This manuscript by de Jong at al. is trying to do two things: first, it is trying to describe how to prescribe stratospheric aerosols forcing in a different model. Second, it's trying to show the surface climate differences in two different CESM1 versions (CESM2-CAM6, CESM1 and CESM2-WACCM6) forced by the same aerosol fields (prescribed, in the case of CAM).

The main thing I take issue with, in the first part, is that essentially what they are proposing to do is not novel at all. Prescribing an aerosol field externally is something that has been done before multiple times not just for volcanism (the entirety of CMIP6 historical simulations used prescribed aerosols) but for SRM, too. Conveniently, the authors leave out all the references of this, trying to pass it off as "novel". Personally, I deeply dislike this way of not acknowledging past work (or to avoid performing a literature search that would have made past works emerge) to hype up one's own.

Here are three main examples:

- *Tilmes et al. (2016) proposed a prescribed aerosol forcing file for models with no interactive sulfate cycle. Such forcing field was then used in Xia et al. (2017) and in one of the models for G6sulfur (CNRM) as described in Visioni et al. (2021).*
- *Still in G6sulfur, the model MIP prescribed their own aerosol field and scaled them up in their two fully-coupled versions, also as described in Visioni et al. (2021).*
- *Finally, Tilmes et al. (2024) proposed a new experiment for CCMI with a new aerosol field for SAI, also describing climatic differences in WACCM when fully interactive vs prescribed aerosols are used, with much more details provided on how to prescribe the aerosols fields in different models provided in Jörimann et al. (2025). Reading Jörimann et al. (2025) could also illuminate for the authors how hard it is to prescribe aerosol fields in other models' version, as one needs to get optical properties that might be treated differently in different models with different ways to translate aerosol size distribution to forcing.*

None of this is ever acknowledged in the (rather short and non comprehensive in general) cited literature, giving the reader the impression that this is the first time something like this has been attempted (cue the word "novel" used frequently in the text, and also in the Key Points).

So, what's novel? The use of a scaled-up, mono-dimensional control algorithm (not that different from what was used in multiple G6 models, see above) used in multiple publications starting in MacMartin et al. (2014)? That's hardly new – and the simplified controller used here hardly seem well tuned, considering the pretty large errors shown in Fig. 2!

We thank the reviewer for the extensive comments and literature suggestions. However, we suspect there was a misunderstanding about the scope of our work, and in particular, what aspects thereof we consider to be "novel". At no point has it been our intention to pretend that we came up with the idea to force models with aerosol fields, either directly or scaled similarly as in previous work, and it never occurred to us that our text might be read in this way. Reviewer 1 seems to confirm that this is a reasonable assumption.

Instead, our paper intends to present and test a useful protocol for a specific application of forcing climate models with aerosol fields, namely, mimicking GLENS / ARISE-like SAI simulations in situations where running a full stratospheric chemistry model is impractical, for example due to computational constraints. For this specific application, our method is novel to our best knowledge, and modellers from several groups expressed interest in using it.

The submitted manuscript aims to disseminate our protocol and outline potential avenues for future use, which fits within the stated purposes of GMD ("development and technical papers, describing developments such as new parameterizations or *technical aspects of running models*" and "model experiment descriptions, including experimental details and project protocols".

We will take care to clarify our intentions. We will pay particular attention to reformulating the abstract, which we agree can be read as if the scope of our work is broader than it is. Please find further specific changes we intend to apply to the manuscript below.

Relation of our protocol with previous work

From preliminary tests we found that simply inserting WACCM-derived aerosol fields in CAM will lead to rather different temperature outcomes (i.e., an aerosol field taken from WACCM cannot stabilise GMST in CAM). Such a forcing can be scaled manually, but this can be cumbersome, especially when running non-standardized SAI forcing scenarios. Hence, for our application of mimicking WACCM forcing in CAM we explored the possibility of using a feedback controller combined with a new method of scaling. Our protocol builds on two prior strands of work:

- SRM simulations with feedback controllers. For us, SAI simulations with a full-fledged chemistry model are most relevant, although feedback controllers have also been applied on simpler simulations and are currently being applied to Marine Cloud Brightening. Examples involving SAI include the simulations with CESM(WACCM) for G6sulfur (Visioni et al. 2021), GLENS (Kravitz et al., 2017), and an inclusion of the same feedback controller in the ARISE-SAI protocol (Richter et al., 2022).
- Prescribing (stratospheric) aerosol forcing to models that do not simulate them explicitly; for us, applications in SAI are the most relevant, but others exist (volcanic eruptions). Examples include the CNRM and MPI-ESM models that participated in G6sulfur (Visioni et al., 2021), which may be forced by the aerosol forcing set provided by Tilmes et al., 2015 (CNRM) or a similar set produced by the same model (MPI-ESM) and scaled by means of choice, CESM(CAM-chem) (Xia et al., 2017), and an inclusion of using prescribed forcing data in an SAI protocol for CCMI (Tilmes et al., 2024). Regarding this protocol, the REMAPv1 code (Jörimann et al., 2024) is proposed as a viable method to calculate aerosol properties if the input dataset does not contain the fields required by the model.

Our method adds to the existing literature in two ways:

- Most importantly: Combining a feedback controller with aerosol forcing
- Allowing for non-linear relationships between AOD and other aerosol variables

Combining a feedback controller with aerosol forcing. In the literature mentioned above and suggested by the reviewer, we have not encountered a publication documenting the combined use of a feedback controller and prescribing aerosols.
Tilmes et al. (2015) provides a predetermined forcing scenario, which is directly prescribed to the model in Xia et al. (2017). Both CNRM and the two versions of MPI-ESM in Visioni et al. (2021) scale such prescribed forcing by multiplying the AOD with some year-dependent factor to achieve the temperature target. Automatising the feedback controller - as has been done in e.g. GLENS and ARISE with full aerosol models - obviously can save considerable time, especially when exploring new scenarios in the simpler model setup (as out SAI2080 in CESM-CAM).
In short, one original contribution of our paper is simply to have worked out a practical way of combining these previous strands of work in order to allow SAI simulations in the (computationally cheap) CAM to mimic SAI simulations in WACCM as closely as possible in terms of (tropospheric / near-surface) climate impacts, here using GMST as target variable - and to document it sufficiently clearly

(including some practical details such as the iteration to get a sufficiently good feedforward) that other groups can hopefully save themselves time and errors if they wish to implement something similar.

Nonlinear relationship between AOD and other variables. Apart from using a feedback controller for scaling aerosol forcing - more precisely, AOD - we also use non-linear fits to relate other variables such as aerosol mass, size and surface area to AOD. As far as we can see from the literature, this was not the case in previous studies where scaled aerosol forcing was used. A consequence of this approach is that aerosol field properties may not change when scaling the forcing strength. Tilmes et al. (2022) for example find that a linearly increasing forcing strength for CNRM in G6sulfur resulted in a linearly increasing SAD because of the fixed aerosol size, which is most likely dependent on the forcing strength (Niemeier et al. 2011, Niemeier and Timmreck 2015). Though the relationship between AOD and the dominant mode aerosol mass is fairly linear in our simulations, other variables show different relations (fig. 1 of the manuscript).

Translating aerosol fields for use in different models The prescription of aerosols into models that require different aerosol properties than provided requires the use of Mie calculations as done by the REMAPv1 algorithm developed by Jörimann et al. (2025). In our analyses, we did not need such translations as WACCM output may be directly prescribed to CAM. Our method can therefore be applied by anyone using a compatible pair of models, regarding the stratospheric forcing, but not when different input is required. In our revised version of the manuscript, we will stress that we have not validated the case for non-matching input fields, e.g. with a less general description in the abstract. It might be possible to combine the REMAPv1 algorithm with our method if one needs to translate between incompatible pairs of models (i.e., the model receiving forced aerosols requires different inputs than the model generating the forcing). We will mention this in the discussion.
Summarizing, the proposed method uses an improved scaling not seen before in prescribed-aerosol SAI modelling and can easily be combined with existing feedback controllers. This makes it a valuable tool and source of inspiration for modelling groups that will perform their own SAI experiments (focussed on impact).

We will add the above discussion on prior works and how our work relates to them to the revised manuscript.

The second part then offers a description of surface climate differences between CAM and WACCM. This part (which would also not be particularly fitting for GMD on its own) is awfully lacking as well.

First, I don't see any kind of discussion of the biases between the two models' versions without SAI, and I think that would be a fundamental starting point to understand the sources of differences to a forcing. Even very similar versions of CESM2-WACCM6 that differ by the inclusion or not of tropospheric chemistry can present relevant distinctions in some modeled trends (see i.e. Davis et al., 2023), let alone two basically different models. Aside from surface climate, most of the differences in the stratospheric response are also left very vague (for instance, Section 3.3 essentially doesn't offer anything more than guesses ("These features occur in Control as well and are assumed to be model differences") that don't really provide much confidence to the reader that the authors know what they're talking about. The entire paragraph from l. 376 to l. 384 is very high level, does not cite any of the relevant literature about the impacts of stratospheric ozone changes on surface climate (to begin with, but the list is very long, see Bednarz et al., 2022).

I do not wish to claim that novelty in itself makes a study worthy of being published, and actually I think quite the contrary: but a study that wishes to claim novelty as its main strength, fails to deliver and also ignores relevant literature that would help frame this work in the broader context (out of cu-

riosity, the manuscript has 24 references, of which 7 are in the first paragraph to prove/disprove the permissibility of SRM research or to discuss climate change, one of them is a non-peer reviewed link to CarbonBrief, and one a never accepted preprint from 2023, casually including one of the authors of this piece) is setting itself up to fail for anyone who has any familiarity with the field.

Literature review - additional points

We thank the referee for pointing out the Bednarz et al., 2022 paper, which can serve as a useful comparison case.
We would like to keep the initial, more general remarks about the controversies around SRM, because we believe that even an otherwise technical paper should provide a minimum of context regarding SRM's controversies and dilemmas. The Futerman et al., 2023 paper is accepted and we expect to have the link to the final version by the time this manuscript is published. The Pflüger et al., 2024 reference is very relevant because the same forcing method is used and is correctly referring to a published peer-reviewed paper.

Validation

We now address concerns raised by referee 2 regarding the quality of our validation. What we present is a tool that essentially emulates the stratospheric aerosol forcing in a model that does not have stratospheric chemistry. The purpose of its validation is to show how well the tropospheric climate responds to this applied forcing, so that modellers may decide to use our proposed method or not.
For our validation we used the following metrics (in decreasing order of importance:

- Can the method achieve the set climate target (GMST)?
  GMST can be stabilised to the desired level, which is the main objective of our protocol. This works nicely when reproducing WACCM scenarios (in our case, SAI2020). When generating new scenarios which strongly differ from the WACCM scenarios used as input (in our case, SAI2080), discrepancies occur but these are limited and explainable and can be reduced further by fine-tuning the procedure (in our case, resetting the integrator term of the feedback) .

- Does the method reproduce the response to SAI of the tropospheric climate?
  In particular we verify if (surface) temperature, precipitation and wind responses to SAI in CAM resemble responses in WACCM for equivalent scenarios. The model discrepancies we find are not larger than inter-model difference in the absence of SAI. Inter-model differences are evaluated in Reference and RCP8.5. Please note that the scope of our validation is merely to check whether our method introduces large additional biases (relative to existing inter-model differences in the absence of SAI), not to provide an in-depth comparison between CAM and WACCM more broadly.

- Does the CAM simulation successfully control all degrees of freedom targetted in WACCM, even if not included in CAM's feedback controller? Does this depend on changes in the scenarios (e.g. SAI2080 instead of SAI2020) or model version (e.g. switching to higher resolution)?
  We find that even if we only control for GMST, other targets used in the unerlying wACCM simulations (T1, T2) are fairly well reproduced if CAM is used to generate a similar scenario (SAI2020). The same does not hold for the rather extreme SAI2080 scenario. This discrepancy can be explained by the long-term AMOC changes incurred during the prolonged warming under SAI2080. As we also discuss in the manuscript, this discrepancy could likely be mitigated by expanding our protocol with additional degrees of freedom, i.e. including several aerosol patterns associated with different injection latitudes in WACCM. We agree with the reviewer that it would be interesting to test this. However, for the time being, we are restricted to using for the validation the simulations we have now (which were partly developed for other pieces of analysis), therefore

we cannot do more than discussing this (current) limitation and suggesting an expansion to multiple aerosol patterns as an avenue for future development.

- Stratosphere:
  Our method should not be used by anyone looking for high fidelity in the stratosphere, as mentioned in our abstract and introduction. For such research interest, clearly using WACCM is a more viable approach. Our interpretations on the differences in the stratosphere such as in section 3.3 are considered to be a nice-to-have and do not comprise an in-depth analysis of the mechanisms resulting in the observed patterns of change. They are a simple check whether the limited representation of stratospheric processes - such as atmosphere-ozone interaction or other things that could affect stratospheric heating - should be expected to have impacts in tropospheric circulation (as for example suspected by McCusker 2015)

Regarding the above, we believe that the validation in the current version of the manuscript is fit for purpose. We agree that we should avoid potential misunderstandings about the range of problems out methods should, or shouldn't, be applied to. In the revised version of the manuscript, we will state more clearly that our method is not to be used for prescribing aerosols in SAI simulations where high fidelity in the stratosphere is required, but rather as a tool for simulations meant to analyse the tropospheric/oceanic impacts of geoengineering.

This manuscript, in another venue but GMD, could provide some interesting analyses of what happens when you prescribe the same aerosol field in different model's versions given much more in depth, careful analyses, exploring the sensitivities, performing further experiments (i.e. different scenarios, which are available in CESM2-WACCM6, could be compared with the same prescribed CAM6 results to understand if the differences observed are a function of magnitude, of the specifics of the AOD pattern, of the broader atmospheric response, of the underlying emission scenario; the ozone field could have actually been changed; CAM6 simulations with fixed SSTs could have been performed to understand the actual forcing response; different patterns of AOD from different injection locations could have been combined to understand the linearity of the response, etc. The authors do acknowledge that "It might be worthwhile to expand the method for additional use cases." But, given the paucity of interesting results otherwise, I think they should be the ones, and not somebody else at a later date). In its current form, however, I cannot recommend publication in this venue.

On the additional suggested experiments

A long list of additional simulations and analyses is suggested by referee 2 to develop a deeper understanding of the stratospheric forcing response. We have no doubt that many of these would greatly contribute to the community's understanding of stratospheric aerosol processes and aerosol-induced forcing. However, the scope of this paper is not a detailed research aerosol processes related to SAI or provide an in-depth model comparison, but to present a useful protocol for practitioners interested in (surface) climate impacts. Therefore the suggested fixed-SST experiment, while very interesting for wider research on aerosol forcing, are beyond the scope of this study.

We agree that some other suggested experiments would be nice to add here. For example, as regards additional scenarios, it would be nice, for the purpose of additional validation, to try using CESM-CAM aerosol fields based on a particular CESM-WACCM simulation (e.g. what we call SAI2020 here) to reproduce another existing CESM-WACCM simulation (e.g., ARISE-like scenarios (Richter et al., 2022)). Also, as discussed above, it would be nice to expand our method by using aerosol patterns from several injection latitudes to test for linearity and improve the performance of new scenarios (here SAI2080) on additional degrees of freedom. Unfortunately, we have to make do with simulations originally designed for other research projects. Adding the suggested new simulations would require a very substantial amount of additional computational and human-time effort, which we cannot commit and which do not weigh up against the gains achievable for this paper. We therefore do not intend to perform the suggested additional experiments.

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