Response to reviewer 1

Reviewer comments are in bold, authors' responses are in blue. Yellow: to do

In this study, the authors investigate how stratospheric aerosol intervention (SAI) using SO2 injections at different latitudes affects the aerosol distribution, aerosol optical depth, and surface climate (temperature and precipitation) in three different Earth System Models. The authors find differences between the models and also between different aerosol setups in the same model. The authors then describe the development of feedback algorithms to be used in future simulations to manage injections of SO2 to meet temperature targets as the runs proceed. In general, the paper is clear and easy to read, and the analysis is logical. The results will be of interest to the geoengineering community and the paper is well-suited to ACP. I have two main comments that I suggest the authors consider before publication:

We thank the reviewer for their helpful and supportive comments. We have addressed all points below and modified the manuscript accordingly.

Given the focus on aerosol microphysics driving differences between these results, can the authors highlight how the aerosol schemes differ between the models, not just in terms of the modal properties (Table 1), but how the aerosol processes are treated? It's mentioned on L145 that condensation is treated differently in GISS, but how? Given the differences found for effective radius, it would be useful to show (perhaps in the SI) some of the other aerosol metrics such as SO2 conversion (highlighted on L419 as an important discrepancy), the nucleation, condensation and coagulation rates, and fluxes between the modes, and explain how these parameterizations differ between the models. Can we learn more here about these uncertainties compared to multi-model volcanic eruption studies that have already shown that differences in AOD are due to different microphysical schemes? What specific areas of improvement have been found in this work as stated in the conclusions at L395?

Thank you for the insightful suggestion. We have updated the manuscript expanding on models' differences in terms of their microphysical schemes. The models unfortunately do not output many of the requested variables, so it is difficult to ascertain exactly which process is the dominant contributor to uncertainties. Nevertheless, all of these schemes have been validated to some degree against volcanic eruptions (see references in the text).

Section 5 - without going back to previous references (such as Kravitz et al., 2017), this section was hard to follow, especially for someone not familiar with such feedback algorithms. It would be useful if the mathematical relationships were described further in the text and that all letters and symbols were defined and listed immediately after the equations – for example, q and equations for T0 – T2. It would also be helpful if the section was more explicit with signposting to the relevant subplot or line on Figure 10 – e.g., expanding L374 to 'pattern 'of AOD' similar to the target (black dashed lines)', or

similar. It was also not clear to me how the feedback algorithms are different/similar to previous work and what the implications are.

Based on this comment from both reviewers, we have modified Section 5 to further clarify all aspects of this portion of the work.

Specific comments

Abstract: an extra sentence at the end summarizing the overall implications of this work would be useful. It was also not clear whether all models included modal aerosol microphysics schemes. I would suggest introducing the 4 model setups at the start

We have added a phrase at the very beginning of the abstract stating:

"Here we present the results from the first systematic intercomparison of climate responses in three Earth System Models where the injection of SO2 occurs at different latitudes in the lower stratosphere: CESM2-WACCM6, UKESM1.0 and GISS-E2.1-G. The first two, and a version of the third, use a modal aerosol microphysics scheme, while a second version of GISS-E2.1-G uses a bulk aerosol microphysics approach."

And a phrase at the end:

"In conclusion, we demonstrate that it is possible to use these simulations to produce more comprehensive injection strategies, large differences in the injection magnitudes can be expected, potentially increasing inter-model differences in the stratosphere while reducing surface ones."

L30-L35: A few more relevant references could be added here e.g., Zanchettin et al. (2016; 2022).

Thank you for the suggestion! We have added these references.

L82: It was unclear at this stage what these targets are

We have specified these are the same targets we referred to in the previous paragraph.

L121: What are the differences in the aerosol scheme?

We have tried to discuss more in depth some of the differences between the schemes.

L149: Please clarify what you mean here

We meant that the value reported in the paper has been subsequently updated (but no publications have reported it). We have added a “used in the current version of GISS used here” to the phrase.

L181: Why AOD and not SAOD? Please also describe the overall evolution of this figure – i.e., the ~2 years of adjustment and therefore why the last 7 years are used in subsequent averages

We have fixed the figure by modifying the title and adding a dashed line to indicate the considered steady state and explained it in the caption.

L235: How is the lifetime defined? Please remove ‘obviously’

We have modified the phrase as follow, for clarity:

“Finally, panels f-h in Figure 4 give an overview for all cases of the ratio between the overall mass of the produced aerosols (shown alone in panel 4f) and the injected amount of SO2. This ratio, shown in panel 4g, represents the lifetime of the added sulfate: as we are in a steady state, where no new mass is added to the global burden, this lifetime can be calculated as the burden divided by its constant source (the injection) (Visioni et al., 2018)”

L253: Is this wet or dry radius? Fig. SX --> Fig. S1.

That’s wet radius. Thanks for spotting the mistake!

L263: What are the dynamical differences? Do the authors have an explanation for the stronger poleward transport in CESM given also the differences in particle size? Why is the transport in the 15S case more similar to GISS modal?

The dynamical differences are discussed in depth in the companion paper. We have added further clarification in this phrase:

“... these differences are discussed more in depth in PART2 and indicate a substantially stronger climatological shallow branch of the Brewer-Dobson circulation in CESM2 compared to other models (see Figure 2 in PART2)”

L279: The initial results shown between the global mean AOD and global mean temperature in Figure 1 could also be discussed here.

Thank you for the suggestion, we have added a comment about Fig.1 here.

L289 – L305: I found this hard to follow. Has the sensitivity to aerosols in GISS been increased or not?

The reviewer is right and the phrase was incredibly confusing. Apologies. We have rephrased below:

"As noted for Fig. 1, this large normalized difference is mainly due to differences in the global cooling produced between the two GISS realizations (the sensitivity to the aerosols amount); even if there is a much smaller amount of AOD simulated in GISS modal, it results in a similar level of surface cooling as is simulated in GISS bulk under much larger AOD values."

L310-311: Unclear exactly what you mean here

We have modified the phrase:

"In contrast, the models tend to disagree with respect to responses in the NH high latitudes much more than for the SH high latitudes; this can be explained by the role of dynamical atmospheric variability and sea-ice variability, which in the NH both contribute to the overall response increasing model disagreement"

Figure 7: What's causing the different response for CESM2 and UKESM for 30N compared to the other injection locations?

The differences in Fig. 7 for 30N appear to be in line with those at other injection locations, so we're not sure what the reviewer means here.

L314: I would suggest moving the overall description of the precipitation changes from the second paragraph to here as it is a long time before the results are described. What about the global percentage changes shown in Figure 8?

Thank you for the suggestion, we have done so. We have also added a phrase for the % changes:

"We also show \% in order to highlight the significance of the absolute changes based on the overall received precipitation. This for instance helps to show that, compared to the baseline values, the very small changes observed at high latitudes may also be significant."

L336: shown on left hand side of Figure 8?

Thank you for the suggestion, added.

L338: There are several newer studies on the impact of eruptions on the ITCZ that could be cited here. Please see Marshall et al. (2022) for some examples.

Thank you for suggesting this, we have added some more recent references here.

Figure 10 caption: Please explain what L0, L1 and L2 are and label the black dashed lines.

Done.

L346: I think it would be helpful to state what these are here, as is done in the conclusions

Agreed!

L399: This paragraph focuses on the methods, but what are the actual results? How do the results differ depending on the injection location?

We have included a final paragraph discussing this issue: We report it here:

"*Overall, the results shown in this section confirm that the methods applied in MacMartin et al. (2017) to produce the results in Kravitz et al. (2017) may be applicable to other climate models as well, and that the inter-model differences in transport and microphysics can, in this kind of optimization strategy, be overcome by modifying the ratio between injection amounts at different locations. However, it is also clear that, as indicated by the differences in the residuals shown in Fig. 10, the use of the specific four latitudes proposed in MacMartin et al. (2017) might not result to be the most optimal set of latitudes in all models, depending on the specifics of the stratospheric circulation. The results of UKESM for instance suggest that, in that model, the 15◦N/15◦S are too close to the tropical pipe and therefore still confine the aerosols too much at low latitudes, as opposed to CESM2 and GISS. Therefore, based on UKESM results, a choice of latitudes further from the equator (such as, for instance, 20◦) may result in a better optimization. Similarly, all models seem to suggest that the successful obtainment of L2 might be hard to achieve with only 30◦ injections, and that some higher latitude injections might be necessary. It is thus worth reiterating that the specific choices of latitudes here should not be read as the only, nor optimal, choice, but merely as a way to start exploring the available space (see also Zhang et al. (2022) for a broader exploration of this issue)*"

L447: This last sentence is difficult to follow

Thanks, we have changed it.

Technical corrections

We have fixed all of these, thank you!

L3: occurs

L23: please add numbers for the three items in this list
L69: only --> one
L71: a --> the
L84: impacts
L86: in --> with?
L120: eruptions
L208: shows
L209: standard deviations
L211: check commas
Figures: please check all x and y labels are present (missing from 2, 6 and 8) and remove red/green line combinations

We have changed the colour schemes and improved all the figures also based on other feedbacks.

L312: 2020 --> 2021
L318: clouds --> cloud
L320: is --> are
Figure 8 caption: five --> seven
L441: insert 'than'
L444: seems