

Response to editor comments

Thank you for submitting the revised manuscript which addresses most of the reviewer comments. Regarding the scope and novelty, my decision is that it is sufficient for publication. However, the report #2 by one of the reviewers has several important minor revision suggestions that I agree will be important to implement. Please address these and we can proceed with publication:

AC - We are glad to hear your decision. Here, we respond to the reviewer's concerns. Line numbers correspond to the revised pdf version, whereas [line numbers in blue correspond to the latexdiff file](#).

EC - Title wise, the title doesn't respect the guidelines set out by GMD itself. As a "Development and technical paper", the guidelines state "If the model development relates to a single model then the model name and the version number must be included in the title of the paper. If the main intention of an article is to make a general (i.e. model independent) statement about the usefulness of a new development, but the usefulness is shown with the help of one specific model, the model name and version number must be stated in the title. The title could have a form such as, "Title outlining amazing generic advance: a case study with Model XXX (version Y)". Therefore, a more correct title for this paper would be "A computationally efficient method to model Stratospheric Aerosol Injection using prescribed aerosols in a lower complexity version of the same model: a case study using CESM(CAM) and CESM(WACCM)" which fairly represents what the paper tries to describe.

AC - Thank you for pointing this out. We indeed only show results for CAM-WACCM, requiring a change of title. Your suggestion was very helpful and has been used for the new title, with a minor addition of being able to run both similar and alternate SAI experiments.

EC - Again, I find the beginning of the Introduction not very professional for a scientific paper. The first context-setting paper for climate change is a blog post – when at least you could cite the WMO State of the Climate report for 2024, which has a ISBN and a Permalink and has been peer-reviewed (see <https://library.wmo.int/records/item/69455-state-of-the-global-climate-2024>, <https://library.wmo.int/idurl/4/69455>) two opinion pieces pro and against SRM research and continues being vague with qualitative statements ("Arguably", "Seems to combine", "highly controversial"...).

AC - Yes this is a fair point. We have limited this paragraph to introducing SRM and SAI in a more neutral way, i.e. without touching upon the ongoing debate around its potential usage or its effectiveness (L15–21,[L16–27](#)). The blog post is now replaced by the WMO State of the Climate report (L16,[L18](#)).

EC - As another note, the Feder et al. (204) paper that is cited hasn't been accepted for publication, it's not In Review anymore.

AC - We have removed the Feder et al., 2024 reference (L29–30,[L36](#)), but it is hard to find a substitute. However, this proves the point of having very few studies with high-res SAI simulations. Also, the Farley et al. (2025) has not yet been accepted and replaced by a similar older paper: Farley et al. (2024) (L463,[512](#)). The Jörimann et al. (2025) paper has been accepted and changed accordingly (L474,[L527](#)).

EC - L. 35: why not just say "a method to approximate" rather than the awkward and imprecise "(approximately) model"

AC - That sounds more accurate indeed. See changes in L33,[L39](#).

EC - L. 216: "rough dry-testing" is not a scientific term, nor is it something that would mean anything to a reader.

AC - We performed some idealized tests with the feedforward-feedback controller in which GMST was determined using the FAIR climate model emulator (Millar et al., 2017) and random noise. We mention this in L193–195,[L202–205](#).

EC - L. 263–265: aside from this phrase being needlessly convoluted, it is also way too generic. Using

the current scenario of injections (excluding higher-latitude injections), in WACCM almost no land areas have residual temperature changes “on the order of a few degrees”. At most you can see areas where the residual is below 1C (absolute), and this can be better quantified, especially in the differences between CAM and WACCM (that might be due to the fact that the injection pattern in WACCM might not be the same in CAM to reduce those residuals).

AC - Yes we agree and have removed this phrase. To improve the quality of this section, we now indicate more clearly about what differences (e.g. inter-model or SAI minus Reference) we are talking, use more accurate numbers (e.g. typical inter-model differences much less than 1°C), noted a few thoughts on the warming bias and describe the seasonal variations a bit more (L246–264,L257–280).

EC - L. 309: why is potential temperature included, and what is the significance of that compared to air temperatures for the LS? Potential temperatures is less related to the heating rates increases due to sulfate absorption, and normally just air temperature changes are analyzed in other works looking at stratospheric response (see the Bednarz paper cited after). So if the authors want to shot PT rather than air temperature, they should justify why.

AC - Potential temperature has been included as it makes thermal wind balance calculations in pressure coordinates easier. However, it is true that plotting temperature is the more sensible thing to do here. This has been changed in Figs. 7,A5 and the accompanying text (L309,311,313) (L325,327,329).

EC - L. 334: the CFC dependence has been analyzed much more in more recent works, such as Tilmes et al. (2021)

AC - Yes this is a great resource. Tilmes et al. (2021) suggest that the combined effect of available chlorine concentrations and aerosol size lead to a peak in ozone decrease per injected amount in the initial decade after starting SAI, leading to limited further ozone decrease in the subsequent decades. We figured the section about ozone is better suited for the Discussion and moved most of it there. We refer to this work in L411 L460.

EC - The new Discussion/Outlook section is also left entirely vague (“a vastly different forcing scenario”, “generally much closer”, “qualitatively similar” etc.). Please quantify some of the outcomes of your paper. Due to its length (and to its lack of clarity), it would also be useful to separate the Discussions vs Conclusions sections explicitly.

AC - We agree that some of the outcomes can be quantified better. On top of that, we think the aims of the paper could be stated more clearly. We made improvements by 1) quantifying some of the changes in T0,T1,T2 (new Table 4 in Results) and 2) relating inter-model changes of key variables to their interannual variability through a performance index (new Figure 8 in Results, introduced in section 2.4 in Methods). These indicators of performance are mentioned in the aims of the paper (L71–77) (L78–84), displayed in Results (sections 3.1.1 and 3.4 (mind numbering error in latexdiff)), used in the Discussion (L350–361,L378–379,L383–387,L400–404) (L380–392,L416–418,L427–430,L450–453). We have also structured the previous Discussion/Outlook section in a Discussion (with a part on validation results and a part on tested use cases), Conclusions and Outlook.

References

- Farley, J., MacMartin, D. G., Vioni, D., and Kravitz, B. (2024): Emulating inconsistencies in stratospheric aerosol injection, *Environmental Research: Climate*, 3, 035 012, <https://doi.org/10.1088/2752-5295/ad519c>
- Jörmann, A., Sukhodolov, T., Luo, B., Chiodo, G., Mann, G., and Peter, T. (2025): REtrieval Method for optical and physical Aerosol Properties in the stratosphere (REMAPv1), *Geoscientific Model Development*, 18, 6023–6041, <https://doi.org/10.5194/gmd-18-6023-2025>

Millar, R. J., Nicholls, Z. R., Friedlingstein, P., and Allen, M. R., (2017). A modified impulse-response representation of the global near-surface air temperature and atmospheric concentration response to carbon dioxide emissions. *Atmospheric Chemistry and Physics*, 17, 7213–7228, <https://doi.org/10.5194/acp-17-7213-2017>

Tilmes, S., Richter, J. H., Kravitz, B., MacMartin, D. G., Glanville, A. S., Vioni, D., (2021). Sensitivity of total column ozone to stratospheric sulfur injection strategies. *Geophysical Research Letters*, 48, e2021GL094058, <https://doi.org/10.1029/2021GL094058>