Detailed reply to the referee remarks on "Radiative forcing and stratospheric ozone changes due to major forest fires and recent volcanic eruptions including Hunga Tonga" (second round)

In the following reviewer remarks are in bold and answers in normal font.

I would like to thank the authors for addressing my previous comments. I think the paper has made improvements in readability and understanding of the science being done. I appreciate the inclusion of a Table describing the simulations which increases understanding significantly. A few major comments remain below that I feel where not adequately addressed in my previous review. I have added further specific comments that also need to be addressed before publication.

Major comments

Please include legends in all line figures following ACP author guidelines. It is cumbersome to have to go back to Figure 4 or the Table to see what line is for each Figure. Axis labels should also be included. The figures are still hard to read without this information readily available for each figure.

Legends and additional short labels are now included in Figs. 1-7 and A1-A5 and the captions shortened.

Regarding my previous comment on HCl, the author's response states: "The time of the HCl recovery depends on size and depth of the ozone hole (Grooß et al., 2011). If local ozone is close to zero HCl recovers quickly, if some is left, e.g. due to advection, recovery takes longer (see Fig. 7 of preprint). Model uncertainties on this are largest near the vortex edge. Text modified." This does not explain why your simulation disagrees with observations. At 72S, in both your control run and fire runs, HCl recovers a month later than MLS from 2020-2022. Therefore, in your simulations, you have enhanced ClO occurring during a time when HCl in MLS is almost completely recovered. If HCl in your simulations recovered at the same time as MLS, would you still expect to see significant ozone depletion in September all the way to the pole, as your simulations suggest in Figure 8? That seems unlikely to me and therefore your ozone loss results may be overestimated. Please check.

Text expanded. In EMAC due to some ozone left from numerical diffusion chlorine deactivation occurs also via $ClONO_2$ formation, delaying the increase of HCl. The delay in HCl recovery of model compared to MLS is also present in Fig. 4 of Solomon et al. (2023).

Specific Comments

Line 10. "The sunlight-absorbing aerosol from the Australian and

Canadian forest fire emissions in 2019/2020 induced the most significant perturbation in stratospheric optical depth since the major eruption of Pinatubo in 1991. It shifted the sign of instantaneous stratospheric aerosol forcing, derived at the top of the atmosphere, from -0.2 Wm-2 to +0.3 Wm-2 in January 2020." Did the Canadian fires really have that large of an effect here? It seems largely insignificant compared to the Australian fires? Please reword.

"to a smaller extent the" inserted before "Canadian". 2017 is now included in most of the figures, more results on the Canadian fires are included in section 3 and the conclusions.

Line 25 "Because of the absorbing aerosol the radiative impact of forest fire plumes differs from the one of major volcanic eruptions like for example Hunga Tonga in January 2022" Hunga was not like a typical major volcanic eruption though?

Added at end of sentence "or Raikoke in 2019 where scattering aerosol dominates"

Line 33: "Hunga Tonga, short Hunga (Santee et al., 2024), the most explosive eruption in the last 3 decades,". This is a little disjointed. Please reword.

Modified to "Hunga Tonga (now shortly referred to as Hunga (Santee et al., 2024)) ..."

Line 125: "Considering the organic aerosol from major forest fires is essential for agreement with the observations by OSIRIS and OMPS-LP" Suggest change "considering" to "including".

O.K.

Line 127: "0.0005 as can be seen by the black," Can this really be seen in the black curve? Or just the model simulations?

Sentence rearranged for clarification.

Line 130-132: "In the tropics the eruptions of Ulawun in June and August 2019 and of St. Vincent in April 2021 have also a large impact on SAOD while the effect of Australian fires with up to 0.0013 is relatively small." Please fix grammar in this sentence. Something like: "In the tropics, the eruptions of Ulawun in June and August 2019 and of St. Vincent in April 2021 also have a large impact on SAOD, while the effect of the Australian fires is relatively small, only up to 0.0013." You mention the ANY enhancement of .0013, but not the Volcanoes even though they are the major cause of the increase.

Changed as suggested, typical values inserted.

Line 135-139: "During northern summer 2020 OSIRIS data were so sparse that OMPS-LP had to be used to estimate SO2 injections of some volcanic events and it looks like that the used factors "f" (Schallock et al., 2023) for these were too large, leading to an overestimate of the sulfate contribution to extinction still visible to the end of 2020 in the southern hemisphere" I think you mean: "it looks like they used factors "f"". Please don't speculate here (although I understand this information may not be directly available).

Modified, now ”correction factors ”f” by Schallock et al. (2023)”. Is defined in reference.

Line 138-139: “In April 2023 there appears to be a southern midlatitude volcanic event missing in our inventory” I asked this in the previous review. What eruption is missing? Are you referring to the dotted black OMPS-LP line? There seems to be a peak in austral winter in this dataset in the southern midlatitudes for 2019, 2021, and 2023 that is not seen in your model runs or the GLOSSAC dataset? Please check and describe the discrepancies here.

Unfortunately, I could not identify the possibly missing eruption in the Smithsonian database or on the NASA SO_2 website (links in Schallock et al. (2023)). Problem more likely due to model or instrument artifact. Text expanded.

Line 163: Change “causes also” to “also causes”.

Corrected.

Line 164-165: Change: “The cooling rate by the injected water vapour we cannot directly derive with our online method.” to “The cooling rate by the injected water vapour cannot be directly derived using our online method” or something similar.

Corrected.

Line 190-191: “Is strongly enhanced in midlatitudes by up to 0.4 ppmv in agreement with Solomon et al. (2023) and HOCl in high latitudes” This sentence is confusing. I assume you mean HOCl is enhanced at high latitudes. It also looks like it is enhanced at 52S too. It is just hard to see because it is on the same axis as ClONO2.

Modified. MLS ClO is now also included in Fig.4 for support.

Line 194-195: “Here we obtain similar results as Zhang et al. (2024b), we found however from a sensitivity study that 195 the reaction HOBr+HCl contributes only a small fraction to chlorine activation.” Do you show this anywhere? What was the sensitivity study? Zhang et al looked exclusively in the middle latitudes.

This refers to a special version of scenario 6 using the * case in Table 1. Text improved, also in the caption of Table 1.

Line 203-204. “the midlatitude lower stratosphere and almost complete loss at the edge of the ozone hole in winter 2020 in agreement with MLS.” Considering the initial offsets between your simulations and MLS in June 2020, it looks like you are overdoing the total depletion compared to MLS, you just arrive at the same minimum.

A bias (offset) is also in Solomon et al. (2023). An exact agreement cannot expected because of resolution differences and necessary parameterizations with uncertainties.

Line 205-206: “Simulated ozone changes are consistent with Solomon et al. (2023), Stone et al. (2025) and Zhang et al. (2024a)” They are consistent with Solomon and Stone in the midlatitudes, but not in the polar region.

Our results look rather similar to Figs. 4 in Solomon et al. (2023) and Stone et al. (2025), and for Hunga, Zhang et al. (2024).

Line 225: "the remaining effect at TOA was about 0.05 Wm-2" This is .07 on line 152.

Thanks, corrected.

Line 228: "Boreal fires in 2017 and 2019 reduced the volcanic forcing at TOA but enhanced it at the tropopause. It is needed to explain the observed AOD" Are the 2017 fires needed? No results are shown before 2019. What volcanic forcing are you referring to? Hunga? Is it the same for the ANY fires? Line 228. What volcanoes are you referring to in 2017 and 2019? Where do you show that the fires reduce the volcanic TOA forcing?

The figures in the main text include now also 2017 and 2018. The forcing changes by the Boreal fires are included now in the text of section 3.1 and the conclusions. The volcanoes Raikoke and Ulawun in 2019 were mentioned in Section 3.1.

References

Santee, M. L., Manney, G. L., Lambert, A., Millán, L. F., Livesey, N. J., Pitts, M. C., Froidevaux, L., Read, W. G., and Fuller, R. A.: The Influence of Stratospheric Hydration From the Hunga Eruption on Chemical Processing in the 2023 Antarctic Vortex, J. Geophys. Res. -Atmos., 129, https://doi.org/10.1029/2023JD040687, 2024.

Schallock, J., Brühl, C., Bingen, C., Höpfner, M., Rieger, L., and Lelieveld, J.: Reconstructing volcanic radiative forcing since 1990, using a comprehensive emission inventory and spatially resolved sulfur injections from satellite data in a chemistry-climate model, Atmos. Chem. Phys., 23, 1169–1207, https://doi.org/10.5194/acp-23-1169-2023, 2023.

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Stone, K., Solomon, S., Yu, P., Murphy, D. M., Kinnison, D., and Guan, J.: Two-years of stratospheric chemistry perturbations from the 2019/2020 Australian wildfire smoke, Atmos. Chem. Phys., 25, 7683–7697, https://doi.org/10.5194/acp-25-7683-2025, 2025.

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