Authors‘ response

The authors thank the editor and the reviewers for their time and the valuable comments on our manuscript. We have addressed the referee comments by answering the questions and implementing suggested changes, with a detailed point-by-point response below. During the review process we have extended the text in some sections of the manuscript to satisfy the more general referee comments, particularly in the abstract and the sections “Experimental setup”, “Heating and total column ozone response”, and “Synthesis and Outlook”. These changes do not present new perspectives or major findings, but rather provide more detail to strengthen and support the key points that we made in the first version of the manuscript.
The authors declare that for this purpose two floats have been added in the revised document:

- Figure A2: a plot of total column ozone anomalies in Dobson units as a function of latitude for each of the five models (temporal average of 2070-2083)
- Table D1: a comprehensive table of heterogeneous chemical reactions included in each models that details, which surfaces each reaction is active on

Adding Figure A2 specifically warranted some more discussion of total column ozone, which has been added in Sections 3.1 and 3.2. These additions also aim to facilitate the planned contribution to the WMO Ozone Assessment of this manuscript and are based on discussions with leading authors of the chapter on stratospheric aerosol injection.
The two floats are referenced in the relevant sections of the main text to further substantiate the key findings of this paper and improve the readability and logical flow of information.
The authors also note that during the review process it was discovered that the postprocessing of total column ozone data from one of the models (WACCM) was inconsistent with the other models, leading to some bias in the anomalies shown in the submitted manuscript (up to around 4 DU). This is now rectified with the total column ozone plots and the Zenodo data repository updated. This does not change any major findings, but the data are now consistent across the models and thus also comparable to other similar modelling studies.

Reviewer 1:

Major comments

The paper contains a comparison of 5 chemistry climate models on geo-engineering in the framework of CCMI. The paper might be suitable for

Atmospheric Chemistry and Physics after some mostly minor revisions.

In the abstract the kind of simulation and the time period should be mentioned. Only acronyms for insiders are not sufficient. The abstract should also include more results. The description of the simulations requires some clarifications. It also looks like that one model has not simulated the full time period (without mentioning in text).

Response:
We thank the reviewer for taking the time to provide valuable comments and corrections, including detailed specific comments. We update the abstract accordingly with significant rewording and new

results, and improve the simulation description with more details and clarifications (also specifically answering later comments on these sections). For the specific and technical comments, **we adopt all corrections that are not separately answered** and address the rest below one by one.

Specific comments

Line 7: Please mention simulation period and if the simulation is transient (or time slice?).

Line 82: Transient SAD?

Changed to increasing SAD, as it is described like that before.

Line 85: 'Forcings' or VMRs of radiatively active species like GHGs, ODS, sulfate? Refer at least to Table 1.

This sentence only mentioned greenhouse gases, which we agree is lacking. We therefore extend it and conjoin this sentence with the sentence before, which references Table 1.

Table 1: Is the here mentioned WMO-Report used or the one of line 40? Please clarify or correct. Are GHGs and ODS transient? Please mention in caption and/or the corresponding rows. QBO nudged to what? References or links?

We see the potential confusion about the 2018 and 2022 Ozone reports and added clarification in the text. For other details the citation of the original experiment description is provided as citation in both in the text and the table caption.

Line 97: 'SSTs and SIC' from model HadISSTI? Please be clear here.

We specified it clearly, by referring to the description of the one model that has no coupled ocean model configuration and thus used a different model for SST and SIC input.

Line 104: Complete the WMO-citation here.

Added reference to Table 1.

Line 135: Are there 2 ozone tracers in this scenario, one for radiation (solar and IR) and one for chemistry? Please clarify and add a sentence on that.

No, there is only one ozone tracer, however, it is updated both on account of changes due to radiative heating and due to chemistry. The differences between the models then originates from the radiation code, which also includes changes in radiation due to chemistry (more or less radiatively active species in different layers). If photolysis rates are pre-calculated (e.g. based on a lookup table), backscattered UV radiation is lower than it becomes with a strongly enhanced anthropogenic stratospheric aerosol layer (which is seen when photolysis is calculated online, respecting the additional aerosol particles). In theory, the altered photochemistry also includes changes to other chemical species that might interact with ozone, but the effect is assumed to be rather small in the first place. This is addressed in the text.

Line 175: Odd assumption. This may add biases.

If this comment refers to the amount of realizations per simulation, this choice was influenced by computational cost and the fact that this analysis is heavily based on the anomalies between different

simulations, often averaged over multiple decades.

Line 185: Assumptions for volcanoes in the future?

This sentence is not meant to imply inclusion of future volcanic emissions and was changed for clarity.

Line 194: Include '(T63)'.

Line 209: Is it possible, to convert this to a notation consistent with the one used in the other models?

The resolution is already given in equivalent degrees latitude and longitude, as well as the spectral truncation, consistent with the other models.

Line 212: List the 6 halocarbons.

Since this information is not consistently provided across all model descriptions, we remove this part entirely, as listing these halocarbons would be unnecessarily detailed for this experiment.

Line 256ff: Does TCO include model specific tropospheric ozone (which only partially cancels out in differences)? I also would prefer to use the abbreviation 'total ozone' later in the text.

Yes TCO includes tropospheric ozone. This is necessary, because changes in stratospheric ozone may be exported to the troposphere. The model-specific anomaly in tropospheric (and total column) ozone is model-specific and this is also part of what we are interested in. Since we are only changing one thing (either chemical or radiative stratospheric aerosol forcing) across each pair of simulations thata we calculate anomalies for, there is nothing that needs to be cancelled out (apart from internal variability, which is why there are three simulations for senD2-sai). Only the background absolute total column ozone values of each model are „concealed“ when showing anomalies, which is by definition. We decide to keep the abbreviation „TCO“ for consistency with existing literature (e.g. Tilmes et al., 2025; https://doi.org/10.5194/acp-25-6001-2025)

Line 349: Photolysis of what? This sentence is confusing, please be more specific. Is more formation of O(1D) by photolysis of ozone and subsequent OH production meant?

We thank the reviewer for pointing this out, the formulation is incomplete. The term „photolysis“ is replaced with the reaction of H2O with O(1D) to produce OH.

Line 350: Insert 'and the largest increase in tropical upwelling'.

Line 401f: It might be too warm for the heterogeneous reactions HCl+ClONO2 and H2O+ClONO2 here.

We aim to clarify this question with the senD2-chem runs that we performed and included in this analysis. When looking at the differences between the chemical-only and the full effects we demonstrate that even over the tropics, significant heterogeneous chemistry takes place, including the destruction of ozone in near-tropopause levels. Despite temperatures rising over time in the senD2-sai simulation, very cold conditions (below 200K) frequently occur.

Line 404: 'because of too high temperatures there...'

We changed the text to include the fact that temperatures are too high there to sustain any meaningful mass in the aerosol phase.

Line 411: 'key regions' should be in title of subsection for easier reading.

Line 413ff: It might be better to merge Fig. 9 and Fig. C1 for easier reading, especially concerning the panel with 'dyn'. The same holds for Fig. 10 and Fig. C2.

We agree that merging would improve comparability of „dyn“ and the three-model „full“ means, but we deem that it would also sacrifice readability and easy access to information due to the figures becoming too large for one page.

Figure 10: Inconsistent to Figure 2 concerning time period.

Unfortunately the multi-model mean TCO anomalies (as well as the three-model mean TCO anomalies) can only be calculated until the end of 2083, as one of the models could only run one of its simulations to that year. This is added in the text.

Line 517 : i.e. equivalent latitude.

Technical corrections

Line 79: Typo.

Table 1: Improve syntax of citation.

Figure 2: WACCM-results missing after 2082. Unit missing at y-axis of panel b.

Missing results are due to computational restraints, leading to an unfinished WACCM senD2-fix simulation. The fact that these results are not available is added in the caption.

Figure 3: 'ppmv'.

Line 423: Better ':' instead of '.'.

Line 452: Improve wording.

“This provides improved conditions for aerosol formation and growth, which further benefits the chemical ozone destruction” changed to: “The colder conditions promote aerosol formation and growth, which further enhances the chemical ozone destruction.”

Line 484: Typo.

Line 569: Typo.

Line 622, 656, 665: DOI or link?

DOIs are not available for these items, but URLs have been added.

Line 739 and 742: Provide links to electronic version or DOI, available at least for WMO (2019).

Reviewer 2:

General Comments

The manuscript by Jörimann et al. uses multi-model atmosphere-only simulations from Phase 2 of the Chemistry Climate Model Initiative (CCMI-2022) to analyse the middle-atmosphere chemical and dynamical impacts from a prescribed enhancement to the stratospheric aerosol layer (senD2-sai) relative to a fixed time-invariant stratospheric aerosol layer (senD2-fix). An additional experiment is also included, in which the enhanced stratospheric aerosol layer is only coupled to the chemistry scheme (senD2-chem), thus enabling dynamical and chemical responses to be isolated.

The study finds that the global mean total column ozone response to the sai scenario is generally small (~10 DU), although there is heterogeneity in response with respect to both latitude and altitude. There is also evidence of strong dynamical heating, changes in tropopause pressure and temperature, and zonal winds. There are also sai-driven responses in age of air, distributions of long-lived greenhouse gases (e.g., N2O) and the partitioning between reactive and passive species for NOx, HOx, and ClOx that drive ozone depletion. Using the sensitivity experiment, the “chemical” responses in ozone from the stratospheric aerosol layer are small and that the bulk of the ozone response is due to dynamical changes.

The manuscript also makes recommendations for improving how the stratospheric aerosol layer could be implemented to improve consistency between the models.

On the whole, I found the manuscript to be interesting. It was generally well written, with clear figures and a logical flow to the analysis presented. The experimental design itself was well defined and removes one source of uncertainty in modelling responses to an enhancement in the stratospheric aerosol layer. However, some re-organization of the opening sections on the models and experimental setup is recommended – see Specific Comments below. I also thought that there was a missed opportunity in relation to the dynamical responses. Further details provided below. And the closing section could benefit from further discussion about the results and more of an in-depth outlook.

On balance, however, it has the potential to be a valuable contribution to our scientific literature and the community's understanding. I, therefore, recommend that the manuscript be accepted for publication, subject to the authors addressing some of the comments below, noting that there may be challenges with some of the points raised.

We greatly appreciate this reviewer's detailed and comprehensive feedback. Many comments are valuable in improving the quality of the science presented and the readability of the manuscript. We address this review by **answering each *specific* comment separately**, and - where applicable - discussing why we decide for or against working some of the suggestions into our manuscript and to what extent.

Specific Comments

I wonder whether the authors considered running an additional sensitivity experiment to capture the responses due to the radiative effects of the stratospheric ozone layer, i.e., “dyn”. The abstract mentions non-linearities and one question is whether the full response is equal to the sum of the chemical and dynamical responses, i.e., non-linearity in the modelled responses.

This is a logical follow-up analysis that we have brought up in discussion and we agree that, indeed, this would be necessary to quantify and fully explain the nonlinear effects. However, with the amount

of material presented in this study and the necessary commitments needed from the modelling centers, we view the proposed extension as subject for an additional, separate study to be conducted in the future. Depending on the availability of resources this potential future work might also differ in its methodology from our manuscript. If only one single model would provide a "dyn-only" simulation, a single-model study might be conducted, which would allow for a very detailed analysis of involved processes.

I think the manuscript would benefit from a re-organization in relation to the models involved in the study and the experimental setup. I'd suggest that you start off with the models, then introduce the generation of the time-evolving stratospheric aerosol layer from a coupled WACCM simulation, and then move on to describe the experiments. I think this would make the descriptions of senD2-sai and senD2-chem easier and would help to avoid multiple repetitions about the WACCM-produced dataset (e.g., in Section 2.1.3).

We decided to keep the main structure but move some items to avoid repetitions and address the points the reviewer is making. For instance, information about the data available from each model is now included in the model descriptions. Generally, we place the experimental setup directly after the introduction, because it explains, how this experiment minimizes the inter-model differences in implementation and how the analysis isolates individual components of the signal. Then the model descriptions can provide the relevant details for the type of analysis that has been described before. The downside we see is that the WACCM-produced dataset is introduced before the model, but seeing as it is not the same model version, this could prevent the reader from assuming so after having gone over the WACCM model description. We update Section 2.1.3 to avoid unnecessary repetitions, where possible.

It would also be good to make clear which models ran which experiments. At present, most model descriptions include how photolysis rates are calculated and whether they include the effects of aerosols on photolysis rates but this hasn't been described consistently across all models (e.g., WACCM). While discussion around this point has focussed on its relevance to those models that have run the senD2-chem simulation, it is also relevant for the responses from the senD2-sai experiment.

The models that ran senD2-chem are listed in the subsection "senD2-chem", but we acknowledge that additional information in the model descriptions would be useful to the reader. We therefore explicitly mention all the simulation data that are **missing** from the "full suite" in all the respective descriptions. Indeed, the aerosol-photolysis effect is relevant for both senD2-chem and senD2-sai, so we describe whether the model includes it or not for each of the five models for completeness.

With those models that use pre-computed photolysis rates, there is a missed opportunity to assess the chemical effects of the prescribed stratospheric aerosol layer, both including and excluding the effects of the aerosol on photolysis rates. How feasible would it be to set up such an experiment?

We agree that such sensitivity experiments would be useful to better understand inter-model differences, as stated in our "Synthesis and Outlook" section. We added this particular assessment of the inclusion of the aerosol-photolysis interaction to this section, as it was not included before. It should be noted that perhaps such an experiment might only be feasible in some models, and not necessarily comparable across models without major efforts in model development. Models that use a look-up table do not have a straight-forward way to add the aerosol effects to their clear-sky photolysis rates, as they are a function of overhead column ozone, solar zenith angle, surface albedo and pressure. Adding the aerosol as an integrated quantity, i.e. stratospheric AOD would not capture the aerosol distribution with height very well. Therefore, a single-model study with a suitable model could provide the better test bed than a multi-model intercomparison..

In Section 3.5, it would be good here to link the positive anomalies in ch4 and n2o abundances with the previous analysis on age of air. There appears to be some correlation between the strength of the ch4 and n2o anomalies with the age of air responses.

We agree with that and add this link more clearly to the text:
The strength of the CH4 and N2O anomalies is very well anti-correlated with that of the age of air anomalies in the three models that provide them (Fig. 2), but this is not the case for H2O, which confirms that an additional process acts on H2O, while the other two species are more tracer-like.

I think the section on synthesis and outlook in the manuscript ended quite abruptly and I guess that I had been expecting more of a discussion and outlook. For example, there was no discussion about potential non-linearities and the focus seemed to be more about how the aerosol forcing was prescribed rather than model responses. The abstract highlighted that “significant nonlinearities from feedbacks between chemistry and dynamics, highlighting where model development and sensitivity experiments are most needed”. However, I thought that there could have been further discussion about this. A clear conclusion with a vision for next steps and relevant research gaps would be of some benefit to the community.

We thank the reviewer for the list of technical comments. **Where no answer is given we adopt these comments as suggested**. Otherwise, we address the point in question individually.

Technical Comments

Line 54: The acronym SADs is used before it is defined

For readers not familiar with the simulations in CCMI-2022, the heading “refD2” is not very informative. How about something like “Heritage of Reference simulation senD2-fix”? This would provide a better linkage to the preceding section on this study’s model simulations and still allows you to introduce refD2 and how it relates to senD2-fix.

This is a good point. Since refD2 is used in Table 1 we change the heading to "refD2 (CCMI baseline future projection)" to still include the name refD2 itself. To make the relation to senD2-fix clearer, we added "(reference simulation)" to the senD2-fix heading.

There is some repetition in Section 2.1.2 in relation to SSTs/SIC and the prescribed stratospheric aerosol in senD2-fix

These parts were rewritten to be more concise.

The term WACCM is used without giving the model its full name (Line 88)

Line 126: Missing closing bracket

Lines 128-133: I think some of the phrasing in this portion of the manuscript is clumsy. Here are some examples of suggested changes.The phrasing “the stratospheric aerosol is considered to be fully radiatively inactive” could be replaced with “the stratospheric aerosol is radiatively inactive”. The sentences “Neglecting the aerosol effect on photolysis would therefore not just separate the "chemical-only" effect in senD2-chem, but more specifically, the "heterogeneous-chemistry" effect. The complete chemical effect, in turn, is separated by still accounting for the photolysis effect, while

keeping the radiative aerosol properties at background (2025) levels" are other examples. How about "Neglecting the aerosol effect on photolysis would therefore only isolate the "heterogeneous-chemistry" effect of the aerosol. However, the full chemistry effect, in principle, should also account for the photolysis effect, while keeping the radiative aerosol properties at background (2025) levels." The sentence "In practice, not every model can easily choose between simulating senD2-chem in one way or the other." could be replaced with "In practice, the implementation is challenging and hence differs across the model ensemble."

Thank you for these writing suggestions. We accept them and add some more rewriting to this paragraph, and detach it from the senD2-chem subsection, as the considerations about photolysis adjustment to the aerosol concerns the entire experimental setup.

Line 147: Maintained at what level? Specific to a particular time period? Please make clear.

Lines 150 and 152: "as a function of"

Line 161: Add full names for DJF and JJA and indicate that they represent Northern Hemisphere winter and summer.

Figure 1 caption: I think it would be better here to indicate that this is the forcing applied in the models or the output from the coupled WACCM simulation that generated the stratospheric aerosol outputs, rather than state that it's an output from senD2-sai. Figure 2 is more about verifying that the models have correctly interpreted the forcing and reflects model output from senD2-sai (Opening lines from Section 3.1).

This is true, only to stay precise we added the statement that this output is near identical to the prescribed input, which is then the point of this figure: to show one of the forcings applies in the models.

Section 2.2; It would be useful to indicate how the timeseries of stratospheric aerosol compares to scenarios used in the Geoengineering Model Intercomparison Project (GeoMIP), for example.

This has been added in the form of a comparison with the G6-1.5K-SAI experiment within GeoMIP.

Line 179: Correct spelling for version (i.e., version)

Line 181: Did you really mean "solution species"? It isn't a term that I've come across before in the context of chemistry-climate models!

Indeed, this is verbatim from the model description, it refers to aqueous chemistry.

Line 187: change "configurations used here run" to "configuration used here runs"

Lines 203-204: Change "which can result in too large reaction rates under in-situ low temperature." to "which can result in reaction rates under in-situ low temperatures to be too large."

Line 215: Is there any need to include refD2? Can you add senD2-chem here?

As the reviewer suggests, refD2 can be omitted, senD2-chem is added instead.

Line 225: Change "resolution 0.7 km" to "resolution of 0.7 km"

Line 226: Change “the QBO” to “a QBO”

Line 252: The term “Junge layer” was new to me – could you add a brief sentence explaining what it is? It might be useful for other readers!

We decided to reword this entirely, as Junge layer typically refers to the persistent natural sulfate aerosol layer, however, here we deal with a mainly anthropogenic aerosol layer, so the heating is only specified as being situated in the lower stratosphere.

Line 278: Change “different extra-tropic latitudes” to “different extra-tropical latitudes”

Line 281: Correct “in the the annual average”

Lines 282-284: Two of the models (CMAM, MIROC) exhibit little variability on a decadal to multi-decadal timescales, and hence, do show positive anomalies (at the 10-20 DU level) in the Arctic. Please alter wording.

This has been reworded with some additional elaboration of the Arctic and Antarctic TCO trends.

Line 299: Although the preceding line refers to analysing senD2-chem, the opening line of this paragraph is referring to the temperature response in senD2-sai (i.e., 5 model responses shown in Figure 4). I think this should be made clearer.

This is a much appreciated comment and should make reading a lot easier here.

Figure 4: Please try to avoid having the “N/A” age of air panels for the models NIES and WACCM – if you can have them completely “whited out”, I think that would be better.

Agreed.

Line 310: I think this is the first occurrence of “SH” and should be defined in full along with the abbreviated form. Same for BDC on line 314.

Line 312: Changed “ideal” to “idealised”

Line 329: Change “observed” to “simulated”

Figure 5: It has a mix of timescales (2069-2079 in panel a and 2030-2043 and 2070-2083 in panel b). I wonder whether the time periods considered could be made consistent and two time periods plotted for the tropopause pressure anomalies.

This change can easily be accommodated to match the two panels more closely. We keep a single set of tropopause pressure anomalies, however, because they display the anomaly when it is largest and smaller anomalies in the same plot would clutter it.

Line 333: Move the word “well” to the end of the sentence.

Lines 345 and 347: Change “percent” to “percentage”

Lines 349/350: Change “hydrogen monoxide” to the “hydroxyl (OH) radical”

Line 364: Change “to play” to “play” and change “hydrogen oxide” to “odd hydrogen”

Line 375: change “from water” to “from the water”

Line 391: Please change to “nitric acid”

Line 424: Region 1 here is in the tropics. Suggest that you change “lowermost stratosphere” to “tropical lowermost stratosphere”

Section 3.7: I really liked this section and how the interpretation of the ozone anomalies was broken down into regions, with the section text clearly linked with the annotated regions of the atmosphere on Figure 9.

Thanks!

From Line 465: I would suggest that you break up this section into a number of paragraphs. For example, one break could be the start of presenting the results for the tropics, i.e., “In the “full” ….”. A second breaking point would be the switch from tropics to southern latitudes.