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

We thank both Reviewers for their valuable comments and suggestions which we believe have led to significant improvements to our study. There seem to be 2 common points of contention which we will address in our response:

1. The justification for investigating this topic is currently insufficient
2. The uncertainty and lack of observational constraints applicable to SAI are magnified by counter intervention

We acknowledge both these points and offer to make constructive and substantive changes to the manuscript which provide (1) greater justification for researching CCI and (2) highlight how our simulations provide a platform (and we believe provoke a necessity) for further research into CCI.

The Reviewer's comments are provided below in black while are responses are in red and changes made to the manuscript in blue.

Reviewer 1

1. I struggled with this paper. The analysis is done quite well, and I couldn't really find any technical errors. But my problem is the motivation for the study and the framing. This is just a weird idea.

The unique aspect of our study is bringing a hypothetical scenario which is prevalent in the social science research field into the physical science fold. In the Introduction, we highlight the work of Parker *et al.* (2018) who provided a useful albeit conceptual analysis of CCI strategies from a social and political science perspective. To quote Parker et al. (2018):

"The idea of counter-geoengineering is not wholly new and has been mentioned in passing in some academic studies (Keith & Dowlatabadi, 1992; Nightingale & Cairns, 2014) and raised repeatedly in prominent popular commentary on SRM (Barrett et al., 2014; Gertner, 2017; Hamilton, 2013; Morton, 2015; Pasztor, 2017), but it has received little scholarly analysis."

We acknowledge that we did not provide such useful background information in our draft manuscript and have added the following contextual paragraph (building on Parker et al, 2008 and including more recent literature) to the Introduction:

The idea of CCI as a strategic counterpart to SCI has long percolated within popular commentaries (e.g., Hamilton, 2013; Morton, 2015), with some scholarly analysis applied in recent years. Using game theory, Heyen et al (2019) and Bas and Mahajan (2020) have shown that the friction between SCI and CCI overcomes the oft-cited "free rider" problem, reducing the strong incentives for unilateral SCI deployment. However, these studies found that the effects of CCI were not always benign and could increase the likelihood of escalating interstate conflict or inducing a "negative welfare effect" (Morrissey, 2024). Abatayo et al. (2020) find that existence of CCI adds significant complexity to SCI governance and cooperation, promoting multilateralism and exacerbating inequality and welfare loss.

A.L. Abatayo, V. Bosetti,M. Casari,R. Ghidoni, & M. Tavoni, Solar geoengineering may lead to excessive cooling and high strategic uncertainty, *Proc. Natl. Acad. Sci. U.S.A.* 117 (24) 13393-13398, https://doi.org/10.1073/pnas.1916637117 (2020).

Morrissey, W. (2024). Avoiding atmospheric anarchy: Geoengineering as a source of interstate tension. *Environment and Security*, *2*(2), 291-315.

Bas, M.A., Mahajan, A. Contesting the climate. *Climatic Change* **162**, 1985–2002 (2020). https://doi.org/10.1007/s10584-020-02758-7

Furthermore, many SCI simulations, including the multi-model simulations performed under GeoMIP, have adopted cooperative scenarios whereby geopolitical differences are ignored and the world acts as one to combat climate change through SCI. Given the current geopolitical tensions, such an approach seems perhaps rather Panglossian. Indeed, Farley et al. (2026) developed a fast emulator that can rapidly assess (albeit in a limited number of variables) arguably more realistic scenarios. A figure from Farley et al. (2026) is presented below where various actors work in various ways to ostensibly benefit their own interests [(a) below]. We would therefore suggest that it is not unreasonable to examine a different approach based on self-interest; to try and negate the impacts of SCI through CCI. We add this detail to our answer to point 2.

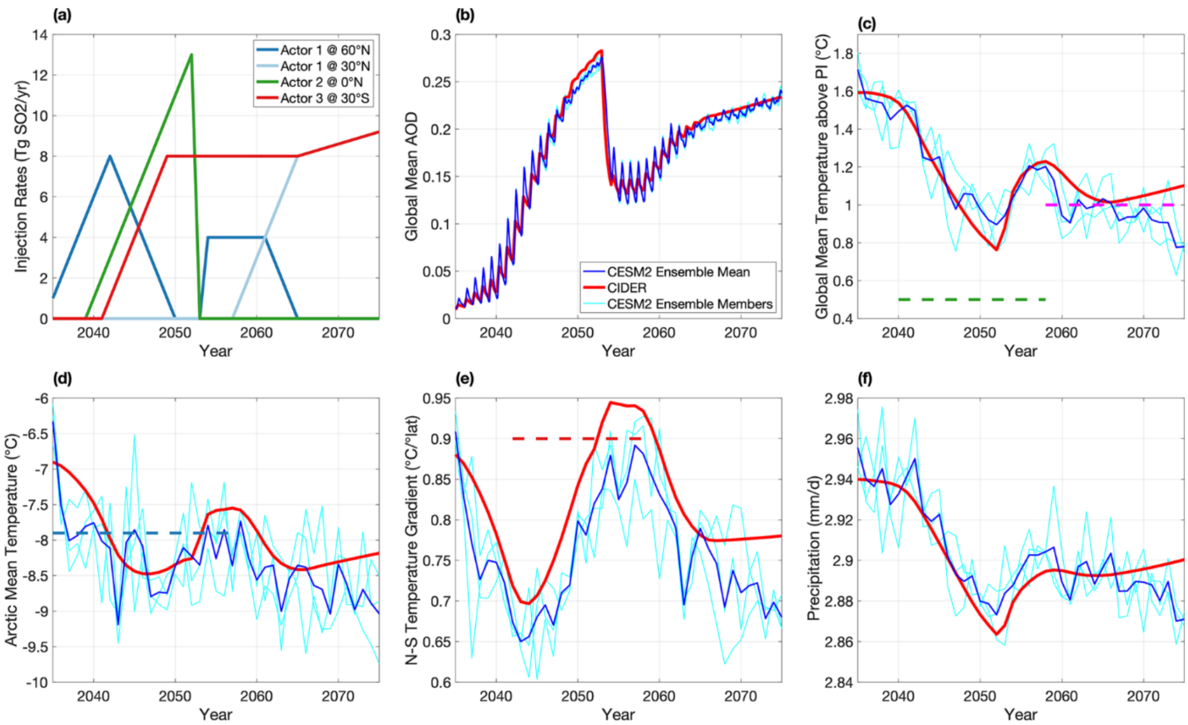

Figure R1. Farley et al (2026)

Farley, J., MacMartin, D. G., Visioni, D., Kravitz, B., Bednarz, E. M., Duffey, A., Henry, M., and Akherati, A.: A Climate Intervention Dynamical Emulator (CIDER) for scenario space exploration, Geosci. Model Dev., 19, 1809–1831, https://doi.org/10.5194/gmd-19-1809-2026, 2026.

2. More specifically, the authors conclude that to scrub SAI, you would need to do an SAI-level effort. So who, exactly, is doing this? Someone who is opposed to SAI and wants to stop SAI is…going to do SAI? I don't get it.

We give two plausible scenarios in which actors have the capability and motivation to perform CCI, building on existing literature which explores the SCI scenario space (e.g., Määttänen et al., 2026) and scenarios manifest in previous CCI papers. Our study is not about an actor that wants to stop SAI per say but stop the impacts of SAI via a counter strategy.

While we acknowledge the potential negative downsides of CCI applications (e.g., Abatavo et al., 2020), we justify the research by conceptualising two scenarios where an actors have the means, motive and opportunity to perform SAS.

Scenario 1 *Uncooperative actors / "climate clash"*: Multiple actors are involved in the design and implementation of SAI, wherein each actor develops equivalent infrastructure. Let Actor 1 and Actor 2 both offer to share SAI responsibility, with Actor 1 injecting aerosol at 15 or 30°N and Actor 2 injecting aerosol at 15 or 30°S. After initiation, Actor 1 decides unilaterally to change the scope of *its* SAI intervention in its own favour, which negatively impacts or is predicted to negatively impact Actor 2. Actor 2 now has the means, motive, and opportunity to perform SAS, by changing its emitted substrate from an SAI substrate to a SAS substrate.

Scenario 2 *Cooperative actors / "cooperative outcomes"*: An actor or multiple actors decide to perform SAI in a mutually beneficial way, sharing the responsibility for infrastructure and implementation. After initiation, a problem is realised which requires a facilitated cessation SAI such as: (1) a super-volcanic eruption which exacerbates the effects of SAI; (2) conflict which threatens to reduce capacity for cooperation; (3) unintended and deleterious climate impacts such as ozone damage or extreme weather phenomena. The actors cooperatively decide to perform SAS in order to reduce the aerosol burden.

Many SCI simulations, including the multi-model simulations performed under GeoMIP, have adopted cooperative scenarios whereby geopolitical differences are not considered and the world acts in unity to combat climate change through SCI. Given the current geopolitical tensions, such an approach appears optimistic. Rather, recent studies such as Määttänen et al (2026) and Farley et al. (2026) have explored the scenario space by developing fast emulators that can rapidly assess (albeit in a limited number of variables) more realistic scenarios incorporating political dissonance. We therefore suggest that it is opportune to examine a different approach; to try and negate the impacts of SCI through CCI.

References

Anni Määttänen, François Ravetta, Jérôme Bureau, Thibaut Lurton and Olivier Boucher: Idealized modeling of stratospheric aerosol injection deployment scenarios with two non-cooperative actors, Environ. Sci.: Atmos., 2026,6, 324-337, DOI: https://doi.org/10.1039/D5EA00022J

Farley, J., MacMartin, D. G., Visioni, D., Kravitz, B., Bednarz, E. M., Duffey, A., Henry, M., and Akherati, A.: A Climate Intervention Dynamical Emulator (CIDER) for scenario space exploration, Geosci. Model Dev., 19, 1809–1831, https://doi.org/10.5194/gmd-19-1809-2026, 2026.

3. Related to that, the authors assert that this method is "effective". That may be true from a purely chemical standpoint (more on that shortly), but that's a really narrow perspective. If your countermeasure takes as much or more effort than the original activity, I wouldn't really call that effective. And again, it requires building a huge

amount of infrastructure that is, until deployment, indistinguishable from SAI. So it's hard to justify effectiveness in many senses beyond chemical.

The definition of *effective* here is subjective, and we provide reasons why we argue that our results support our conclusion of effectiveness.

Firstly, the idea that 30-40 % does not amount to "effectiveness" is questionable. The offset of 30-40 % climate impacts could be the difference between 5 and 2 natural hazards in a region, which have compound impacts on society, therefore amounting to a greater positive impact than the sum of individual benefits. As a metaphor, a 40 % reduction in disease transmissivity could be the difference between an epidemic and a contained outbreak. Assuming that CCI could ever be totally effective is simplistic due to the inherent scattering properties of the substrate, which we note when we say:

"There is a trade-off between the reflective properties of calcite which make it an SAI candidate (Dai et al., 2020) and its scrubbing ability"

We have determined using a basic cost analysis that CCI *costs* are of very similar order to SCI assuming equivalent infrastructure, with examples of when actors would have access to such infrastructure provided in our answer to Point 2. Using these and similar scenarios, the additional cost of CCI over SCI is solely in the manufacture and storage of a suitable substrate which is comparatively affordable (O(600 million USD) in our assessment) and much cheaper than the cost of SCI infrastructure such as manufacturing a suite of suitable aircraft. We already say this in the text.

"Nevertheless, the fact that SAS mirrors SAI in design should mean that any infrastructure designed to deliver SO_2 to the stratosphere could be adapted for SAS, albeit needing specialised nozzles to emit calcite and limit agglomeration."

The second point is that the physical or chemical efficiency of CCI to ameliorate the impacts of SCI may be a sufficient deterrent to minimalize the risk of unilateral or 'uncooperative actor' SCI implementations. The *effectiveness* here is then in reducing SCI risk and is an indirect consequence of CCI's physical or chemical efficacy. This is one potential outcome scenario found in Heyen et al (2019), in which CCI acts a successful deterrent to unilateral SCI deployments, reducing the "free-rider" problem.

Nevertheless, we agree that whether there is a degree of subjectivity as to whether CCI is *effective* based on our results and it is important to state this explicitly. We have added the following to the Discussion:

Whether ~30-40 % reduction in aerosol burden due to SAS is "effective", "partially effective" or "ineffective" is subjective depending on cost-benefit assumptions and it is important to note the high scenario-related uncertainty in the SAS response in our simulations (e.g., Fig. 1). Nevertheless, an SAI deterrent such as SAS could provide an effective barrier against unilateral SAI deployments or the "free rider" problem (e.g., Heyen et al., 2019) which should be factored into this discussion.

4. Getting into the chemistry, the choice of calcite is poorly justified. There are also really important processes missing, including aerosol mixing state (it looks like you use a simple assumption for this), chemical interaction (offline oxidants), and aerosol

aging. There is strong evidence to suggest that after a while the calcite aerosols just get covered in sulfate and look a lot like sulfate aerosols. And in terms of particle growth, using a modal double-moment scheme probably has some issues. The point being, while this is an interesting idea, you've made enough assumptions in your model that your results could be totally wrong.

The Reviewer highlights two points here that we respond to in turn: (1) the assertion that calcite is a poor choice of substrate, and (2) that simplistic assumptions in our model limit its capability to simulate CCI.

Calcite as a CCI substrate

We disagree with the Reviewer that the use of calcite is poorly justified and give further reasons below. We agree with, and have in effect answered to, Parker et al. (2008)'s list of requirements for a suitable candidate substrate:

1. *"It would need to be sufficiently **cheap to manufacture and release**.*
2. *It would need to have acceptably **low side effects on health or the environment**.*
3. *Its **atmospheric lifetime would need to be similar to or lower than the atmospheric lifetime of the geoengineering technique** that it was countering"*

They later provide more recommendations for the choice of candidate SCI for a countervailing measure:

*It might, for example, be possible to use high-altitude aircraft to add a **base** to the stratosphere to counteract the sulphate aerosol that is most commonly considered for geoengineering, though that would only reduce radiative forcing if the resulting salt had a lower radiative forcing or lifetime (Keith et al., 2016). Alternatively, it might be possible to introduce a substance that would accelerate the coalescence or coagulation rate of the geoengineering aerosol and accelerate its removal from the atmosphere by sedimentation.*

Calcite is a **base**, it is **cheap** to manufacture (as quantified in the penultimate paragraph in the Introduction), would have negligible side effects on health in the quantities simulated here and no more deleterious than SO_2, and inherently would have **a lower atmospheric lifetime** than SO_2/H_2SO_4. We already include the following text in the manuscript which says this:

"Additionally, calcite has a low real part of the refractive index and so is not an optimal light scatterer (Pope et al., 2012); it is alkaline thereby reacting readily with the acidic oxidation products of SO_2; it is hygroscopic; is relatively inert or potentially positive in its impacts on stratospheric ozone (Dai et al., 2020; Vattioni et al., 2024b) readily available and non-toxic."

Vattioni, S., Weber, R., Feinberg, A., Stenke, A., Dykema, J. A., Luo, B., Kelesidis, G. A., Bruun, C. A., Sukhodolov, T., Keutsch, F. N., Peter, T., and Chiodo, G.: A fully coupled solid-particle microphysics scheme for stratospheric aerosol injections within the aerosol–chemistry–climate model SOCOL-AERv2, Geosci. Model Dev., 17, 7767–7793, https://doi.org/10.5194/gmd-17-7767-2024, 2024b.

Model deficiencies

Every climate model uses parameterizations to represent processes on the sub-grid scale and UM-GAL9 is no exception. Nevertheless, the complexity of the UKCA-mode aerosol model to simulate stratospheric aerosol microphysics is well-established, which we refer to in the text, e.g., aerosol plume evolution following the 2019 Raikoke eruption (Wells et al., 2024).

The use of slightly different model configurations as emblematic of a model skill's is well-established practice. For example, the UM version used in Perny et al (2025) is said to differ from prior versions by *"including the adaptations for stratospheric aerosol described in Dhomse et al. (2014), with additionally heterogeneous nucleation of sulfate aerosol from meteoric smoke particles, as described in Brooke et al. (2017), and used equivalently in the GA4 UM-UKCA simulations in Marshall et al. (2018, 2019); Dhomse et al. (2020)."* The UM-GAL9 evaluation process was guided by systematic internal assurance procedures which required aerosol predictive skill to be at least as good as its predecessor models (see e.g., Martin et al., 2025 for assurance protocol). None of the relevant aerosol routines were changed during this process and so any impacts on aerosol physics will be indirect. From this we infer that we can use other UM-based models such as UKESM1 as representative of stratospheric aerosol predictive skill, following the precedent in papers such as Perny et al (2025).

We have been very careful to make explicit reference to the aspects of UM-GAL9 which may affect the results we present, as noted by the Reviewer. Additionally, we have made suitable recommendations for follow on studies which can verify our results e.g.:

"The representation of calcite in UM-GAL9 is simplified and omits heterogeneous chemical reactions which change the condensation uptake rate of stratospheric vapours such as nitric acid (HNO_3), hydrochloric acid (HCl), and H_2SO_4 onto the aerosol surface (Vattioni et al., 2024; Cziczo et al., 2019). These simulations therefore represent condensation onto stratospheric calcite in a simplified manner, assuming an uptake coefficient of gaseous H_2SO_4 onto soluble aerosol of 1 as is standard for UKCA-mode. Another simplification of our approach is that UM-GAL9 uses 'offline oxidants' chemistry which precludes a full impact assessment of SAS on stratospheric ozone which would require interactive stratospheric chemistry (Bednarz et al., 2025)."

Nevertheless, we would argue that stratospheric aerosol growth on CCI-particles would be continuous *despite* the coating effect supported by observations following the Pinatubo eruption (Stenchikov et al., 1998) and laboratory studies. For example, the highly germane laboratory study of McGrory et al (2022) where silica and alumina particles continued to grow in a sulphur rich environment *despite* coating effects (Fig 3, McGrory et al., 2022). The aerosol mixing state is simple in UM-GAL9 (volume mean) but the dominant impact here is enhanced aerosol growth leading to sedimentation which is well captured and in agreement with observations (e.g., Wells et al., 2024). The use of offline oxidants will change the oxidation rates of SO_2 to H_2SO_4 but this caveat is already in the text. We add the following to the Discussion:

Only a few models exist with fully interactive stratospheric chemistry, 3-D atmospheric dynamics and sectional aerosol microphysics schemes (Kleinschmitt et al., 2017; Tilmes et al., 2023). The UM relies on a modal aerosol scheme which may not represent aerosol coagulation processes between calcite and sulfate aerosol with as much fidelity as more complex sectional aerosol schemes (e.g. Laakso et al., 2022). Based on these caveats, our results should be interpreted with caution given uncertainty in the representation of processes, the dependence on one climate model, and sensitivity of results to the scenario design. Clyne et al. (2021) found that differences in model physics and chemistry caused significant differences in volcanic aerosol evolution for multi-model simulations of the Mt Tambora eruption. The use of offline rather than online oxidants could too rapidly oxidise SO2 to form H2SO4, as found by Clyne et al. (2021). Given that we show the condensation effect to dominate over coagulation (e.g., Fig 10), this could imply that SAS efficacy is understated in our simulations. In their laboratory study, McGrory et al (2022) found that silica and alumina particles continued to grow due to condensation in a sulphur rich environment despite coating effects. Laboratory studies would provide useful constraints to test future SAS modelling studies in the absence of natural analogues.

References

Megan R. McGrory, Rosalie H. Shepherd, Martin D. King, Nicholas Davidson, Francis D. Pope, I. Matthew Watson, Roy G. Grainger, Anthony C. Jones, and Andrew D. Ward: Mie scattering from optically levitated mixed sulfuric acid–silica core–shell aerosols: observation of core–shell morphology for atmospheric science, Phys. Chem. Chem. Phys., 2022,24, 5813-5822

Laakso, A., Niemeier, U., Visioni, D., Tilmes, S., and Kokkola, H.: Dependency of the impacts of geoengineering on the stratospheric sulfur injection strategy – Part 1: Intercomparison of modal and sectional aerosol modules, Atmos. Chem. Phys., 22, 93–118, https://doi.org/10.5194/acp-22-93-2022, 2022.

Tilmes, S., Mills, M. J., Zhu, Y., Bardeen, C. G., Vitt, F., Yu, P., et al. (2023). Description and performance of a sectional aerosol microphysical model in the community earth system model (CESM2). *Geosci. Model Dev.* 16, 6087–6125. doi: 10.5194/gmd-16-6087-2023

Kleinschmitt, C., Boucher, O., Bekki, S., Lott, F., and Platt, U. (2017). The sectional stratospheric sulfate aerosol module S3A-v1 within the LMDZ general circulation model: description and evaluation against stratospheric aerosol observations. *Geosci. Model Dev.* 10, 3359–3378. doi: 10.5194/gmd-10-3359-2017

Perny, K., Sukhodolov, T., Kuchar, A., Arsenovic, P., Rosati, B., Brühl, C., Dhomse, S. S., Jörimann, A., Laakso, A., Mann, G., Niemeier, U., Pitari, G., Quaglia, I., Sekiya, T., Sudo, K., Timmreck, C., Tilmes, S., Visioni, D., and Rieder, H. E.: Assessing the stratospheric temperature response to volcanic sulfate injections by Mt. Pinatubo: insights from the Interactive Stratospheric Aerosol Model Intercomparison Project, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2025-5915, 2025.

5. And finally, I don't understand the call to include this in GeoMIP. Getting the microphysics and chemistry right for this idea is highly important. Only a small handful of GeoMIP models can do this. If you had proposed it to CCMI I might agree.

We agree with the Reviewer on this comment and after careful consideration have decided to remove the direct invocation to GeoMIP. However, we still believe that this warrants further research given the significant societal ramifications and the demonstration of partial efficacy.

6. Overall, I have trouble with what the authors did, including the justification for doing the study in the first place, how the modeling was done, and the interpretation of the results. I think these issues could ultimately be addressed by reframing the paper, as there's some good technical work in here. But considering the headlines this idea might generate, overclaiming what you've done and the results you've found is irresponsible.

We have addressed the Reviewer's concerns by adding the following:

1. Further justification for doing the study in the first place (point 1)
2. A more balanced interpretation of results accounting for subjectivity in terms of "effectiveness" (point 3)
3. Further explanation for why the results are likely robust routed in observations and laboratory studies and renewed emphasis on the need for more studies to confirm these results (point 4)
4. Removal of the call to GeoMIP (point 5)

Given the sensitivity of this study, we are keen to provide a responsible interpretation of our results and so we hope our responses may reassure the Reviewer that we take their concerns seriously. Nevertheless, we believe that CCI as a concept does not solely belong to the social sciences and we provide a platform for efficacy assessment and preliminary results which will be of wide interest.

Reviewer 2

1. The authors have run quite a large number of simulations to explore this scenario (Table 1), and the science topic aligns with SAI predictions, the interaction of the calcite with the sulphate aerosol a novel aspect, and then certainly scientifically relevant to the Atmospheric Chemistry and Physics journal.

We thank the Reviewer for their comments and suggestions.

2. However, I have to say I agree with most of the comments made by the other reviewer, in relation to this scenario being somewhat far-fetched, or too secondary to an already uncertain main SAI simulation. The main effect explored is the scavenging/scrubbing effect, which is implicitly strongly dependent on the model's simulated SAI enhancement, and then it is unclear what the reader can conclude meaningfully from these secondary integrations.

As in our response to Reviewer 1, we have added additional justification for the study to the manuscript, building on Parker et al. (2018) and updating the literature:

The idea of CCI as a strategic counterpart to SCI has long percolated within popular commentaries (e.g., Hamilton, 2013; Morton, 2015), with some scholarly analysis applied in recent years. Using game theory, Heyen et al (2019) and Bas and Mahajan (2020) have shown that the friction between SCI and CCI overcomes the oft-cited "free rider" problem, reducing the strong incentives for unilateral SCI deployment. However, these studies found that the effects of CCI were not always benign and could increase the likelihood of escalating interstate conflict or inducing a "negative welfare effect" (Morrissey, 2024). Abatayo et al. (2020) find that existence of CCI adds significant complexity to SCI governance and cooperation, promoting multilateralism and exacerbating inequality and welfare loss.

3. **Suitable for ACP? (collected suggestions)**

I also tend to agree with the other reviewer's questioning of the motivation and framing for this study. And having thought carefully about this decision, I feel this study is too exploratory for publication within a specialist journal such as ACP. Other journals (e.g. Earth's Future or Geoscientific Model Development) would be more suitable for these simulations, towards then publishing what is currently a highly exploratory study, geared to a somewhat secondary counter-intervention scenario that seems poorly motivated, at least within this initial manuscript (see specific comments below).

In summary, whilst I appreciate this study can still be valuable at this exploratory level, my review finds this too exploratory to be publishable within a specialist journal such as ACP. The study should be re-submitted to either a model development journal such as GMD, or a broader-based journal such as Atmospheric Science Letters or Earth's Future. I recommend the authors consider a different journal such as GMD or Climate Futures, rather than Atmospheric Chemistry and Physics.

As I say, the results and interpretation presented is of good quality, but the scenarios explored are not sufficiently constrained to yield meaningful results for a specialist journal such as ACP. In fact, it occurred to me rather the interaction between the dust and sulphate is more towards assessing the sensitivity of a particular aspect of the model, and then this study could fit well to a model development journal such as Geoscientific Model Development or JAMES. Or alternatively to broader-scope journal such as Earth's Future or Atmospheric Science Letters.

I realise the authors have put a considerable effort into running and analysing the model simulations and do share also the other reviewer's positive comments that the results within the manuscript are well explained, and presented well. However the manuscript is in my opinion out of scope for ACP.

This point is contradicted by Point 1 where the Reviewer agrees with us that the material is scientifically relevant to ACP. For context, this paper has previously been submitted to a less specialist journal (as the Reviewer suggests doing), with feedback provided from *that* Editor that the paper belonged to a more specialist journal and explicitly referred us to ACP. This suggests that some level of subjectivity is applicable here. We believe ACP is the right journal for the following reasons.

We would argue that the paper has useful *physical* science, which is applicable to both specialist and transdisciplinary journals, being routed in atmospheric aerosol theory and closely aligned with existing SCI studies published in ACP but also consisting of hypothetical scenarios which are yet to be explored with a climate model. ACP has a *long* history of publishing hypothetical and exploratory SCI scenarios routed in atmospheric physics and chemistry (e.g., Quaglia et al., 2022) and this is no more exploratory than those scenarios, whilst being routed in the theory of aerosol microphysics. Saying that this paper is not appropriate for ACP sets a dangerous precedent for other exploratory SCI studies and unnecessarily limits the scope of ACP.

Quaglia, I., Visioni, D., Pitari, G., and Kravitz, B.: An approach to sulfate geoengineering with surface emissions of carbonyl sulfide, Atmos. Chem. Phys., 22, 5757–5773, https://doi.org/10.5194/acp-22-5757-2022, 2022.

5. One specific point the other reviewer makes, is to question whether this scenario be included within the next phase of GeoMIP. And whilst I do not respond with my own views on that, I do see why the reviewer asks the questions there. It relates to the central question of the study exploring model responses from a control run which is itself very highly uncertain. And then these secondary experiments, to assess a "counter climate intervention" effect seem premature, when there is such a large uncertainty around the main question of efficacy and risks from SAI geoengineering.

We agree with the Reviewer on the first comment and after careful consideration have decided to remove the direct invocation to GeoMIP. However, we still believe that CCI warrants further research given the significant societal ramifications and the demonstration of efficacy.

We disagree with the statement that studying CCI any further is premature, despite agreeing in part that there is uncertainty over SAI efficacy and risks. The barriers to deployment for SAI are similar to the barriers for SAS (e.g., aircraft, infrastructure, etc). We say this in the text:

"Nevertheless, the fact that SAS mirrors SAI in design should mean that any infrastructure designed to deliver SO_2 to the stratosphere could be adapted for SAS, albeit needing specialised nozzles to emit calcite and limit agglomeration"

Whilst SAS adds an additional level of uncertainty on top of SAI in terms of design space, both are routed in the same aerosol microphysics (nucleation, condensation, coagulation) and therefore the uncertainty is not mutually exclusive. The SAS scenarios is similarly uncertain to SCI studies that employ calcite or solid aerosol injection such as Vattioni et al. (2024) wherein background stratospheric sulfate in the Junge layer accumulates on the solid aerosol surfaces.

6. Whilst for the Pinatubo observed case, the different interactive models agree quite well (Quaglia et al., 2023), Clyne et al. (2021) have shown how differing treatment of chemistry and physics within the interactive models lead to markedly different results for cases where there are fewer observations to constrain/calibrate model predictions, e.g. early historical major eruptions. It is certainly relevant for ACP to explore what the models predict for SAI geoengineering, and whilst I have considered whether this exploratory simulation could potentially be caveated sufficiently to be publishable in ACP, I just don't see how that can be the case for these simulations, not for ACP.

We thank the Reviewer for the useful reference to Clyne et al (2021). However, there is a contradiction in the Reviewer's comments where they say: "It is certainly relevant to explore these hypothetical scenarios (SAI) but not these other hypothetical scenarios (SAS)". All SAI scenarios are exploratory, and the volcanic analogue is not perfect for SAI. We appreciate the reference to Clyne et al. which demonstrated the high sensitivity of AOD in different models but the peak sulfate burden in their simulations (Figure 3) showed relatively small range (27-29 Tg S excluding LMDZ) which is perhaps more useful constraint for these SAS simulations. We have added further justification for studying SAS (point 2), and have added the following caveat to the Discussion:

Based on these caveats, our results should be interpreted with caution given uncertainty in the representation of processes, the dependence on one climate model, and sensitivity of results to the scenario design. Clyne et al. (2021) found that differences in model physics and chemistry caused significant differences in volcanic aerosol evolution for multi-model simulations of the

Mt Tambora eruption. The use of offline rather than online oxidants may overestimate the oxidation rates of SO2 to form H2SO4, as found by Clyne et al. (2021). Given that we show the condensation effect to dominate over coagulation (e.g., Fig 10), this could imply that SAS efficacy is understated in our simulations. In their laboratory study, McGrory et al (2022) found that silica and alumina particles continued to grow due to condensation in a sulphur rich environment despite coating effects. Laboratory studies would provide useful constraints to test future SAS modelling studies in the absence of natural analogues.

"Model physics and chemistry causing inter-model disagreement within the VolMIP-Tambora Interactive Stratospheric Aerosol ensemble", Atmos. Chem. Phys., 21, 3317–3343, https://doi.org/10.5194/acp-21-3317-2021 .

7. The results from the counter climate intervention scenario must be extremely dependent on the SAI-enhanced state predicted there, and specifically for this case of the main effect being a scavenging or scrubbing of the existing aerosol predicted by the model. That is not to suggest this particular model is any more likely or less likely to yield a realistic SAI enhancement. It's just to observe that further work needs to be done to understand differences between the base SAI case, and the natural analogue large volcanic cases, before this type of secondary model experiments around counter climate intervention can be presented, at least for this specialist ACP journal.

We agree with the Reviewer that it is important to highlight that different models may respond differently. However, it is customary in the SAI literature to say "more work needs to be done to understand inter-model differences" which sometimes can be unhelpful. Indeed, we invoke this sentiment by calling for further multi-model studies investigating CCI to understand differences in response. Not everything can be achieved using multiple models given the limited resources and our pilot study is a good example of blue-sky thinking which may provoke a multi-model comparison. We've added the necessary caveats to our paper to highlight deficiencies in the model setup, including the dependency on one model, and offer ideas on how to improve the methodology (see answer to point 6). Nevertheless, climate models including UM with UKCA-mode are routinely assessed for agreement with volcanic observations (with VolMIP and AerChemMIP being excellent examples). We have explicitly given examples in the text:

"We use a climate-resolution model, albeit one which in previous configurations (with exactly the same representation of aerosol microphysics) has been used for volcanic eruption assessments (Zanchetting et al., 2022) and SAI experiments (Wells et al., 2024) and shown to have good skill in representing stratospheric dynamics and aerosol evolution compared to post-volcanic observations (Wells et al., 2023)."

Our study is not appropriate for GMD or JAMES being a science paper and we have provided suitable justification for publication in ACP in our answer to point 3. The interaction between dust and sulphate is irrelevant given the small volume fraction of calcite in dust and the different regions of the atmosphere under investigation, but the ageing of volcanic ash by SO_2 which we have previously looked at in our model (Wells et al., 2023) is more relevant in the stratosphere.

8. Interactive simulations are important, but for SAI these should focus on the main SAI enhanced case, whilst still the response can be (to some extent) testable comparing to atmospheric observations of a similar response. The study here is essentially assessing the combination of two unconstrained aspects at once, and then the predictions can remain only exploratory secondary experiments.

   The study of Weisenstein et al. (2022) has shown that the interactive models differ a lot within their predictions of the main SAI forcing effect, for example Figures 6, 7 and 9 in the SAI intercomparison study indicate the two ECHAM-based models predict around a factor of 2 lower SAI forcing than seen in the CESM simulations

The scope of the study is to perform an efficacy assessment of SAS rather than SAI; other papers may concentrate on SAI. SAI is testable in a sense that the volcanic analog exists, but even that is an imperfect analog (Duan et al., 2019) and there are no observations of volcanic eruptions in a well-mixed stratospheric aerosol layer, suggesting that the testability of SAI may be limited. Rather, models depend on the evaluation at the process level, to which the parameterisations are well tuned empirically. Many SAI papers preclude or exclude a direct evaluation of their stratospheric aerosol scheme against volcanic constraints. The uncertainty in the aerosol processes between SAI and SAS are not mutually exclusive, both depending on aerosol microphysics. We have offered suitable references which show that our stratospheric aerosol scheme has been evaluated using volcanic observations (see answer to point 7), and suitable caveats where deficiencies in our approach could be improved (see answer to point 6).

It is true that interactive models show differences in the SAI forcing effect and the caveat added in point 6 affirms the single-model dependency and further reinforces our call for future studies investigating CCI with different models, informed by coordinated laboratory studies.

Duan, L, Cao, L, Bala, G, Caldeira, K. 2019. Climate response to pulse versus sustained stratospheric aerosol forcing. *Geophysical Research Letters,* 46, (15):8976–8984, DOI: http://dx.doi.org/10.1029/2019GL083701.

Specific revisions, to improve the framing of the study

S1. Introduction, line 31 – This is a key sentence within the framing of the manuscript, introducing for the first time the stated concept of "Counter Climate Intervention" (CCI). And yet the stated basis "CCI could be attractive to a rival actor who opposes SCI" is not credible. I mean actually deploying a large-scale injection of calcite would be too prohibitively expensive to have the scenario be something a "rival actor" could then begin, to conteract the SRM forcing.

Within this same sentence there is then a 2nd stated rationale, that the same actor could have in mind a "contingency measure" in relation to some unexpected outcome from an initial deployment. That's potentially more plausible, compared to the 1st rationale, for example if an unexpected hemispheric imbalance or large magnitude of a polar ozone effect or other stratospheric response occurred.

We have specifically added 2 scenarios to the text in which actors may have the means, motive and opportunity to perform SAS. The significant scrutiny of CCI in the social sciences (e.g., Heyen et al., 2019; additional references added to Introduction) highlights the legitimacy with which social scientists and others believe CCI scenarios are plausible. We take the point that SAS could be a contingency matter seriously and have added this as Scenario 2. These scenarios are directly taken from the social science literature.

Scenario 1 *Uncooperative actors / "climate clash"*: Multiple actors are involved in the design and implementation of SAI, wherein each actor develops equivalent infrastructure. Let Actor 1 and Actor 2 both offer to share SAI responsibility, with Actor 1 injecting aerosol at 15 or 30°N and Actor 2 injecting aerosol at 15 or 30°S. After initiation, Actor 1 decides unilaterally to change the scope of *its* SAI intervention in its own favour, which negatively impacts or is predicted to negatively impact Actor 2. Actor 2 now has the means, motive, and opportunity to perform SAS, by changing its emitted substrate from an SAI substrate to a SAS substrate.

Scenario 2 *Cooperative actors / "cooperative outcomes"*: An actor or multiple actors decide to perform SAI in a mutually beneficial way, sharing the responsibility for infrastructure and implementation. After initiation, a problem is realised which requires a facilitated cessation SAI such as: (1) a super-volcanic eruption which exacerbates the effects of SAI; (2) conflict which threatens to reduce capacity for cooperation; (3) unintended and deleterious climate impacts such as ozone damage or extreme weather phenomena. The actors cooperatively decide to perform SAS in order to reduce the aerosol burden.

2. Introduction, line 33 -- The sentence after that in point 1) states that "CCI research to date has focused on the ethics and political will for CCI…"., but does not provide any citation in the sentence.

There is only the citation to Parker et al. (2018) in the follow-on sentence, but the text there indicates more than 1 study on Counter Climate Intervention. If that is the case these other studies need to be cited here.

We thank the Reviewer for this suggestion and have added the following text to the Introduction with more background / contextual information.

The idea of CCI as a strategic counterpart to SCI has long percolated within popular commentaries (e.g., Hamilton, 2013; Morton, 2015), with some scholarly analysis applied in recent years. Using game theory, Heyen et al (2019) and Bas and Mahajan (2020) have shown that the friction between SCI and CCI overcomes the oft-cited "free rider" problem, reducing the strong incentives for unilateral SCI deployment. However, these studies found that the effects of CCI were not always benign and could increase the likelihood of escalating interstate conflict or inducing a "negative welfare effect" (Morrissey, 2024). Abatayo et al. (2020) find that existence of CCI adds significant complexity to SCI governance and cooperation, promoting multilateralism and exacerbating inequality and welfare loss.

A.L. Abatayo, V. Bosetti,M. Casari,R. Ghidoni, & M. Tavoni, Solar geoengineering may lead to excessive cooling and high strategic uncertainty, *Proc. Natl. Acad. Sci. U.S.A.* 117 (24) 13393-13398, https://doi.org/10.1073/pnas.1916637117 (2020).

Morrissey, W. (2024). Avoiding atmospheric anarchy: Geoengineering as a source of interstate tension. *Environment and Security*, *2*(2), 291-315.

Bas, M.A., Mahajan, A. Contesting the climate. *Climatic Change* **162**, 1985–2002 (2020). https://doi.org/10.1007/s10584-020-02758-7

3. Introduction, line 37 – The wording here "could be effectively offset by this approach" over-states the findings in the Fuglestedt et al. (2014) paper, and hasn't sufficiently explained what "this approach" means here. And which criteria determine to be able to say "effectively offset", or otherwise. Global-mean or regionally balanced?

We stated "*global* surface cooling" and did not mention regional impacts. The Fuglestedt et al paper is somewhat tangential to our SAS study but is useful for contextualising the CCI literature. We have revised the text to remove "effectively offset" and define "this approach", as well as highlighting the obstacles to deployment as noted in that paper.

Fuglestvedt et al. (2014) found that a global-mean surface cooling of 2 °C under a super-volcanic eruption could be effectively countered by the emission of short-lived greenhouse gases, albeit the authors highlighted that implementation would face significant obstacles.

4. Introduction, line 38 – similarly this study re: fluorinated gases seems add-on, and potentially not relevant or at best under-developed in this version.

The Reviewer has requested further justification for our study and motivation for researching CCI. We include this line and the Fuglestedt study as useful context on the status of CCI research, despite their tangential nature, and opt to keep the references in the text.

5. Introduction line 40 – "with the goal of making SCI technology inert". What is meant by "inert" here? This is poor writing here, and somewhat underlines the concept of a "Counter Climate Intervention" just seems hard to reconcile its name with a realistic purpose or achievable goal.

We agree with the Reviewer that this text is ambiguous. We have replaced this line with:

PA18 also propose CCI methods which they denote 'neutralizing measures', with the goal of counteracting an SCI application, such as by directly removing aerosol from the atmosphere.

6. Introduction line 41 – "PA18 suggests adding a substance that promotes condensation or coagulation to force the aerosol to grow and sediment faster". This is too simplified a narrative, and the specifics here need to be clearer. What substance – at emission? In addition to sulphur or in place of the sulphur? In what mechanism would this be effective? Need to be as large as the sulphur emission itself? Not clear at all how this could even be feasible on paper.

This is an introductory sentence that sets the scene and we believe is a succinct and complete summary of SAS. The additional information that the Reviewer asks for is explicitly provided later in the Introduction or answered by the manuscript.

7. Introduction line 42 – "This has parallels to flue-gas scrubbing, i.e. generation of particles that are subsequently removed". Not really. It's a completely different effect, the scrubbing is uptake onto the calcite particles, and occurs very quickly, within the plume itself.

The growth processes happen over much longer timescales, for example the peak in aerosol particle size after Pinatubo occurred much later than the peak in optical depth, around a year after the eruption. So the timescales and associated spatial extent of the changes caused by the two mechanisms are completely different. The sentence should be re-framed to point out the differences in timescales and the corresponding consequences in terms of strategies within model experiments to assess these effects.

We disagree with the Reviewer on this point and will corroborate our rebuttal using our results. Firstly, the aerosol ageing after Pinatubo *was* gradual, but this was because of a lack of ambient coarse aerosol particles (as directly added in SAS), and therefore the condensational growth was limited. The timescales of aerosol growth in SAS are necessarily shorter than after a volcanic eruption or under SAI *because* of the addition of coarse aerosol particles, as corroborated by the increased decay rate of aerosol burden in Figure 5.

The parallels between SAS and flue-as scrubbing are in the objectives of both – injection of condensation nuclei to remove H_2SO_4 gas. The timescales may be slower in SAS than in flue-gas scrubbing but this is partly due to the time needed to oxidise SO_2 to form H_2SO_4 in the stratosphere, and not in the scrubbing process. We find in our simulations that the condensation effect dominates over coagulation in SAS (Fig. 6 and Fig. 10) which again agrees with the underlying process governing flue-gas scrubbing. Certainly, we agree that the spatial scales are different, but ultimately the parallels (in terms of objectives and processes) are sufficiently close to make the link. We have added the following caveat to the text:

Such a process could be considered analogous for SAS, despite differences in the time and spatial scales of the mechanisms given the similarity in the objectives and the underlying processes.