Reviewer 1:

Everything in the paper seems to be correct, but it is not an exciting paper. The paper does not have interesting new scientific discoveries. Rather, it reads like a long technical report. The descriptions of how each variable responds are overly comprehensive. This would make a nice introductory paper for a special issue where the results of the experiment they describe are carried out by multiple models. The results are based on only one, albeit excellent, model, but I wonder how model-dependent the results are. And I wonder how forcing created with WACCM will interact with other models with different physics, chemistry, and resolution. By the way, the paper does not describe the resolution of the model simulations.

There are small edits for the authors to consider in the attached annotated manuscript.

Reply:

We thank the reviewer for taking the time to review the paper and adding helpful edits to the manuscript. These will all be addressed in the revised version of the manuscript and also add information to the resolution of the model simulation. The paper serves indeed as a description paper of a multi-model comparison experiment, which will be the basis for upcoming papers that discuss the comparisons of different models. However, it also serves a second purpose to present new scientific findings based on unique comparisons between interactive vs constrained model simulations (prescribed aerosols and prescribed SSTs), showing the importance of SAI-induced stratospheric changes vs feedbacks from the ocean. The differences between simulations with an interactive ocean or specified SSTs is a particularly important aspect for follow-up papers based on the multi-model experiments because for the multi-model experiments all modeling groups were asked to use specified SSTs/sea-ice to constrain climate sensitivity. Hence, this paper shows the effects arising from the ocean coupling, which are missed by the models participating in the coordinated CCMI protocol. In the revised version of the manuscript, we adjust the abstract, introduction, and conclusions accordingly. We agree that only one model has been used for the study since we focus on comparing, within the same model, two different set-ups, which allows us to analyze the suitability of the discussed prescribed experiment compared to a free-running experiment. With this, we have also gained scientific insights into important processes and climate feedbacks using the same model framework. We have changed the introduction to state the purpose and scientific questions of the paper more clearly, as well as the outline.

We agree with the listed minor edits; additional responses are listed here:

- We need to keep the name of the CCMI SAI model experiment in the paper consistent with the CCMI-2 overview paper by Plummer et al., 2021, which introduced the name "senD2-sai".
- We add information on the model's horizontal resolution: 0.9 x 1.25 degrees.
- We remove the "complicated names for wet diameter and number in the text. We agree that those are not needed.

Reviewer 2:

The main goal of the paper is to present the CCMI-2022 simulation setup senD2-sai as an alternative to running a fully coupled model with Stratospheric Aerosol Injection (SAI), and to justify its validity by showing WACCM6 senD2-sai output and comparing it to a fully coupled simulation (SSP2-4.5-SAI), which is also used to generate the aerosol input. The main conclusion is that the setup is valid to isolate the direct effects of SAI on stratospheric and tropospheric climate that are not mediated by changes in the ocean. However, the differences with the fully coupled simulation highlight that the setup is not valid to study surface climate, as the response in surface temperature and precipitation is very different. The stratospheric response is isolated in senD2-sai as a strengthened polar vortex in both hemispheres, which impact surface climate by inducing a positive NAM and SAM. The ocean-mediated response is El Niño-type including its known teleconnections affecting wave activity in the troposphere and stratosphere. Therefore, the conclusion is that the setup is useful to study the effects of SAI on the stratospheric dynamics and chemistry in a homogenized framework, but that doing this has non-negligible limitations due to the missing ocean feedbacks.

The manuscript includes noteworthy and novel insights on the separation between the stratospheric only effects and the effects including the ocean feedbacks. The analyses are robust, the figures clear. However, the large number of simulations and having to remember their technical description and slight differences somewhat difficults following the paper and obscures the main points. I have some questions and suggestions to improve the readabilty and facilitate interpretation of the results. I also include some specific comments on the text below.

We thank the reviewer for reviewing the paper and providing detailed and very useful suggestions for improvements. We are addressing the comments below on a point-by-point basis.

General comments:

For instance, could the number of simulations used be reduced? For the reader it is diffucult to remember the differences between all the simulations, so I suggest keeping a smaller number of simulations in the main results description, and discuss the complementary runs in a separate discussion section. For example, I understand the interest of comparing with refD2 from CCMI-2022, but can be in a separate section after the main results are presented.

We cannot reduce the number of simulations used in this study because they are all required to provide the analysis. However, to address this comment, we updated the description of the experimental design to outline the purpose of each of the different experiments more clearly. The description of the different simulations is provided in Table 1. In the revised manuscript, we move the references to the table to the beginning of the experimental design section.

Experiment Name	GHG; ODS Scenario	Simulation Period	Ensemble Members	SSTs	Control Experiment 2020 – 2030
SSP2.4-5	SSP2.4-5	2015–2100	3	interactive	SSP2.4-5
refD2	SSP2.4-5; refD2 CFCs	2015–2100	3	interactive	refD2
SSP2.4-5 SAI	SSP2.4-5	2020 – 2100	2	interactive	SSP2.4-5
senD2-sai	SSP2.4-5; refD2 CFCs	2020 – 2100	3	prescribed (2020 – 2030)	senD2-fix
senD2-fix	SSP2.4-5; refD2 CFCs	2015 – 2035 (2100)	3	prescribed (2020 – 2030)	

To improve the readability of the different experiments, we added more explanation and changed the sub-sections to Section 2: Model Description and Experimental Design. We are merging subsections 2.2, 2.3, and 2.4 under the new subsection 2.2, called Experimental Design, with a brief introduction to the three subsections to outline the need and purpose of the experiments. We also update the abstract, introduction, and conclusions to outline the scientific questions of the paper and the purpose of different experiments.

If I understand correctly, the only difference between SSP2-4.5 and refD2 is the implementation of eruptive sulful injections and the evolution of ODS. In this sense, the cleanest comparison would have been to create a simulation exactly equal to refD2 but including the SAI. However, this is not available and that is why SSP2-4.5 and SSP2-4.5-SAI have been used. Is this correct? Please explain the reasoning for having both simulations in the manuscript in Section 2.2.

Yes, the reviewer is right, and we hope that the more detailed description of the different experiments outlined above will clarify the point. Again, the main difference between SSP2-4.5 and refD2 is the slight difference in CFC concentrations shown in Figure A1. The second difference is the details of the eruptive volcanic SO2 emissions between 2015 and 2025, which resulted in differences in stratospheric AOD for this period (Figure 1, bottom panel). Differences in surface temperature and precipitation between the two runs are not significant. However, it is important to be aware of the differences in CFC concentrations, which explain the differences in the evolution of future total column ozone between these runs.

As explained in the manuscript, the interactive WACCM model simulation with SAI was performed using SSP2-4.5 as a baseline (control) simulation using the same GHG concentration. Differences between the simulation with and without SAI for the interactive model simulation have to use SSP2-4.5 and SSP2-4.5 SAI. We agree with the reviewer, and we pointed out in the text that it would have been better to perform the interactive SAI model simulation based on refD2, instead of with SSP2-4.5. This would have reduced the discussed simulations. This cannot be changed at this point, since other modeling groups have already performed simulations with the stratospheric aerosol distribution from the SSP2-4.5 SAI experiment from WACCM.

For the prescribed aerosol experiment with fixed SSTs and sea-ice, senD2-sai, used GHG concentrations aligned with the refD2 simulation. This experiment used the available stratospheric aerosol distribution from SSP2-4.5 SAI. To assess the impacts of SAI in this case, we will have to perform differences between senD2-sai and refD2.

Also, Figures 3, 5 and 6 show the differences between SSP2-4.5 SAI in 2080 – 2099 and the control experiment (SSP2-4.5) in 2020 – 2030. Is it really necessary to use control simulations, SSP2-4.5 and senD2-fix, for the present period? Could you compare the SAI simulations in the later period to the earlier period of the same simulation, when SAI should not have an impact yet? This is typically done in climate change studies and would simplify the interpretation.

The goal of the comparison in Figures 3, 5, and 6 is to compare the future conditions with (or without SAI for SSP2-4.5) of different variables with a control experiment. The control experiment is defined as the present-day 2020-2030 condition, aligned with the period of the SST climatology and a background aerosol distribution. While we can use SSP2-4.5 (refD2) in 2020-2030 as the control experiment to compare to SSP2-4.5 (refD2) in the future (see table 1), we cannot use SSP2-4.5 SAI or senD2-sai as the control experiment in 2020-2030 to compare to SSP2-4.5 SAI or senD2-sai for future conditions. The reason is that both SSP2-4.5 SAI and senD2-sai include enhanced stratospheric aerosol starting in 2025. The period 2020-2030 does, therefore, not represent clean background aerosol conditions and some changes due to the enhanced stratospheric aerosol burden may be already introduced. We therefore have to use the equivalent experiment to SSP2-4.5 SAI and senD2-sai that has background aerosol conditions throughout the entire period 2020-2030. For this reason, we produce the senD2-fix experiment, which is senD2-sai using the SST climatology for 2020-2030 but uses background aerosol conditions through the simulation. We hope that the new introduction to the subsection provided above will clarify this point.

The senD2-sai setup is not coupled to the ocean and thus the SST and sea ice need to be prescribed. The proposed setup is to prescribe climatological SST and sea ice from the present. However, approach largely dampens the climate variability in these simulations (as seen in Fig. 1), and this is important because the type of responses that are analyzed are modes of variability (NAO, SAM, ENSO). Is there an alternative way to construct these simulations? For example, prescribe the SST from a coupled model SSP-4.5-SAI simulation, similar to what is done for refD2? This would likely improve the total model response to SAI. Is this not proposed to avoid imposing an ocean response which is suspect of being model dependent? Has this been discussed in previous literature? This discussion should be included in the paper.

The CCMI SAI experiment has been defined to use a climatological SST and sea-ice distribution between 2020-2030 from each model not to impose an ocean response. For this experiment, we require each model to use its own 2020-2030 conditions to prevent any sudden jumps in temperature when starting the experiment. One could have used a timeseries between 2020-2030 instead of climatology to better present modes of variability. The question would have been how to set up such a distribution, which would have to be repeated for the entire simulation between 2020 and 2100. Due to this complication, using a climatology is a more straightforward approach. Furthermore, since SSTs and sea ice are prescribed, it is unclear how this change would improve the response to SAI since it would not change during the simulation.

Since an important motivation for the study is the need to increase the number of models that perform SAI experiements, it would be good to add information of how many CCMI-2022 models do not have a coupled ocean currently.

We agree with the reviewer, currently, only 3 of 8 models that participate in CCMI-2022 simulations have a coupled ocean. We are adding this information to the introduction.

Specific comments:

L47: Keeble et al. 2021 looks at CMIP6 models.

We agree and adjust the text accordingly.

L53: 'stratospheric aerosol distribution or optical properties'. What does this 'or' mean? Is it an option or are both things equivalent?

Different models can either implement a prescribed stratospheric aerosol distribution or have to prescribe optical properties instead. We remove this detail from the introduction and outline it in detail in the experimental description for senD2-sai.

L80: 'SSP2-4.5 and refD2' . The name SSP2-4.5 is a bit confusing, since refD2 follows that same IPCC scenario. Could this be called perhaps Ref_coupled, and the other one SAI_coupled or something similar?

We adopt the experiment names from the CMIP6 and CCMI-2 definitions and do not want to confuse the readers by introducing new names. With the updated introduction of the experiments, we hope the names make more sense to the reviewer. refD2 has been the official CCMI future experiment defined by Plummer et al., 2021.

L119: Is it correct that you changed the interhemispheric temperature gradient condition by an interhemispheric symmetry aerosol distribution condition? It could be stated more clearly.

With the change, we are changing the interhemispheric temperature gradient. We add a sentence to the text to clarify:

In SSP2-4.5 SAI, we modified the existing feedback control algorithm to require that sulfur injections are equally distributed into both hemispheres to maintain the global near-surface temperature and the equator-to-pole at the desired level (Figure 1, top for near-surface temperature). "*However, with this modification, we do not maintain the interhemispheric temperature gradient to the present day, which results in an overcooling of the NH, as discussed below*".

L150-151: the globally averaged precipitation change is half as large in the senD2-sai compared the coupled simulation, and this is not clear from the text, which only states 'the feedback from the atmosphere and land changes in the globally averaged precipitation in senD2-sai is similar to that in SSP2-4.5 SAI'.

To address the comment, we revise the text to:

"On the other hand, the feedback from the atmosphere and land changes in the globally averaged precipitation in senD2-sai is similar to that in SSP2-4.5 SAI (Figure 1, red lines, middle panel). Details of the difference in surface climate between these two experiments will be discussed in Section 3."

To

"On the other hand, the feedback from the atmosphere and land changes in the globally averaged precipitation in senD2-sai **is significantly lower than** in SSP2-4.5 SAI (Figure 1, red lines, middle panel). Details of the difference in surface climate between these two experiments will be discussed in Section 3."

L187-189: might this explain the strange change in values in Figure 3 top row central and right panels, at 50N/S near the tropopause?

This is explained further below, Line 190ff, we add to the test:

"In WACCM6, the tropopause is internally calculated via the WMO lapse rate calculation for latitudes lower than 50◦N and S. It uses climatological tropopause values for latitudes larger than 50◦N and S due to difficulties deriving the tropopause at all times in higher latitudes, which is generally lower than the model internal tropopause. **This feature results in the abrupt change in SAD around 50N/S around the tropopause(Figure 3, top middle and right panels).**"

L243: correct typos 'stratosphre' and 'coupld'

Thanks for pointing this out; we will correct it.

L253: 'Compared to the present day and independent of SAI' does this mean in scenarios that do not implement SAI? It could be stated more clearly.

Thanks, this is a general statement and so we change the sentence to: "**In general,** the interplay of chemical vs dynamical drivers of ozone depends on location and season (e.g.,WMO, 2022)."

L275: Here and at other places the difference in wave driving between senD2-sai and SSP2-4.5SAI is mentioned, but it is not shown. Could you include a figure of the changes in Eliassen-Palm flux divergence, at least in the supplementary material.

We included the requested figure in the supplementary material and referred to it at that point as well as at an earlier place where we point to wave-driving changes.

L285: Here an El Niño like response is mentioned but this has not been shown yet in the paper, so please include a mention that it will be shown later.

We change the sentence to: "These changes may be in part driven by the El-Nino like response in the tropical Pacific and its teleconnections, as well as uneven near-surface temperature changes in the coupled simulation **(see Section 3.2)**."

L293-294: 'the discussed differences between the prescribed and interactive experiment are similar' The phase 'differences are similar' sounds a bit strange, I suggest rephrasing to 'changes are similar'.

We agree and change the text accordingly.

L320-321: 'The symmetric injetion in both hemispheres using SAI has been shown to counter the weakening of the AMOC due to climate change'. This seems at odds with the fact that a strong cooling is observed in the North Atlantic in Figure 9 central panels (SSP2-4.5).

While there is a strong cooling in SSP2-4.5 (without SAI) compared to the present day, AMOC has been shown to weaken still; references are given in the text. The cooling with SAI is stronger and imposes an even stronger cooling over the NH and therefore counters the AMOC. The text is already pointing to various papers that discuss this.

L352: 'dominated the response in the region'. Suggested: 'in the Pacific region'

We agree with the change.

L363-367: 'This austral summer negative SAM and weakening of eddy-driven jet are therefore likely connected to the equatorial Pacific and the El-Nino-like response resulting in changes in tropospheric winds and Kelvin wave fluxes into the stratosphere to contribute to the winter and spring stratospheric easterly response.' I do not understand this sentence. First, it is unclear how Kelvin waves propagating in the stratosphere influence the SAM. L'Heureux and Thompson

(2006) do not mention Kelvin waves. Second, the beginning of the sentence refers to the austral summer SAM response, but the end of the sentence refers to the ‘winter and spring response’.

We have corrected the sentence to “This austral summer negative SAM and weakening of eddy-driven jet are therefore likely connected to the sea surface temperatures changes in the equatorial Pacific and the El-Nino-like response there, resulting in changes in eddy heat and momentum fluxes and tropospheric winds.”

L455: modeling groups (add ‘s’)

Change will be added.

Reviewer 3:

General comments

This paper deals with the Stratospheric Aerosol Intervention, a geoengineering technique proposed to mitigate global warming. In this article, a new simplified Stratospheric Aerosol Intervention (SAI) experiment (senD2-sai) is presented as an alternative to run a fully coupled model (SSP2-4.5-sai) for assessing the SAI impacts on both the Troposphere and Stratosphere in the context of the Chemistry-Climate Modeling Initiative (CCMI-2022). In order to prove the overall equivalence of senD2-sai and SSP2-4.5-sai, a detailed comparison between the simulations is presented. It is argued that the results regarding the SAI effects on the stratosphere are overall similar and that the senD2-sai configuration can be used to evaluate the impacts of SAI on stratospheric chemistry and dynamics for multi-model comparison studies. On the other hand, for the tropospheric and surface climate response, the two setups show very different near-surface temperatures and precipitation patterns. Therefore, senD2-sai is not suitable for investigating near-surface climate. Still, results concerning the SAI-induced changes at the surface are discussed as well. For example, the senD2-sai experiment allows the authors to attribute part of the cooling in the North Atlantic near Greenland in SSP2-4.5-sai simulation to the strengthening of the polar vortex due to the SAI impacts on the Stratosphere in the Noth Hemisphere, which can propagate down to the surface, resulting in a positive phase of the NAO.

Overall the study can be a valuable reference for future studies about the effect of SAI in the stratosphere using a simpler model setup that in turn can attract more modeling groups, which do not have the possibility to run a fully coupled model, to participate in the CCMI. Therefore, I consider it worth publication. However, there are several caveats that prevent the publication at this stage, which I list below as major comments. Further, I have multiple questions and suggestions aiming to improve the clarity of the paper. Moreover, some technical comments are also given in chronological order.

We thank the reviewer for their very valuable comments and suggestions, which are addressed on a point-by-point basis.

Major comments:

1) The manuscript is not acceptable in its current form as a research article, because it does not report any substantial new results. Also, the current title and abstract look rather like a technical report. Technical note would be a more suitable format and further changes to the manuscript should be made with this goal. However, note that GMD is the recommended journal for description of numerical experiments.

We disagree with the reviewer that the paper does not report substantial new results. However, we have now modified the abstract, introduction, and conclusions to more clearly state the novelty of the paper and its key scientific questions and findings, as outlined below.

2) The main message of the manuscript is not clear (if not the technical note). What I am particularly missing is a formulation of some scientific questions in the introduction that would be answered in the conclusions based on the results presented. Right now, the conclusion section is a very incoherent mixture of summary and discussion of technical aspects.

We agree with the reviewer that the main message of the paper did not come through properly. We have now changed the abstract, introduction, and conclusions to clarify the two purposes of the paper. First, we discuss the setup and details of the new CCMI-SAI experiment. Second, we present new scientific findings dealing with comparing interactive vs simplified model simulations and the importance of SAI-induced stratospheric changes vs feedbacks from the ocean and sea ice. Research questions addressed in this paper are now included in the introduction, and in addition to other changes, we add: "Various studies in the literature attempted to elucidate whether and how much SAI-induced stratospheric heating contributes to regional climate changes (Simpson et al., 2019; Banerjee et al., 2021; Jones et al., 2022; Bednarz et al., 2023). Other studies have also noted changes in the equatorial Pacific associated with the modulation of El-Nino Southern Oscillation variability under SAI (e.g. Zhang et al., 2024) and identified changes in its teleconnections (e.g. Bednarz et al., 2023). With the experimental setup, for the first time, we can compare the results in interactive model simulation with the constrained setup in one modeling framework, allowing us to identify and isolate the importance of top-down stratospheric changes for the tropospheric climate response from the bottom-up feedbacks from changes in the ocean and sea ice. We further show that the CCMI SAI experiment is suitable for assessing the impacts of SAI-induced changes on stratospheric chemistry and dynamics."

3) I am doubtful about the proposed experimental set-up of the senD2-sai experiment because the prescribed aerosol distributions from WACCM will be non-native for other models (different tropopause heights, tropical widths, BDC structure..). It would make more sense if the prescribed distributions were derived individually for each model.

The main purpose of this experiment is to constrain the experiment that models use the same or very similar aerosol distribution. The large inter-model difference in the simulated aerosols under SAI was the main obstacle in interpreting differences in climate response to SAI in earlier studies between models that derived their own aerosol distribution (discussed in Pitari et al., 2014, Tilmes et al., 2021). This experiment thus purposely specifies one aerosol distribution for input to all models to remove the uncertainty arising from model internal SO_2-to-aerosol conversion and aerosol transport.

Many CMIP models use "non-native" aerosol distribution to simulate the effects of stratospheric aerosols, including for larger volcanic eruptions (e.g. Bilbao et al., 2024, https://doi.org/10.5194/esd-15-501-2024). In addition, when prescribing a fixed aerosol distribution, inter-model differences in tropopause height will matter to some degree if aerosols are only prescribed in the stratosphere. We have already acknowledged in the manuscript that the senD2-sai prescribed aerosol distribution would not be exactly the same as that of each model, which would be simulated interactively. However, as noted above and throughout the manuscript, 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.

4) At many places in the manuscript, linearity and hence separability of the responses is often implicitly assumed, leading to statements as "...this experimental setup allows us to isolate the stratospheric (top-down) impact of SAI on the troposphere and removes any potential (bottom-up) influences from an uncertain ocean response.". It has to be acknowledged that in reality there will be non-linear interactions between the mechanisms and that the "isolated" stratospheric impact is in fact (arguably highly, but as your result show definitely significantly) idealized.

We agree with the reviewer that the CCMI SAI experiment is idealized since it constrains both the stratospheric aerosol distribution and SSTs and sea ice. To assess the differences between the constrained setups, we have added a comprehensive discussion on these differences in the second part of the paper. This setup does indeed allow us to isolate the stratospheric impact from the ocean feedback in one model framework.

Specific and technical comments

To increase the clarity of the model description, it would be beneficial to include Table 1 right after paragraph 2.1 in order to have all the differences among the simulations at hand instead of including it in the appendix. Furthermore, if I understood correctly, the control simulation SSP2-4.5 is only needed since the refD2 experiment was not available at the time of the SSP2-4.5-sai simulation. That is the reason why both are described in section 2.2. For completeness, since at L316-L317, it is said that SSP2-4.5 looks almost identical to refD2, it would also be better to include the plots as in Figures 9 and 10 (first column) for the refD2 simulation in the supplementary material (appendix).

We agree and move the table up, and following the comments of the second reviewer, we more clearly outline the details of the experiments used in the paper. As suggested, we also added a new figure showing both SSP2-4.5 and refD2 surface changes to the appendix.

In paragraph 3.1 on p. 8, it is stated that "in the interactive stratospheric aerosol simulation, aerosols are transported and sediment into the troposphere and deposited at the surface. In contrast, for the prescribed stratospheric aerosol experiment, the model zeros out the prescribed aerosol distribution and SAD below the tropopause." Why are the prescribed aerosol distribution and SAD below the tropopause set to zeros in the simplified simulation (senD2-sai)? If I understood correctly, in senD2-sai instead of using interactive stratospheric aerosols the provided aerosol properties by the SSP2-4.5-sai experiment are used. Are these properties not available for the troposphere? Is the aerosol presence in the troposphere negligible?

Many CMIP and CCMI models prescribe the stratospheric aerosol distribution only and not the troposphere because aerosol in the troposphere is often derived differently. Prescribing aerosols also in the troposphere, can cause the models to double count that aerosol. In the case of SAI, ignoring the SAI-induced elevated tropospheric aerosol load is small. Since this setup is applied consistently to all the models, this does not change the impacts of SAI on stratospheric dynamics and chemistry.

L25: G6 experiments not introduced or referenced.

We add the provided reference (Visioni et al., 2021) earlier to clarify.

L25: typo SSP5-85, it should be SSP5-8.5.

Thanks for finding the typo.

L26: Would not be better to cite Gidden et al. (2019, https://doi.org/10.5194/gmd-12-1443-2019) or Meinshausen et al. (2020, https://doi.org/10.5194/gmd-13-3571-2020) where the SSP5-8.5 scenario is described?

Thanks for the suggestions, we use those references instead.

L45: change "Co-authors" with "et al."; this variation appears also in lines L46 and L100, look carefully if other occurrences are present and change them accordingly.

We will correct the reference.

L81: as in L26, would not be better to cite Gidden et al. (2019, https://doi.org/10.5194/gmd-12-1443-2019) or Meinshausen et al. (2020, https://doi.org/10.5194/gmd-13-3571-2020) where the SSP2-4.5 scenario is described?

We added the references.

L84: in the parenthesis "(Figure 1, top and panel, green lines", do you mean "(Figure 1, top and middle panels, green lines)"?

Yes, thanks for catching this.

L87: before using the acronym "AOD" (Aerosol Optical Depth) state its meaning.

Changed

L91: typo “SSP2.4-5”, change it with “SSP2-4.5”; this typo is present throughout the paper, look carefully and change it accordingly.

Thanks, we fixed those typos.

Fig. 1 and L96 - Statistical significance not assessed in Fig. 1 (missing information on the standard deviation or spread of mean values). What is the meaning of dashed lines in the upper two panels?

We are not showing the mean of the available ensemble members but all the ensemble members using different line styles. That way, the variability and spread can be better identified. We revised the caption in Figure 1 accordingly.

L97: In the parenthesis “(Figure 1, top and middle panels, blue and red lines)”, do you mean “(Figure 1, top and middle panels, blue and green lines)”?

Yes, we meant blue and green lines, and corrected it.

Figure 3 and Figure 6: the control experiment for the senD2 experiment is referred to as “senD2-bg”, should not it be “senD2-fix”?

Yes, corrected.

L102: change “Figure S1” with “Figure A1” as presented in the appendix.

Thanks, we also corrected the other references to the appendix.

L126-127: "However, differences in the aerosol distribution..." But this is underlaid by the intermodel-differences in atmospheric structure and circulation that will persist and the prescribed SAI distributions will not be in balance with this in other models.

A symmetric aerosol distribution (between hemispheres) in different models will result in a specific dynamical and chemical response in the different models. Using a prescribed aerosol distribution will not be impacted by differences in circulation responses. So, the response is indeed constrained. But that is the idea of this experiment.

L138: "...the 5-day instantaneous varying stratospheric aerosol distribution.." - Really, you want to prescribe this from WACCM to all models based on the corresponding model time?

Various models have already run this experiment and did not have a problem with using the input. We point out (as already stated in the text) that the aerosol distribution is zonally averaged which reduced the amount of data substantially. Further details on how other models prescribe the aerosol distribution are given in Jörimann et al., 2025.

L147 - Why climatological SSTs from REFD2 and not from say SSP2-4.5-SAI?

The SSTs have to be consistent with the CCMI2 model experiment used to impose the stratospheric aerosols so we do not impose changes in surface temperature.

Around L195 - the issue of different tropopause heights - This issue will get far bigger when the prescribed field will be used in other models.

As stated above, we don't think, that these differences will have a large effect.

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.

Fig. 4 - Why relative differences for H2O and O3? Also, the restriction to a fixed meridional belt does not take into account possible changes to the horizontal structure of the circulation and width of the upwelling region.

We use relative differences to show impacts in both the troposphere and stratosphere because of the strong gradient in H2O and Ozone across the tropopause. We agree that zonal mean comparisons in the tropical belt do not identify upwelling regions with changes that are also dependent on season. Here, we focus on annual mean and large-scale changes around the UTLS and the stratosphere.

L207: after "compared to the control" add a reference to the Figure "(Figure 3, middle row, first and second columns)", otherwise the sentence appears misleading and one can interpret it as referring to Figure 3, middle row and third column.

Here we refer to changes shown in Figure 4, top left panel. We add this information to the text.

L231 "..simulated weakening of zonal winds..contributes to the direct radiative changes" - How can the zonal wind weakening contribute to the radiative changes and warming? I suspect that you have the causality absolutely reverse here

We agree that this text was not clear and changed it to:

"The simulated temperature reductions in the winter polar lower stratosphere are also impacted by dynamical changes, namely a stronger polar vortex under SAI (e.g., Tilmes et al., 2022), shown for both Arctic and Antarctic polar vortices (Figure 5). **In general, changes in polar**

vortex strength are associated with changes in BDC and polar downwelling, which modulates the strength of adiabatic warming in winter high latitudes.

L236: "..with a somewhat weaker polar vortex.."-> s with a significantly weaker polar vortex

changed

L243: typos, change “stratosphre” to “stratosphere” and “coupld” to “coupled”.

changed

L244-L246: add the reference to Figure 5.

added

The whole discussion of Fig. 5 - The impact on QBO and the difference between coupled and prescribed in this respect is not mentioned.

The effects on the QBO are not investigated here since the paper is already quite long and detailed, and this requires more discussions, which are planned for the multi-model comparison papers. However, we added a sentence about the importance of the QBO to the paper: **“Finally, SAI-induced differences in vertical advection and resolved wave drag (illustrated by the changes in the Eliassen-Palm Flux divergence in Figure A2) can affect the Quasi-Biennial Oscillation (e.g., Richter et al., 2017, 2018), which will be investigated in future studies.”**

L250 "..due to the combined effects.." - You forgot to list tropical upwelling and QBO that are also affected and will play a role.

The causes for change in ozone are changes in GHGs, stratospheric halogen levels and SAI forcing, resulting in both chemical and dynamical changes (including the QBO). We revise the sentence to:

“The simulated changes in stratospheric ozone between 2080–99 and 2020–30 in the SAI runs, as shown in Figure 6, are due to the combined effects of increasing GHGs, decreasing stratospheric halogen levels, and SAI forcings, **resulting in both chemical and dynamical changes**.

L251: change “Figure S2” to “Figure A3”, according to the appendix.

We have changed the naming and order of the figures in the appendix according to changes in the revised manuscript.

L262-L263: add the reference to Figure 4, second panel.

added

Fig. 7 - What is the meaning of thick lines? No information on the robustness of the mean value is given.

We updated the Figure caption and added that the thick lines are a 5-year running mean to better identify differences between the experiments.

Section 3.2 - I think that this section should be deleted because the definition of the prescribed simulation renders it useless for assessing the tropospheric effects. To my understanding you allow the surface temperatures (not SSTs) to evolve according to radiative and other forcings, which contradicts your claims about isolating the top-down mechanism in this section.

SSTs are the main drivers of surface temperatures. We agree that we allow the land to adjust to the changes an, therefore, not completely isolate the effects from the stratosphere alone. But we certainly remove the effects of the ocean and sea ice. We modified the section to clarify that here, only feedbacks from the ocean and sea-ice are excluded. However, we point out that adjustment in the land cannot be responsible for the large-scale surface temperatures

L265 and L267: it is not clear what "(Figure S2)" refers to.

Figure A2 shows the direct effect of SAI on ozone and is therefore referenced here.

L267-L271: add the reference to Figure 6.

Figure 6 had been referenced just a sentence before.

L310: at the beginning of section 2 it is said that CESM2 (WACCM6) is called WACCM6 throughout the paper, hence here just call it "WACCM6" instead of "CESM2 (WACCM6)".

Thanks for catching that! We changed the text accordingly.

Figure 8 - Again, not clear what the dashed and solid lines stay for and there is no information on the spread behind the mean.

We changed the figure caption to note that the different lines illustrate different ensemble members and not the mean.

L385: change "(Figure S3)" to "(Figure A2)", according to the appendix.

done

L411-413 - "The constrained experimental setup using a prescribed stratospheric aerosol distribution and fixed SSTs and sea ice successfully isolates the SAI-induced stratosphere-controlled (top-down) processes from ocean processes and their feedbacks with the atmosphere." - I think that this is an unsupported claim. As argued throughout the review, the set-up plus minus works in the same model that has been used to create the aerosol distribution. Besides this, feedbacks can be and are nonlinear and cannot be fully isolated. This should be at least acknowledged. Also, in my eyes, the effect of and on QBO remains unclear.

We are not sure about the point the reviewer is making. We agree that in one model framework, a comparison with the interactive model experiments vs. the constrained experiments is successful in isolating ocean and sea-ice feedback from stratospheric-induced changes. The CCMI experiment in a multi-model setup is to assess differences in stratospheric dynamics and chemistry in response to the prescribed aerosol distribution. All experiments will be performed with prescribed SSTs and sea ice and we are not planning to compare the interactive models vs constrained models. As mentioned above, we revised the abstract, introduction and conclusion, and hope that the scope of the CCMI experiment and of this paper is more clear.

L426: "..model specific internal adjustements.." What does this mean? And what about validity of the model-specific tuning to other than SAI conditions?

Earlier studies (Fasullo and Richter, 2023) have identified different reasons why WACCM6 requires more injections into the SH than the NH to prevent the hemispheric unsymmetric near-surface temperature cooling, which includes cloud and climate feedback. Model-specific tuning may play a role, especially cloud feedbacks, but this discussion seems beyond the scope of this paper.

We clarify this sentence in the manuscript: “However, this setup in WACCM6 results in hemispherically unsymmetrical near-surface temperature cooling, which could be linked to model-specific internal adjustments, including cloud and climate feedback (Fasullo and Richter, 2023).”

L433: " The setup also removes the potential for an over or under-cooling of models using a prescribed aerosol distribution from a different model (Zhang et al., 2024a)" - How? I probably missed this important feature of the setup.

The setup of fixed SSTs and sea-ice and prescribed aerosol constraints the models to the same long-term surface response from the oceans, thereby providing a consistent dynamical response from the surface up, allowing future comparisons to focus on differences in the stratospheric responses of the models rather than on their different surface cooling patterns. We are not suggesting to have models run with interactive SSTs.

L435 " ..are relatively small, as shown here" - This is a very vague statement. Moreover, it is unclear, whether this will hold also in other models, where you prescribe WACCM distributions.

We point out that this statement refers to stratospheric changes. The set-up of this experiment requires prescribing SSTs and sea ice for all climate models: the original interactive WACCM simulations are the only ones with interactive ocean. Therefore, when other models have performed the same fixed-SST simulations, together with identifying inter-model differences in response to SAI, we will be able to more precisely quantify the relative differences existing between the two WACCM model runs. We hope that the text in the revised manuscript clarifies the setup.