We thank the editor very much for making important points that will improve the paper further. Please find our responses below:

Minor comments:
1) The referees #1 and #3 both raised the point that the results are based on a single model and can be model-dependent to some extent. Further, they are both concerned (and this concern is also echoed by myself) how the forcing created with WACCM will interact with other models with different physics, chemistry, resolution and vertical and horizontal structure of the atmosphere and circulation. I also understand your point that this cannot be estimated before conducting the multi-model experiments, however, without having in hand a strong proof to dismiss the concerns, I think that it would be fair to discuss the issue thoroughly on a prominent place of the manuscript (a paragraph in Conclusions?)

Thanks for raising this point again. We will try to explain this in more detail and add a paragraph to Section 2.2.1, where we think this would fit best, and add changes to the text accordingly.

It is a standard practice for Climate models to prescribe a stratospheric aerosol distribution, either from observations or other models, e.g., most CMIP6 models use a prescribed stratospheric aerosol distribution over volcanically quiet and active periods. Also, a method to produce aerosol inputs for different models has been developed to assess the impacts of volcanic eruptions like Hunga Tonga. For the recent GeoMIP experiments, 3 out of 6 models used prescribed aerosol distributions from different models: CNRM-ESM2-1 used an input dataset provided by Tilmes et al., 2015, using a previous version of WACCM, the MPI-ESM prescribed their aerosol distribution derived from the aerosol microphysical simulations described in Niemeier and Schmidt (2017) and Niemeier et al. (2020). We should also point out that the same approach for SAI prescribing optical properties from a WACCM6 into the GFDL-ESM4.1 model has already been successfully done (Zhang et al., 2024a). We finally stress again that this experiment purposely specifies one aerosol distribution for input to all models to remove the uncertainty arising from model internal SO2-to-aerosol conversion and aerosol transport.

For the paper, we will add the extra information in Section 2.2.1:

“The CCMI senD2-sai experiment is designed to keep global mean surface temperatures from changing from 2020 – 2030 conditions while greenhouse gas concentrations, ODS, and emissions follow the refD2 CCMI future model experiment (see Figure 1, top panel, red lines). However, instead of including interactive stratospheric aerosols through stratospheric SO2 injections, it uses the stratospheric aerosol distribution or aerosol properties provided by the SSP2-4.5 SAI experiment starting in 2020. **This method has been applied in earlier SAI studies, e.g., 3 out of 6 models participating in the GeoMIP G6 experiment used prescribed aerosol distributions (Tilmes et al., 2022). In addition, WACCM6 aerosol optical**

information was also recently used and imposed in GFDL-ESM4 to perform SAI model experiments (Zhang et al., 2024a).”

2) The referee #2 and particularly the referee #3 raised concerns about how cleanly you isolated the downward influence of SAI. For example, the ref#3 argues with your statement (newly around L495) "...the experimental set-up successfully isolates the SAI-induced stratosphere-controlled (top-down) changes ..."
Also in my eyes, this sentence makes the impression that absolutely accurate decomposition is possible as if the net influence were a mere linear combination of ocean induced and stratospheric responses. In your response to the referee comments and also scattered around the manuscript, you acknowledge that land surface temperature effects and differences in the stratospheric response between the fully coupled and constrained simulations arise, which inevitably contribute to non-linearity of the response and hint at uncertainty of the separability of the mechanisms. I recommend that this issue is again thoroughly and consistently discussed at one prominent place of the revised manuscript.

We thank the reviewer for pointing this out and agree that this should be improved. We have added the extra discussion in the conclusions :

“In addition, the experimental setup successfully isolates the SAI-induced stratosphere-controlled (top-down) changes from the uncertainty in ocean processes under SAI and their (bottom-up) feedbacks with the atmosphere. “
To
“**In addition, the experimental setup removes the uncertainty in ocean processes and feedbacks under SAI by keeping SSTs and sea ice from changing. This setup, therefore, helps to identify changes that result directly from the SAI-induced changes in the stratosphere while some feedback from the interactive land is also included. While the top-down response from the stratosphere and bottom-up feedbacks from the ocean are not completely additive owing to the two-way coupling between the stratosphere and the troposphere/surface climate, such a decomposition facilitates an improved understanding of the tropospheric response that is directly affected by the SAI dynamical impacts in the stratosphere**.”

Technical:
Ref#3 Fig. 3 - Why do you show the results in a vertical reduced domain, not even showing the stratopause region? "Our figures reach above 45km and, therefore, very close to the stratopause region, we think this is sufficient since we are focussing on change in the troposphere and stratosphere"....This argument should appear in the manuscript.

We agree with the editor and add the following information in Section 3, first paragraph:
"In the following, we compare the results of these two SAI experiments, SSP2-4.5 SAI and senD2-sai, for the future 2080 – 2099 period against their respective control simulation from SSP2-4.5 and senD2-fix, respectively, for the period 2020 – 2030, see Table 1). **We are focusing our analysis on the surface climate, troposphere, and stratosphere, and therefore, we limit the top of the analysis to around 45km.** "

Fig. 7 - the information on a spread of the ensemble mean is missing here. Consider adding a shading region for this.

An added shaded region for the intra-ensemble spread makes the figure very hard to read with all four lines and a large spread. The main point of this figure is to focus on the long-term evolution of ozone columns in each experiment, for which looking at the ensemble means and their 3-year running mean is most suitable. We, therefore, prefer to keep the figure as it is.