Replies to reviews for: Modulation of the Northern polar vortex by the Hunga Tonga-Hunga Ha'apai eruption and associated surface response

ACP, Ales Kuchar et al.

1 Reply to reviews

Dear editor,

thank you very much for helping in the review process of this paper. We appreciate the helpful comments of the both reviewers. We revised the manuscript accordingly, such that we hope it can then be published in ACP. Please find our point-by-point answers (in blue) to the comments (in black) below.

Best wishes

Ales Kuchar et al

2 Ref #1

Dear anonymous reviewer #1,

thank you very much for your valuable comments. Please find our point-by-point response (in blue) to your comments below.

2.1 General comments

The paper presents some interesting results, as a possible stratosphere-troposphere coupling mechanism and subsequent surface climate response indirectly caused by the HT stratospheric water vapor injection is of considerable interest. This is primarily a model study, as comparisons with observations are limited to the HT forcing agents, H2O and aerosols. The paper is generally well written and structured but should have additional significant clarification and/or explanation in certain places (although not quite at the "major revisions" level). These and a few other specific concerns are detailed below and should be addressed prior to publication.

The authors compare the model with observations of the HT H2O and aerosols in the Appendix. But what about comparison with observations of the model responses, e.g., temperature and ozone? For example, do the features seen in Figure 1 (for temperature and ozone) and Figure A4 (HNO3) show up in de-seasonalized MLS data? Comparing the model with vs. without the anomaly against de-seasonalized data can be difficult given the background variability, but do the model features show up at least qualitatively in the observations? The authors should discuss this. This is especially important for the downward propagation of the signal (Figure 2B) and the surface responses. The downward propagation seems to be robust in the model stratosphere, but does it show up in observations or reanalysis, at least qualitatively? And are the surface responses detectable in reanalysis data? There is only a brief mention of comparisons with stratospheric/mesospheric observations documented in other studies (L65-66). If these features are difficult to ascertain from observations due to atmospheric variability, this should be made clear. The authors seem to allude to this difficulty somewhat (L112-113), but this should be clarified.

To compare SOCOLv4 with observations, we added Fig. A4 showing global evolution of stratospheric and lower-mesospheric temperature, water vapour and ozone similarly to Figs. 1, 2 and 3 in Randel et al. (2024). This figure indicates a slight deficiency of our model as the anomalous water dissipates faster as seen in observations (e.g. MLS) or occuring in other models (e.g. WACCM in Randel et al., 2024; Wang et al., 2023). In addition, our experiment protocol is different to the WACCM simulations, which were either nudged to reanalysis or initialized from the observed sea-surface temperatures, while the SOCOLv4 simulations presented here were performed with both atmosphere (except QBO) and ocean start from a random state. We highlight this aspect in the method section and now refer to the new A4 figure in the text.

The discussion of the downward propagation is extended using Fig. A8 (see our response to your comment below). Note that in the observations, the winter of 2022/2023 was characterized by three stratospheric disturbances between December an February, a weakening of the vortex, a minor SSW and a major SSW, which were accompanied with surface weather anomalies across the NH (Lu et al., 2023). However, we abstain from making the direct comparison with reanalysis, because our modelling set-up was fully free-running (both atmosphere and ocean) and thus, our ocean state at the month of the event differs from observations (as the ensemble members are initialized several years prior to 2022). The central dates of SSWs as well as their surface effects depend on many dynamical factors such as the tropospheric basic state and planetary wave forcing (e.g. Karpechko et al., 2017), which differ in our experiments from observations of the specific case of 2022/2023. We prefer to use the ensemble mean perspective for our analysis, to show that the HT effects would create favorable conditions for the occurrence of SSWs and their downward propagation, but abstain from claiming that any specific SSW of the last two winter was solely due to the HT event. For that, a more detailed attribution study would be needed, which is outside of the scope of this paper.

I also found the statement on L40, “validate these simulations with observational data” to be somewhat misleading, as the model is evaluated/compared with observations in the Appendix only for H2O and aerosol (forcers), but not the model responses in temperature, ozone, etc. This statement should be qualified, or observational comparisons of the model responses should be included.

We modified the statement to "validate these simulations with observational data for H_2O and aerosol and discuss other variables using available studies (see Section A1 in Appendix).". As mentioned above, we expanded discussion in Section A1 using Fig. A4 and its direct comparison with MLS and WACCM in Randel et al. (2024).

While there is a statistically significant signal in SLP and (a small area) in surface temperature (Figure 3), it's not clear if the signal is really propagating down from the stratosphere through the troposphere. There is a vague hint of this in Figure 2, but this appears to be mostly not statistically significant in the troposphere. Do maps of, e.g., geopotential height anomalies, show significant signals in the mid and upper troposphere similar to SLP? If so, it would be important and compelling to show maps like Figure 3A, for example, at 500, 300, and/or 200 hPa. If the responses in the mid-upper troposphere are not similar and/or not significant, the authors should provide a more detailed and clear explanation as to why the surface response is still expected to be part of the "dynamical stratosphere-troposphere-surface coupling in the NH following the eruption" as stated on L97-98, and what the mechanisms are that drive the surface response. For example, is this an indication of a surface amplification of the stratospheric signal as discussed in previous studies (e.g., Baldwin et al., Rev of Geophys., 2021; Domeisen et al., JGR, 2020).

As mentioned below, we updated Fig. 2C which is obviously crucial for discussing whether the signal propagates down from the stratosphere through the troposphere. It allows us to be fully transparent about p-values and highlights the signal propagation.

Furthermore, we added Fig. A8 showing anomalies of geopotential height at 10, 50, 100, 200 and 500 hPa. The geopotential height response reveals anomalies supporting the statement that the signal is indeed propagating down from the stratosphere through the troposphere to the surface, where we can identify the negative NAO response (see Fig. 3**A**).

2.2 Specific comments

L16 -17: "Massive" is appropriate for the amount of H2O injected. But for SO2, suggest changing to "modest amount of SO2", or something to that effect. 0.4 Tg is is not "massive" compared to Pinatubo, and "massive SO2" also contradicts "lower emissions of SO2" stated on L27.

We revised the sentence as follows: "It released massive amounts of water vapor (WV) and modest amount of sulfur dioxide (SO_2) into the stratosphere, far exceeding previous records."

L19: "have been marked"? It's not clear what "marked" means here. Have been "studied" or "documented"? Please clarify.

We deleted "marked" as ambiguous.

L25: Note that the statistical significance of "surface warming over Eurasia" following major volcanic eruptions has been challenged recently (eg, DallaSanta and Polvani, ACP, 2022). Suggest tempering this statement.

We tempered the statement and added the reference.

L57-59: Does radiative cooling by the H2O anomaly drive any of the temperature response around the stratopause, or is it all due to the reduction in ozone heating? If H2O cooling is (or is not) a factor here, should briefly state this to clarify.

Unfortunately we do not have the radiative-rate outputs available. Nevertheless, we conclude that anomalies in temperature at these pressure levels emerge mainly due to reduced absorption of ultraviolet radiation by ozone (see Fig. 4.24 in Brasseur and Solomon (2005)) as also similarly reported by recent modeling studies (Fleming et al., 2024; Randel et al., 2024).Thus, we added "mainly" to the sentence.

L66-67: “no significant mesospheric temperature anomaly is found. . . ” Suggest being more specific here, e.g., change to: ". . . .no significant persistent mesospheric temperature anomaly. . . ." since there’s a brief significant cold anomaly in March in Fig, 2B.

We added "persistent" as you suggested.

L75: “we observe” should be changed to “we calculate” or something to that effect. Fig. 1S-T shows model calculations, not observations.

We replaced "observe" with "detect".

L77: Should define "NAM" here since this is the first time it’s used in the main text, and point to section A3. Also, it would be very helpful here to give a brief description of what the positive/negative NAM is in geophysical terms, e.g., stronger/weaker zonal jet, SLP and temperature changes, etc.

We defined the abbreviation there and pointed to Section A3. Furthermore, we added the following sentence: "Positive and negative NAM values correspond to strong and weak PV events, respectively, with different thresholds used for the SSW identification (Baldwin and Dunkerton, 2001; Gerber and Polvani, 2009; Jucker, 2016).".

L85-86: The negative anomalies close to the surface in spring in Figure 2C are not statistically significant. This at least should be mentioned. Also, the anomalies appear to extend only through April 2023 in Figure 2C; May is not included. Either the text should be modified accordingly, or the plot should be extended to include May.

While negative NAM anomalies close to the surface in March and April are not statistically significant at 2σ, they reveal significance at 1σ. To be fully transparent about p-values, we integrate both thresholds in Fig. 2C by masking out all non-significant NAM values at 1σ. This further highlights that the signal likely propagates from the stratosphere through the troposphere (see also new Fig. A8 and accompanying discussion). As seen in Fig. A8, the zonal mean tropospheric signal looses the significance due to the appearance of two signals of different signs at similar latitudes.

We appologize. The original submission did incorrectly state May instead of April. We modified this typo in the revised manuscript accordingly.

L89-90: "...negative modulation of the stratospheric PV..." I assume this means a weakened polar vortex as is stated in the next sentence. If so, suggest clarifying to read something to the effect of: "This pattern is characteristic of a weaker stratospheric PV associated ..." Also, the cited Kidston et al., 2015 reference (their Box 1) states that there is “a net poleward shift of the tropospheric jet… when the stratospheric winds are strong and westerly." However, the statement on L89-90: ".... negative modulation of the stratospheric PV associated with a poleward shift of the tropospheric jet stream." seems to contradict this. This should be checked and/or clarified.

We revised the sentence as suggested above and corrected the formulation about the equatorward shift of the tropospheric jet stream in agreement with Kidston et al. (2015).

L154 - should state "return to the troposphere" somewhere here when discussing "… transport to higher latitudes .... via the Brewer-Dobson circulation (BDC)." The excess H2O returning to the troposphere and subsequent rainout is the actual removal process (along with PSC sedimentation as is stated).

We revised the sentence as follows: "The excess of stratospheric WV returns to the troposphere by sedimentation of PSCs within the SH PV, and is transported to higher latitudes of both hemispheres via the Brewer-Dobson circulation (BDC)".

As a suggestion following from L43-44, "an outlook of how these dynamically-induced events could be further explored" (and L123-125): It would be of interest to mention that examining the response in the Southern hemisphere could be investigated in the future, especially since the upper stratospheric cooling is also significant in the SH lower latitudes in Figure 1P-T. It would also be interesting to examine if/how a possible future stratospheric response (Figure 2) could be impacted by the phase of the QBO.

We added the following sentence: "Given the interhemispheric extent of cooling in the upper stratosphere and lower mesosphere, which could similarly affect the persistence of PV in the SH, future studies could explore the PV response in the SH and its coupling with the troposphere. Furthermore, the stratospheric response could be impacted by the phase of the quasi-biennial oscillation as recently suggested by Jucker et al. (2024)".

2.3 Technical corrections

Captions for Figures 1,2, and 3: When mentioning "FDR correction", suggest pointing to section A2, since "FDR" is not discussed/defined in the main text.

Thank you for the suggestion. We added "(see Section A2)".

Figure 2: It would be very helpful to indicate the latitude ranges at the top of each panel.

The figure has been revised as suggested.

Figure 2C. Suggest reversing (or changing somehow) the colors to be consistent with panels A and B. Red colors are positive anomalies in 2A-B but are negative anomalies in Fig. 2C. This was somewhat confusing, and I had to frequently look at the color legend to be reminded of the differing color schemes.

We thank the reviewer for this comment, however we wish to stay with this colour scheme as our motivation is that (warm) colors align with the warming in the polar stratosphere as suggested by the negative NAM values. This is frequently adopted as visualization practice such as e.g. in the seminal work of (Baldwin and Dunkerton, 2001).

L75: Should this be "(see Fig. 1I-J"?

Thank you. We agree.

L89: Change “characteristic for” to ”characteristic of”

Revided as suggested.

L108: To clarify, suggest inserting “negative” before “temperature response”

Thank you for the suggestion. We revised the statement accordingly.

Figure A1: should state that this is a global average.

Thank you for the suggestion. We added "Globally average".

Figure A3 caption, first word: Should be “Seasonal”.

Thank you for the correction".

3 Ref #2

Dear anonymous reviewer #2,
thank you very much for your valuable comments. Please find our point-by-point response (in blue) to your comments below.

3.1 General comments

The study authored by Kuchar et al. offers an analysis of the impacts of the water vapor and sulfur dioxide injections from the Hunga Tonga-Hunga Ha’apai eruption into the stratosphere and mesosphere using simulations with the Earth System Model SOCOLv4. They compare an ensemble of 10 simulations without the volcanic forcing to 10 simulations with the volcanic forcing to identify anomalies. They conclude that the enhanced water vapor resulted in significant dynamical responses including a weakening of the Northern Hemisphere polar vortex which is a consequence of the decreased temperature gradient. They also assert that is signal propagates downwards to drive surface-level temperature and pressure anomalies. Providing a mechanism by which volcanic eruptions influence stratospheric dynamics and ultimately the surface is a scientifically relevant complement to the radiative analyses.

Given that the study is primarily an analysis of model output, however, a more rigorous validation of the simulations' recreation of the atmospheric response to the HT eruption is critical. Specifically, while the appendix compares the water vapor and sulfate aerosol anomalies from the model to observation, and I agree that there is large agreement in the general shape and evolution of the anomalies, how might the differences in, for example, spatial extent of the anomalies impact the conclusions? Similarly, to fully explore the performance of the model, the predictions should also be compared to observations. For example, how well do the temperature anomalies match?

Overall, the paper is exceptionally well-written, and no improvements to the presentation are necessary. Additional context on the model performance, and a brief discussion of any limitations associated with that performance, however, are needed.

We thank the reviewer for the overall positive assessment and provide additional comparison with observations, as requested, in the revised manuscript. We added Fig. A4 showing the global evolution of stratospheric and lower-mesospheric temperature, water vapour and ozone similarly to Figs. 1, 2 and 3 in Randel et al. (2024).This figure indicates a slight deficiency of our model as the anomalous water dissipates faster as seen in observations (e.g. MLS) or occuring in other models (e.g. WACCM in Randel et al., 2024; Wang et al., 2023). In addition, our model set up is different to the WACCM simulations, which were either nudged to reanalysis or initialized from the observed sea-surface temperatures, while the SOCOLv4 simulations presented here were performed with both atmosphere (except QBO) and ocean start from a random state. We highlight this aspect in the method section and now refer to the new A4 figure in the text. Both models, however, and the MLS retrievals indicate a stratopause cooling at the beginning of 2023. Given that this cooling is the starting point for the further dynamical changes, analyzed in our paper, we conclude that our simulations, even with the highlighted faster vertical transport in year 1, are a good basis for the analysis in year 2.

3.2 Specific comments

Line 19: "marked" is ambiguous, in what direction?

We deleted "marked".

Lines 30-31 and 52-53: It's likely worthwhile to also reference the work investigating the contribution of heterogeneous reactions to the HT impacts (e.g. Santee et al., 2023; Evan et al., 2023)

Thank you. We added both references.

NAM acronym is only defined in the Figure 2 caption

Thank you, we ensured to introduce the NAM in the running text of the revised manuscript.

Lines 57-59: do the water vapor anomalies contribute radiatively here?

Unfortunately we do not have radiative-rate outputs available. Nevertheless, we conclude that anomalies in temperature at these pressure levels emerge mainly due to reduced absorption of ultraviolet radiation by ozone (see Fig. 4.24 in Brasseur and

Solomon, 2005) as also similarly reported by recent modeling studies (Fleming et al., 2024; Randel et al., 2024). Thus, we added "mainly" and these two references within the sentence.

3.3 Technical corrections

Figure 1: the top and bottom rows are slightly offset

We align the rows in Fig. 1 at the cost of adding white spaces among the rows. We hope to find a final solution before the publishing.

References

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