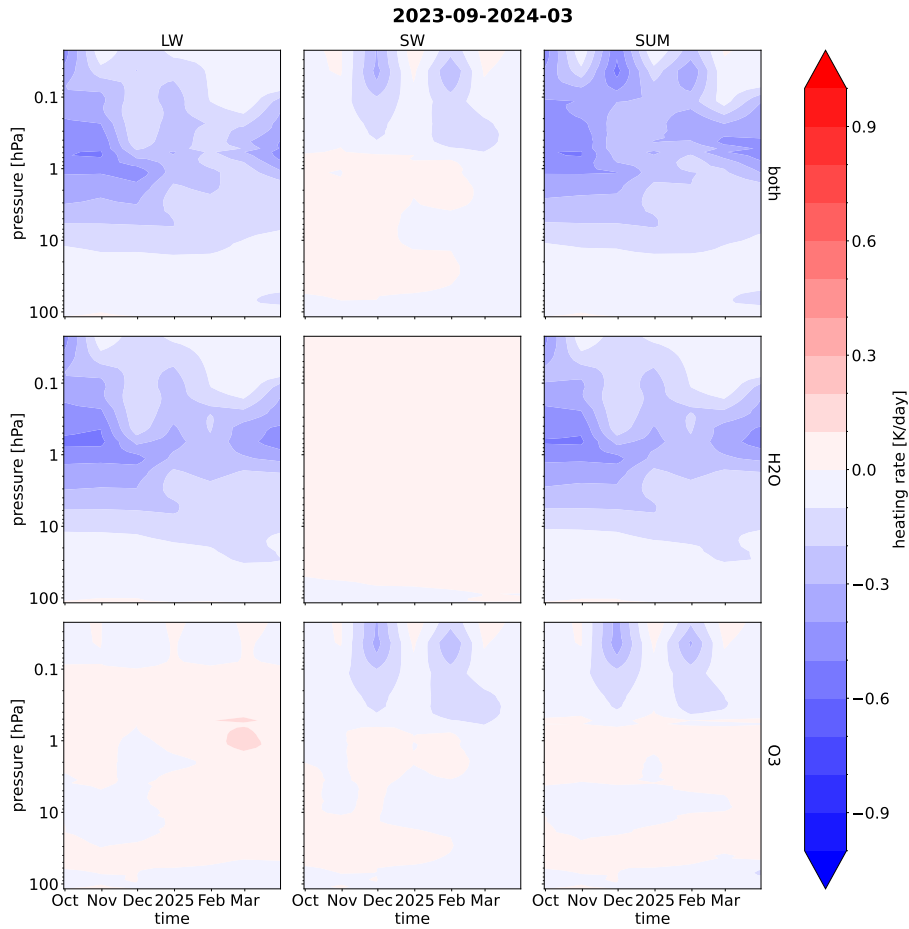


**Figure R6.** RRTMG radiative heating rates (longwave, shortwave and their sum) for prescribed weighted zonally-average over  $60^{\circ}$ – $90^{\circ}$ N of water vapour and ozone monthly anomalies from MLS for the extended winter 2024/2025 (OW3).

240 these processes in the SOCOL model? Can you quantify how “slow” the WV anomaly descent is? A slower Brewer-Dobson  
circulation relative to climatology would be contradicted by the more rapid dissipation of the WV anomaly in the simulations.  
We added Fig. R8 in the manuscript showing anomalously older age of air (green shading) corresponding to the water vapour  
descent from the lower mesosphere. While these anomalies reveal stat. significance only at the p-values of the t-test < 0.3,  
anomalies related to younger age of air (pink shading) in the lower mesosphere reveal higher levels of stat. significance. We  
245 attribute these negative anomalies to increasing mixing since observed also in lower latitudes.

HO<sub>x</sub>, NO<sub>x</sub> and ClO<sub>x</sub> chemistry is shown in Fig. A2 and discussed in our opinion appropriately — taking into account the letter  
format of the manuscript.

We also added the following statement to Section A2 in the manuscript: "SOCOLv4 is adequate for broad NO<sub>x</sub>/HO<sub>x</sub>/ClO<sub>x</sub>



**Figure R7.** RRTMG radiative heating rates (longwave, shortwave and their sum) for prescribed weighted zonally-average over  $60^{\circ}$ – $90^{\circ}$ N of water vapour and ozone monthly anomalies from SOCOLv4 for the extended winter 2023/2024 (MW2).

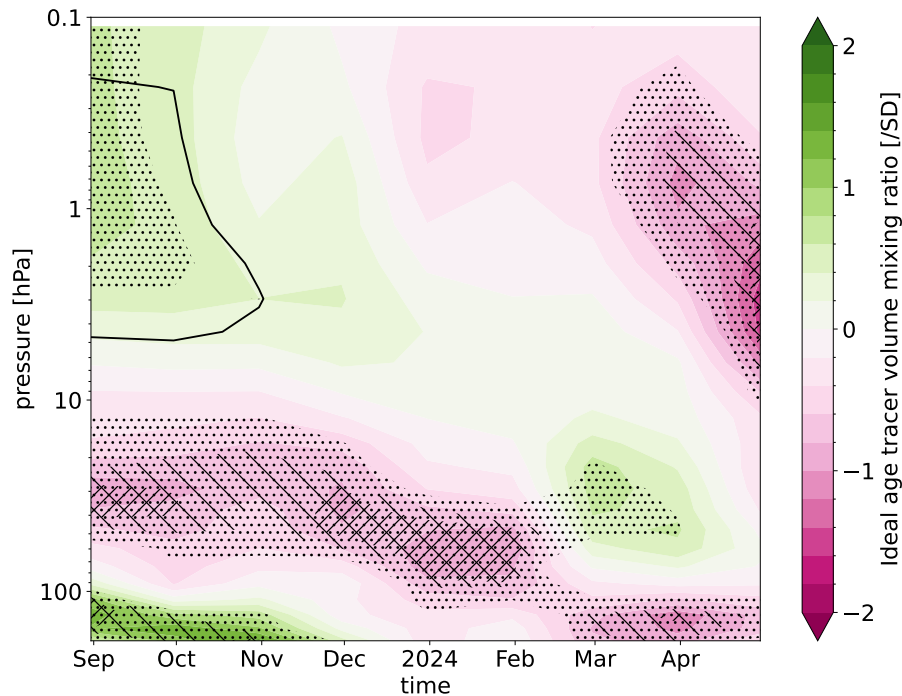
250 -driven ozone depletion/recovery studies (Sukhodolov et al., 2021). The main remaining weaknesses are transport and mixing, photolysis at large solar zenith angles over the high latitudes southern hemisphere (e.g. Imanova et al., 2025)".

### Minor comments

:

255 - Line 93: "These temperature anomalies are comparable with those . . . simulated" is contradicted by the following sentence and not reflected in figure 3C. The colourbar in 3C has too many levels to be easily interpreted, but it looks like a minimum temperature anomaly around -5K.

We modified this paragraph according your comment about RCE and RAE, respectively. We changed the colorbar maxima in Figs. 2C, 3C, 4C and A3C so that they are easily comparable with Fig. A4.



**Figure R8.** Weighted zonally-average over  $60^{\circ}$ – $90^{\circ}$ N of age of air estimated as idealized age tracer volume mixing ratio monthly anomalies for the extended winter 2023/2024 (MW2). Anomalies are expressed as the difference between the SOCOLv4 simulation with and without the HT forcing and normalized to its climatological standard deviation (SD). Dots ... and hatching  $\backslash\backslash\backslash$  and  $////$  show where the p-values of the t-test are  $< 0.3$  and  $< 0.1$  and  $< 0.05$ , respectively. The black contour line represents the contour of 1 ppmv of water vapour anomaly.

- Line 190: What exactly do you mean by “We report the Holton-Tan mechanism (Holton and Tan, 1980)”? Do you mean that you can demonstrate a strong link between the QBO phase and the strength of the polar vortex in your model?

260 We reformulated it as follows: "We can demonstrate a link between the equatorial ZW at 10 hPa (QBO phase) and the polar vortex in SOCOLv4, i.e. so called Holton-Tan mechanism (Holton and Tan, 1980)."

## References

- Caballero, R. and Merlis, T. M.: Polar Feedbacks in Clear-Sky Radiative–Advective Equilibrium from an Airmass Transformation Perspective, *Journal of Climate*, 38, 3399 – 3416, <https://doi.org/10.1175/JCLI-D-24-0031.1>, 2025.
- 265 Cronin, T. W. and Jansen, M. F.: Analytic radiative-advective equilibrium as a model for high-latitude climate, *Geophysical Research Letters*, 43, 449–457, <https://doi.org/https://doi.org/10.1002/2015GL067172>, 2016.
- Holton, J. R. and Tan, H.-C.: The influence of the equatorial quasi-biennial oscillation on the global circulation at 50 mb, *Journal of Atmospheric Sciences*, 37, 2200–2208, 1980.
- Imanova, A., Egorova, T., Zubov, V., Mironov, A., Polyakov, A., Nerobelov, G., and Rozanov, E.: Seasonal Cycle of the Total Ozone Content  
270 over Southern High Latitudes in the CCM SOCOLv3, *Atmosphere*, 16, <https://doi.org/10.3390/atmos16101172>, 2025.
- Millán, L., Read, W. G., Santee, M. L., Lambert, A., Manney, G. L., Neu, J. L., Pitts, M. C., Werner, F., Livesey, N. J., and Schwartz, M. J.: The Evolution of the Hunga Hydration in a Moistening Stratosphere, *Geophysical Research Letters*, 51, e2024GL110841, <https://doi.org/https://doi.org/10.1029/2024GL110841>, e2024GL110841 2024GL110841, 2024.
- Sukhodolov, T., Egorova, T., Stenke, A., Ball, W. T., Brodowsky, C., Chiodo, G., Feinberg, A., Friedel, M., Karagodin-Doyennel, A., Peter, T., Sedlacek, J., Vattioni, S., and Rozanov, E.: Atmosphere–ocean–aerosol–chemistry–climate model SOCOLv4.0: description and  
275 evaluation, *Geoscientific Model Development*, 14, 5525–5560, <https://doi.org/10.5194/gmd-14-5525-2021>, 2021.
- Zhou, X., Dhomse, S. S., Feng, W., Mann, G., Heddell, S., Pumphrey, H., Kerridge, B. J., Latter, B., Siddans, R., Ventress, L., Querel, R., Smale, P., Asher, E., Hall, E. G., Bekki, S., and Chipperfield, M. P.: Antarctic Vortex Dehydration in 2023 as a  
280 Substantial Removal Pathway for Hunga Tonga-Hunga Ha’apai Water Vapor, *Geophysical Research Letters*, 51, e2023GL107630, <https://doi.org/https://doi.org/10.1029/2023GL107630>, e2023GL107630 2023GL107630, 2024.