

Response to reviewers (2): Efficacy assessment of Stratospheric Aerosol Scrubbing as a Counter Climate Intervention strategy

We thank the Reviewer for their suggestions and have responded below. The Reviewer's comments are provided below in black while our responses are in red and changes made to the manuscript in blue.

1. The scenarios used in this work seem unrelated to the realistic scenarios presented in the introduction, and no interpretation for linking the results with these realistic scenarios is given in the end. It would be good to: a) better justify the choice of the idealised scenarios used in the simulations performed in this work, and b) it would be interesting to provide an interpretation of the results in the context of the more realistic scenarios (see below).

An explanation of why the authors did not use standard scenarios, such as in GeoMIP, as the baseline (and then add SAS to them), since this might have facilitated comparison of their SAS-results to GeoMIP simulations, widening the scope of the work. This could have also indirectly encouraged other modelling groups to test SAS using the same standard scenarios. Also, GeoMIP-type scenarios could be more useful to start with than the scenarios proposed in the Discussion (lines 442-445) that go much further in the parameter space exploration. Please consider adding a few lines in the discussion on the implications of the results on more standardized scenarios.

Our intention was to galvanise the GeoMIP community to engage with these simulations. Unfortunately, both reviewers disagreed with this ambition, suggesting that most GeoMIP models were unable to model the aerosol-chemistry and growth mechanisms required to accurately model SAS. We agreed to remove this call to GeoMIP from our revised manuscript. However, the Reviewer asks an interesting question as to why we did not use GeoMIP as our basis simulation. We note in the Discussion:

“For the pulse simulations, we commence interventions in June in a nod to the 1991 Mt Pinatubo eruption, but this is an arbitrary design choice which should be tested in future studies for seasonal dependence.”

We expand on this by saying our decision to use UM-GAL9 rather than UKESM-1 was partially based on model readiness and partially based on the cost of running a fully coupled atmosphere-ocean model, seen as prohibitive for this sort of exploratory study. UM-GAL9 marks an improvement on the UKESM-1 atmospheric configuration (we provide model updates and a recently published evaluation paper) and will form the basis of UKESM-1.3 and UKESM2 used for CMIP7. The use of an updated model configuration was seen as a forward-facing architecture for the study, enabling future comparison with UKESM-2 submissions to GeoMIP and potentially other CMIP7-era SAS simulations. We note that our results are conditional on these decisions and provide a thorough model description in the Methodology.

“Our results are conditional on the scenario design, meteorological conditions, and model details.”

Nevertheless, we agree with the reviewer that our scenario design limits the readiness for intercomparisons and add the following to the Discussion:

Our decision to simulate idealized SAS applications in the recent historical period rather than utilize GeoMIP scenarios as a basis is motivated by the state of readiness of UM-GAL9, which is an updated atmospheric configuration of UKESM1. In a future study, we will explore more realistic SAS applications using CMIP7-era GeoMIP scenarios as a basis allowing for a wider comparison with other climate models.

2. How realistic is the deployment of SAS simultaneously, and at the same location, with SAI? As mentioned in the introduction on the realistic scenarios, SAS would probably be deployed after a delay as a reaction to SAI deployment or as a result of change in SAI deployment strategy. One experiment explores a delayed SAS deployment, but this idealised way of describing simultaneous SAI and SAS deployments should be accompanied with some discussion. One of the experiments also explores the SAS deployment on the other side of the globe, and its results should also be discussed in the context of realistic SAI-SAS deployment by two or more actors. For example, on lines 297-300 it is stated that: “We infer that SAS intervention contemporaneous to SAI produces a larger effect than a delayed response because of the increased availability of coarse condensation nuclei in the former. If SAS misses the SO₂ reservoir window, it relies primarily on coagulation rather than condensation to produce the desired growth effect.” The consequences of this result, and those of the SAS_20y_r180 experiment, on more realistic deployment scenarios could be mentioned.

The Reviewer highlights our efforts to explore the scenario space in terms of spatiotemporal proximity between the SAS and SAI applications (e.g., Table 1 and Figure 1). We already say in the Discussion section:

“We have performed sensitivity simulations where SAS is co-located and antipodal with SAI. We find that co-located SAS allows rapid condensation onto the coarse aerosol particles suggesting that efficacy decreases with increasing distance from the SAI region but that SAS may be partially effective with remote application.”

Additionally in the Results section we say that:

“If SAS is delayed by 1 month (‘SAS’ vs ‘SAS_delay’) then peak sAOD perturbation is reduced from -39 % to -26 % suggesting that SAS may still be effective even for a delayed intervention.”

We have mentioned the need for future studies to base their scenarios on realistic applications rather than idealised scenarios which we simulate here:

“Such studies could be informed by online game-theory models in which actors react to the actions of other actors, simulating the geopolitics which may govern SAI and SAS deployments (Heyen et al., 2019).”

We have added the following to the Discussion:

We have also explored the consequences of SAS delayed by 1 month and shown that this remains partially effective at reducing peak sAOD, albeit less effective than contemporaneous deployment (-26 % compared to -39 %). This preliminary exploration of the SAI-to-SAS proximity domain in effect makes different assumptions on the readiness of an SAS actor to begin “scrubbing” and its areal proximity to the original SAI application.

3. For future updates of your model: it is recommended to update the parametrisation of Vehkamäki et al. (2002) to the version published in Määttänen et al. (2018) that uses an improved theory and model that has been validated against measurements and that also provides a continuous parametrisation for large temperature, RH and sulphuric acid concentration ranges, and includes barrierless particle formation.

We thank the reviewer for highlighting this study and have added the following to the manuscript:

We note that Määttänen et al. (2018) provide an improved parameterization of this process applicable for a wider range of environmental conditions and suggest that future UKCA development utilises the updated scheme.

Määttänen, A., Merikanto, J., Henschel, H., Duplissy, J., Makkonen, R., Ortega, I. K., and Vehkamäki, H.: New Parameterizations for Neutral and Ion-Induced Sulfuric Acid-Water Particle Formation in Nucleation and Kinetic Regimes, *J. Geophys. Res.-Atmos.*, 1269–1296, <https://doi.org/10.1002/2017JD027429>, 2018.

4. The discussion mentions “specialized nozzles to emit calcite and limit agglomeration”: Could you provide a reference?

We have added the following suitable reference:

Neukermans, A., Cooper, G., Foster, J., Galbraith, L., and Jain, S.: Methods for Dispersal of Precipitated Calcium Carbonate for Stratospheric Aerosol Injection, *J. Atmos. Oceanic Technol.*, 38, 9, 1571-1584, <https://doi.org/10.1175/JTECH-D-20-0205.1>, 2021.