We would like to thank the reviewers for their additional comments which have further improved the paper. Author response to reviewer comments is in bold.

Review 1

General Comments
The paper has improved a lot and is better to understand with the revised figures. There are still some minor issues concerning clarity.

Specific Comments
Line 106: Can be misleading. Do you mean "measurements of aerosol extinction at 675 nm using the retrieval method of the NASA Goddard space flight center" or just refer to the data base?

Changed to reviewer's suggestion to avoid any confusion

Move "using MERRA2 reanalysis (Gelaro et al., 2017)" from line 119 to line 113. Is the nudging applied to the whole atmosphere or only the lower part of it?

We have moved the reference to when specified dynamics is first mentioned as the reviewer suggested. We have also included more information of what the specified dynamics is doing.

Line 113: added in "…using MERRA2 reanalysis of winds and temperatures"

Line 114: A remark on ClY and BrY and included halocarbons should be included here (if the list is too long, in the supplement) to avoid the speculations in line 254. In the AGAGE-database are plenty of halocarbons to use for quantification and boundary condition.

Added in on line 114: "Emissions of ozone depleting substances are from CMIP5 (Meinshausen et al., 2011). In the southern hemisphere midlatitudes, the model total column inorganic chlorine (Cl_y) is between 4×10^{15} and 5×10^{15} molecules/cm^2, and inorganic bromine (Br_y) is between 19-21 ppt in good agreement with the observed and inferred values (WMO, 2022).

Line 166: Mass ratio or? Please be precise.

Changed to mass ratio on line 162.

Line 197: Why? This does not look like external mixing. In the formulae by Solomon et al (2023) the molecular weight of hexanoic acid is used for organics as in line 245. Also in Shi et al. (2001) molecular weights are needed.

Shi et al. (2001) uses molecular weight of H2SO4 to convert from H2SO4 molality (mol kg-1) to weight percent, which is defined as the mass of H2SO4/mass of solution*100. Therefore, when we assume organics are mixing with the sulfate aerosols, we only need the organic mass to adjust the H2SO4 weight percent, we don't need molecular weight.

In Solomon et al. (2023), the molecular weight of hexanoic acid was used to convert from organic mass fraction solubility to molarity for conversion to Henry's solubility, which doesn't apply here.

Line 236: "mass fraction" not clear.

Line 215: Change from "mass fraction of background organics to the mass fraction of sulfate in mixed aerosols" to "mass fraction of background organics to sulfate in mixed aerosols".

Line 297 and Figure 1: Is this information not in MERRA2 that you have to use ERA5 here?

We have changed to MERRA2 instead of ERA5 for Figure 1 and Figure S3 10 hPa 60S zonal wind transition time. The results are identical, but it is now consistent with the reanalysis used in the specified dynamics model runs. It was a good catch by the reviewer.

Line 432: Isn't there a free running part for January and February 2020 (line 116)? Is that only for the wildfire case? Please be precise here. With 'specified dynamics' effects of radiative heating on dynamics are underestimated or almost suppressed, especially if the nudging includes the whole stratosphere.

Yes, it is free running during January to February only for the wildfire case, However, the statement is discussing austral spring, a time period when specified dynamics is used for all simulations.

The wildfire smoke radiative heating is not captured. However, the specified dynamics will have the radiative heating feedbacks baked in on the large scale. We feel that no change needs to be made here.

Supplement, Figure S1, caption: Mention that sulfate includes 3 volcanic eruptions. The figure is referenced before this is explained in the text (one page later).

Thanks, added in: Note that both control and wildfire simulations also have emissions from three volcanic eruptions (see main text).

Technical Corrections
Line 54: Replace "very lowmost" by "lowermost".

Thanks, fixed.

Lines 152 to 155: Typos in citations, remove '.'.

Removed the commas, thanks.

References: Please use subscripts for chemical species. In Bernath et al. (2022) journal, issue and link are missing.

Thanks, fixed.

Supplement: Figure S4 (new): pressures in caption inconsistent with panel labels and titles.

Thanks, fixed.

Author Response: Title of Figure R4 (reviewer 2) contains wrong latitude.

My apologies.

Reviewer 2

The revised manuscript did take into account all of my suggestions. I think it is much better to clearly point out that the model is able to absolutely reproduce the observations. I am still somewhat puzzled by the
very low numbers of ClO anomaly much below the MLS accuracy, that also seem to be reproduced, so I am happy to leave them in the paper.

I would ask you to add some more labels in the figures:
- in fig 4e/f it is not clear which ozone data you are showing, likley MLS, similarly in fig S4

The reviewer makes a good point here. We have added in more legends to make it clear what observations we are showing each subplot pair.

In my printout there are some strange misalignments and formatting issues. They will likely be solved when type-setting, just in case I send screenshots from page 4 and 14 to the editor, as I cannot attach them here

Thank you! I have looked at the attached misalignments and I am not sure what caused them. They look fine to me in both word and pdf format, so hopefully should be fine during type-setting.