Short reply to major remarks on "Radiative forcing and stratospheric ozone changes due to major forest fires and recent volcanic eruptions including Hunga Tonga"

1 General

We thank the reviewers for their constructive remarks on improvement of figures and text, including references that we were not aware of that they should be discussed in the paper. We reply here to the major points and misunderstandings concerning the main focus of the paper, a detailed point by point reply will follow in the final response phase. The reviews and discussion in the APARC Hunga Tonga community convinced us to perform another sensitivity study with more realistic Hunga water vapor which will be included in the revised paper. In the following the reviewer's remarks are in bold.

2 Review 1

However, the paper suffers from a lack of an appropriate literature review, especially regarding science around Hunga and its effect on ozone loss. The authors also need to discuss how their work falls within the existing literature within the discussion of their results.
We will expand the introduction and the discussion on that, especially with regard to Hunga Tonga water vapor. This will include a statement that our main focus is heterogeneous chemistry in the lower stratosphere and on radiation.
The author's need to do a proper literature review regarding Hunga in particular. I know you mention Santee et al., (2024) and direct readers there for other studies, but you are modelling Hunga impacts, so please provide a proper literature review here and later on discuss your results in the context of these references and their conclusions.
We selected the references regarding our main focus, but agree with the referee that some additional material on gas phase effects of Hunga should be included in the discussion.
I find the description of the simulations in section 2.1 and 3 very confusing. You mention, in multiple instances, simulations where you co-inject SO2. A lot of times I interpreted the co-injection as a sensitivity simulation, but how does this differ from what actually happened? For example on line 138 you mention an experiment with co-injection. Is the only difference between this run and the normal Hunga run an extra 100 Kt of SO2? Was the normal hunga run also have co-injected H2O and SO2? These sections need to be made clearer, with each distinct simulation having a title. It would make things even clearer if the titles of the different simulations were included in a legend in the figures. A table that shows the different

simulations with what is and isn't included would also be welcome.
This will be improved. A table at the beginning of the results-section will help. 'Co-injection' with respect to the vertical distribution of SO_2 and H_2O from Hunga was considered only in one scenario, in the others the Hunga SO_2 injection was estimated from 3D OSIRIS observations covering a latitude belt and Hunga H_2O was injected in a relatively small slab (line 63ff) leading to too much loss by ice formation. The table and the figures will include a more realistic Hunga scenario where SO_2 and H_2O are injected in similar spatial patterns with the H_2O injection occuring about 1.5 km higher than the SO_2 one to avoid most of the ice formation.

For example, Figure 4 can be a 4 panel figure separating HCl and ClO. That way readers could actually distinguish ClO. Figures also need axis labels and panel letters.
We agree, see the revised Fig. 4 of preprint with colors changed according to referee 2 (Fig.1). The missing panel letters were due to a problem in the graphics software 'ferret'.

A major concern I have is your model's timing of August HCl recovery in the polar region in 2020-2023. In 2019, HCl in your model control simulation recovers similarly to MLS, but in 2020, HCl in your control and experiments seem to recover almost a month later than MLS. This will give an extra month (September) where you will likely have enhanced activated chlorine due to enhanced het. chem than what is likely occurring in MLS and therefore you may be overestimating your ozone loss. If HCl in your experiments recover later than your control it would be different, but they are recovering at the same time, and the timing does not agree with MLS. This needs to be addressed by comparing with MLS HCl climatology and investigated if it is a dynamical or chemical issue in your simulations.

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.

Line 123-124: The author's state "In April 2023 there appears to be a southern midlatitude volcanic event missing in our inventory or the Hunga Tonga SO2 injection is underestimated." What do you mean there appears to be? If you think there is an event missing in your dataset, please check it, and then either correct it or definitively state that it is missing. Also, why would an underestimation of the Hunga SO2 injection affect April 2023 values, a year after the eruption, but not earlier?

I downloaded the newest version of GloSSAC where this feature is not present in contrast to OSIRIS and OMPS-LP (Fig.2). Text will be modified.

Line 197-198: "Hunga Tonga water vapour had only a small effect on ozone and radiative forcing." Is this discussed anywhere in the paper? I can't find it, but apologies if I missed it. If it is not discussed please add it in and also please compare to existing literature.

For example: Zhang et al., 2024 shows ozone decreases in the midlatitudes due to the diluting of aerosols increasing the HOBr +HCl reaction. Wilmouth et al., 2023 also shows gas phase ozone loss in the midlatitudes due to elevated OH from the injected water vapor? How do your results compare with these and other papers?

Text will be expanded using the simulation with more realistic Hunga water vapor. This will include short comparisons with Wilmouth et al. (2023) and Zhang et al. (2024). Some results, e.g. for 22^oS and 26hPa, might be shown in the Appendix.

3 Review 2

Specifically, Figure 1 gives the impression that the forest fires caused larger global stratospheric AOD enhancement, which is not the case. The magnitude of the observed global stratospheric AOD in 2022 summer has been established (Khaykin et al., 2022; Legras et al., 2022; Knepp et al., 2024; Joerimann et al., 2025) to have been substantially higher in 2022 than in 2020 and 2021 (indeed 2022 had the highest since the Pinatubo aerosol in 1992), and this can be seen clearly for example within Figure 10c of Khaykin et al. (2022).

The underestimate of Hunga SAOD in EMAC was an artifact of the used SO_2 injection method based on OSIRIS. In the new simulation SAOD enhancement by Hunga is much larger in tropics and southern midlatitudes and more consistent with the most recent GloSSAC data, see Fig.2, light blue curve. Text will be adjusted and expanded.

There is a similar issue with Figure 3, with the 2019-2023 variation in SAD shown only at 1 altitude level (68hPa), this being in the lowermost stratosphere, the altitude at which the wildfires emitted, but 5-10km below the altitude of the Hunga aerosol.

The main focus of this paper is on the lower stratosphere but a panel on the tropical middle stratosphere is included in the revised version to avoid confusion, see Fig. 3.

In summary, Figure 1 needs to be re-drawn with a solid line used for the GloSSAC stratospheric AOD, this being the benchmark stratospheric AOD dataset (currently represented by a dotted line), and the current solid line used for OSIRIS changed to a dotted line. The y-axis range also needs to be increased accordingly (see revision M-MR1 below).

Has been done, see Fig.2. The color scheme has been adjusted in all figures, using a full black line for observations.

Figure 3 needs to be re-drawn to show impacts comparing vertically-integrated stratospheric column-SAD, then inclusive of both impacts, and providing readers of the article with a balanced representation of stratospheric variations through the 2019-2023 period.

Such an integrated quantity does not represent the strongly temperature

and composition dependent heterogeneous chemistry where local SAD is for. Integrated quantities are shown in Fig. 1 (or Fig.2 here) but useful only for radiation effects.

Again, these presentational issues are relatively easy to correct. And whilst this is somewhat disappointing that the authors should have submitted an article that seems not yet ready for expert peer review, I am aware of the APARC Hunga impacts report has a deadline of 31st July for papers to be citable, and may have affected the authors decision to submit prematurely.

This is exactly the case and led to the improved scenario mentioned earlier.

References

Grooß, J.-U., Brautzsch, K., Pommrich, R., Solomon, S., and Müller, R.: Stratospheric ozone chemistry in the Antarctic: what determines the lowest ozone values reached and their recovery?, Atmos. Chem. Phys., https://doi.org/10.5194/acp-11-12217-2011, 2011.

Wilmouth, D. M., Østerstrøm, F. F., Smitha, J. B., Anderson, J. G., and Salawitch, R. J.: Impact of the Hunga Tonga volcanic eruption on stratospheric composition, PNAS, 120, https://doi.org/10.1073/pnas.2301994120, 2023.

Zhang, J., Wang, P., Kinnison, D., Solomon, S., Guan, J., Stone, K., and Zhu, Y.: Stratospheric Chlorine Processing After the Unprecedented Hunga Tonga Eruption, Geophys. Res. Lett., https://doi.org/10.1029/2024GL108649, 2024.

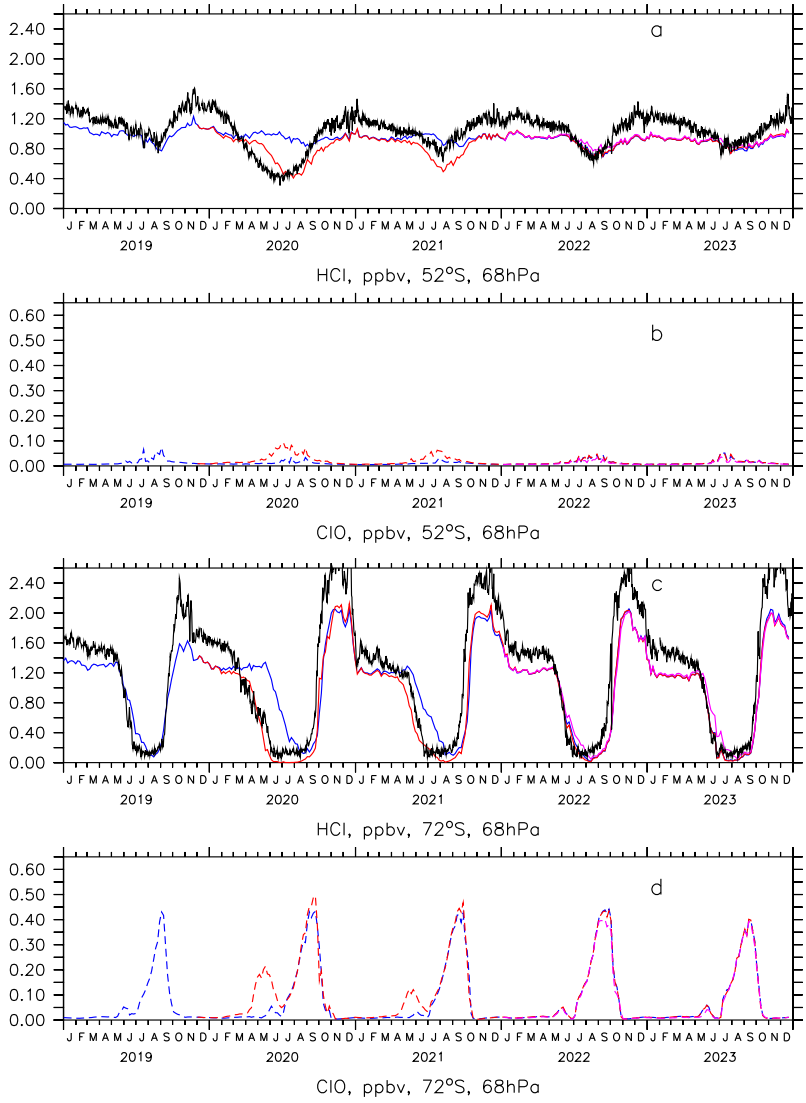

Figure 1: Simulated and observed HCl at 68 hPa at 52^{o}S (a) and 72^{o}S (c). Blue: EMAC with Hunga Tonga including water and dynamical effects of fires, Purple: EMAC without Hunga Tonga, Red: EMAC with Hunga Tonga and enhanced heterogeneous chlorine activation on organic particles. Black: MLS observations. Green curve for EMAC without fires not shown because very close to the blue one. Sensitivity studies close to the red curve not shown. Dashed curves for calculated ClO (b and d, MLS not shown). 5 day averages for EMAC, daily averages for MLS.

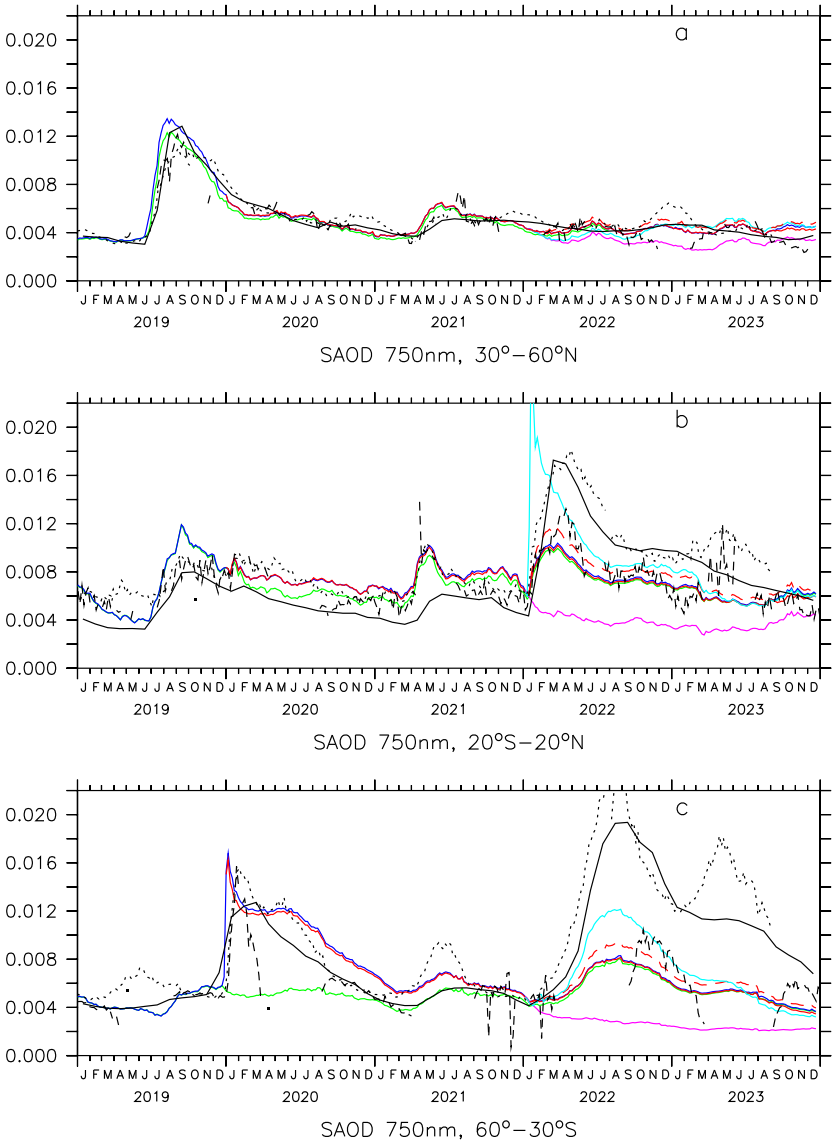

Figure 2: Stratospheric aerosol optical depth in tropical latitudes (b), northern (a) and southern midlatitudes (c), zonal 5 day averages. Green: EMAC without fires, blue: EMAC with 3 major fires (w/o heterogeneous chemistry on organic aerosol), red: EMAC with heterogeneous chemistry on smoke particles, red dashed: same with 500 kt of SO_2 from Hunga Tonga, light blue: same but SO_2 almost co-injected with H_2O but H_2O injection shifted 1.5km upward, purple: EMAC without Hunga Tonga, black dashed: observed by OSIRIS, black: GloSSAC, black dotted: observed by OMPS-LP.

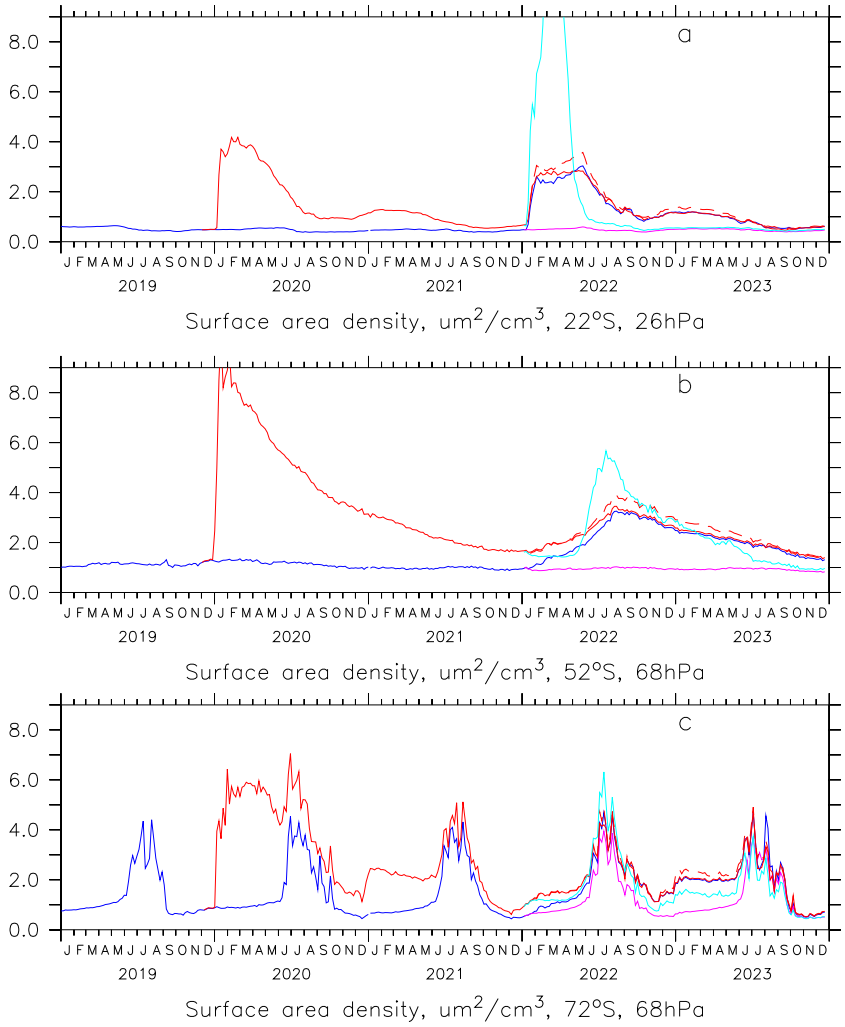

Figure 3: Calculated surface area density [μm^2/cm^3] in the middle stratosphere in southern subtropics (a) and the lower stratosphere in southern middle (b) and high (c) latitudes. Blue: sulfate and PSCs only with dynamical effect of smoke; Purple: without Hunga Tonga; Red: Including smoke particles; Red dashed and lightblue as in Fig.2