Peer review of “Change in negative emission burden between an overshoot versus peak-shaved Stratospheric Aerosol Injections pathway”

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General Comments

This study examines stratospheric aerosol injection (SAI) in the CNRM-ESM2-1+ climate model. Specifically, the study considers the SSP5-3.4 “overshoot” scenario with and without an idealized SAI strategy which limits global warming to approximately 2°C. The authors look at carbon fluxes between the atmosphere and the land and ocean components; because the CO_2 concentrations in the atmosphere are fixed throughout, any change in flux to or from the land or ocean must be balanced by a change in anthropogenic emissions. Therefore, changes to these fluxes represent how much additional (or less) anthropogenic emissions are “allowed” in order to follow a given pathway under a given intervention. The authors conclude that, in this experiment, the SAI intervention allows for approximately 60 Gt of additional CO_2 emission during SAI ramp-up, 30 Gt during peak deployment, and -30 Gt during phase-out.

The study will be suitable for publication with little additional work. The writing is very high-quality; the text is extremely clear, citations in the introduction and discussion are plentiful, and comparisons to other studies are thorough. I also think the paper identifies an important gap in the literature, as most SAI simulations only run for 30-50 years or until the end of the century, while this study follows SAI through peak deployment, wind-down, termination, and post-deployment out to 2250. However, I would like to see more documentation for the study’s methodology, including the process of copying aerosol optical depth (AOD) fields from one model, scaling them up or down, and pasting them into another model, and more acknowledgement that the results may be influenced by the methodology and SAI strategy chosen. I offer specific comments below.

We thank the reviewer for taking the time to go through our work and the constructive feedback. We are pleased to hear that they find the study to be a valuable contribution to the SRM discourse and hope to address their concerns with our modifications.

Specific Comments
Abstract, line 19-20: “SAI is used to maintain 1.5°C warming” - I think this is a typo; the rest of the article describes SAI being used to limit warming to 2°C.

Yes, thank you.

Abstract, lines 20-23 and generally throughout: this study only examines one SAI strategy. While this is perfectly fine (many studies only look at one strategy), the summarized results in the last sentence of the abstract *may* not be applicable to other strategies - e.g., if the initial injection rate were constant instead of ramped up, if the injection happened at different locations, or if the total amount of cooling were different.

Yes, the paper only relates to SAI, hence SAI in the title and everywhere else in the text. We have a short mention in the discussion on the fact that results would likely differ for Marine Cloud Brightening or Cirrus Cloud Thinning. We have added more explanation related to the dependence of the results on the chosen SAI strategy and potential deviations for different deployment designs. We will add a sentence to the abstract (L.20-23) along the lines of ”Results may differ for other injection strategies and deployment design”.

Line 100, “a global mean temperature increase of 2°C” - relative to what? Temperatures in this study are all presented as an increase relative to some baseline (presumably some preindustrial value), but it is never stated clearly what that baseline is or how it is computed. The baseline value, and its definition, should be stated clearly.

We will add “Warming since pre-industrial” to the titles of Figure 1a and Figure 2 and add a sentence on our definition of temperature baseline in the methods.

Lines 116-117, “The amount of AOD was determined with a trial-and-error approach...” - more needs to be said about how the “injection rates” (AOD magnitude) were chosen. The text offers the explanation that AOD was chosen to limit warming to 2°C ± 0.1°C, but injection starts well before 2°C is reached, and all three ensemble members appear to overshoot the tolerance level around ~2115. The curve in Fig. 1b is very smooth, suggesting that a desired AOD curve was fit, and the shape of the curve tweaked rather than the AOD in each individual year (is 1b a 10-year running mean?). Was AOD chosen first for one ensemble member, and then the same quantity used for the other two ensemble members? This is important because the main message of the paper is “SAI which limits warming to 2°C does X and Y to the carbon cycle,” but the magnitude of AOD used doesn’t seem to have been chosen purely based on the 2°C target.

We are going to add clarifications on the design of the SAI scenario. In short: Indeed, the AOD curve was fit. 1b is not a 10-year running mean. The same AOD was used for all members, the only difference in the members is the background state of the SSP585 baseline ensemble members.

Lines 118-119, “The difference in global mean forcing was then translated into spatially resolved AOD using Tilmes et al.’s (2015) G4SSA AOD distribution” - this needs much more explanation. Tilmes, et al. (2015) simulated 8 Tg SO_2 injection over the equator during the years 2020-2070 and presented the AOD distribution for that injection strategy. It sounds like this study copied and pasted that AOD distribution into this model, and then scaled it up or down. This is a substantial idealization, and needs to be discussed more - have other models or studies used the AOD fields provided by Tilmes, et al. (2015)? Has it been done in this model before? Has this model done simulations of SAI or volcanic eruptions? The authors should also mention that this AOD distribution corresponds to equatorial SAI, which is relevant because a.) injections at different latitudes could have different results, and b.) equatorial SAI is less commonly studied now because it is known that the aerosols tend to remain confined to the tropical pipe, over-cooling the tropics and under-cooling the poles.

Yes indeed, the reviewer correctly identified the SAI modeling process using a prescribed AOD distribution. Using the G4SSA AOD distribution is recommended by the GeoMIP protocol for models that cannot dynamically treat sulfur aerosols in the stratosphere (Kravitz et al., 2015) and has been done with this model before (Jones et al. 2022, Chen et al. 2023, Tilmes et al. 2022, Baur et al., 2024a,b). Visioni et al., 2021 compares the models participating in GeoMIP including CNRM-ESM2-1 with the prescribed G4SSA AOD distribution.

The offline calculation of an AOD field is also done by other ESMs (MPI-ESM prescribes its aerosol distribution from simulations described in Niemeier and Schmidt (2017) and Niemeier et al. (2020)).

We are going to include more details on the SAI setup in the text, the latitude of injection in the AOD distribution and its potential impact on results.

Lines 119-120, “A sufficiently well calibrated SAI magnitude is classified as mostly staying in the range of 2°C ± 0.1°C of warming” - is this your definition, or someone else’s?

We are going to add that this is our classification.

Figure 1: it would be very helpful to add atmospheric CO_2 concentrations to this figure somehow, either as a separate panel or as a right-hand-side axis on panel (a). They’re an integral part of the study’s methodology and also play a role in how the three “phases” are defined, but don’t appear to be shown anywhere.

Yes, we agree with the reviewer and are going to add CO2 concentration as a second y-axis.

Lines 144-145, “the results of this study are based on the ensemble mean of the three members except if indicated otherwise” - this should be stated everywhere it is relevant (e.g., in each figure caption) rather than hidden in one place in the text.

We are going to add this information in every figure caption where relevant.

Lines 145-146, “a 10-year rolling mean is displayed on the figures” - is this true for all panels of all figures? If so, this should be stated in each figure caption, rather than buried in the text.

Same as above.

Figure 4b: I recommend adding some kind of errorbar or ensemble spread to this panel.

We will add the ensemble spread to the bars.

Figures 4c-4d: Not strictly necessary, but it would be helpful to the reader to add markers to these panels to denote different points in time - it is not immediately obvious to the reader which end of each curve represents the start of the experiment, what year the “peak” represents, and so on.

Yes, we will add some clarification to these two panels.

Discussion, last paragraph: The authors do a good job of discussing the limitations of using one model and one emissions scenario, but I recommend they also address a.) the idealized nature of the experiment (using prescribed AOD instead of simulating aerosols) and b.) that only one SAI strategy was considered. The same evolution of global mean AOD could have been accomplished with different AOD distributions representing, for example, subtropical or subpolar injection instead of equatorial. Additionally, the 2°C temperature goal could also have been met with other amounts of AOD; for example, in SSP5-3.4-over, the 2°C threshold is reached in ~2050 without any SAI - consider an experiment where you used same emissions scenario but with no injection until 2050, at which point SAI is ramped up very suddenly to maintain the 2°C target. Such a change would probably affect this study’s conclusions substantially, while still claiming similar methodology.

Ok, we will add more scenario disclaimers to the discussion and emphasize that the results are influenced by the chosen methodology and SAI strategy.