We thank three anonymous referees for their helpful comments. We have revised our manuscript, and respond to individual comments below in blue.

Author response to comments of Anonymous Reviewer #1

This paper presents initial results from four different Earth system models, comparing the updated G6-1.5K-SAI strategy against the previous GeoMIP experiment, G6sulfur. The findings indicate that while all models successfully meet the target global mean temperature (2020-2039 SSP2-4.5 average), they show significant variations in injection efficiency, aerosol distribution, and regional impacts on precipitation and Arctic temperatures.

Overall this paper is very well written, thorough and explains each step very clearly. In particular I would like to commend the authors for their detailed explanations of previous works and the presentation and explanation of figures 5 onwards. The description of the three time periods and how they relate within each figure, especially compared to the G6sulfur results, is a great way for readers to understand the differences in results.

I only have a few very minor comments, otherwise I recommend this study for publication.

We thank the reviewer for their feedback and kind words. We have revised our paper appropriately; we respond to individual comments below.

Section 2.1 - I think it would be helpful to have the 2020-2039 average temperature relative to pre-industrial for each model and/or the difference in warming by 2065-2084. This could also be well placed in section 3.1.

This is a good suggestion; we have added PI temperatures, and the amount of warming in the 2020-2039 period relative to PI, to Table 1.

Table 1 - The UKESM atmospheric resolution is 1.25° *lat* vs 1.875° *lon*. I also had a question regarding the MIROC and E3SM atm. resolution - are they the same for both lat and lon?

Yes; MIROC and E3SM each have the same horizontal atmospheric resolution for latitude as for longitude. We have clarified this in the table and fixed the UKESM resolution.

Figure 2 - (c) is difficult to see all the ensemble means here, is it possible to have all the ensemble means on top of the individual ensemble members? Or reduce the alpha of the individual members?

Another reviewer also commented on the readability of this figure; we have adjusted the width, transparency, and layering of some lines in Fig. 2c (as well as 2d and 2e) to make them easier to interpret.

Line 213 - “is the *only G6-1.5K-SAI* model to show…”

Fixed

Line 235 - “In all four models,...” the authors should clarify that the first half of this sentence refers to the warming scenario to make it more clear for readers.

We have clarified the wording.

Figure 6 - It would be useful to include the G6sulfur models in the figure caption, similar to Figure 2, it would be useful for readers to easily find which models have been included.

Added

Lines 302-303 - It would be worth naming the warming scenarios for G6sulfur and G6-1.5K-SAI in these sentences.

Done

Author response to comments of Anonymous Reviewer #2

This manuscript presents first initial results from the G6-1.5K-SAI experiment, which is a new modeling study of stratospheric aerosol injection (SAI) within the GeoMIP project. The study compares results from four different climate models to understand how injecting sulfate aerosols into the stratosphere might affect global climate. The experimental design improves upon the previous G6sulfur experiment mainly in two key ways: (1) it injects aerosols at subtropical latitudes (30°N and 30°S) instead of at the equator, and (2) it aims to keep global temperatures at 1.5°C above pre-industrial levels, which is a key target in climate policy discussions. A particularly important finding in the current study is that subtropical injection cools the Arctic more effectively and causes less reduction in tropical precipitation compared to the equatorial injection strategy used in G6sulfur. The authors have performed detailed diagnostics, and the manuscript is also written nicely with clear and informative figures. I’ll recommend a minor revision for the current version of the manuscript before its acceptance.

We thank the reviewer for their feedback and kind words. We have revised our paper appropriately; we respond to individual comments below.

My comments are given below:

1. It is unclear how multi-model ensemble means are calculated. Are all four models weighted equally despite different ensemble sizes? Additionally, there is no description of how statistical significance tests are computed (except briefly in figure captions). I suggest these details should be clearly mentioned in the Methods section.

We have added more detail to our methodology for computing ensemble means (we weight the models equally) and t-tests. Putting this information into the Methods section as stand-alone text felt awkward, so we have elaborated on our methods throughout the paper where the data are presented (e.g., Table 1 caption for ensemble means and Figure 5 caption for t-tests)

2. L115-L124 contain many numerical values describing temperature errors that are difficult to follow in paragraph. I’ll suggest adding simple visualization like boxplot to improve readability.

This is very reasonable; other reviewers also commented on our presentation of this data. We have added a plot and placed both the numbers and the plot in the supporting information.

3. L274: Eastern Pacific warming pattern in G6-1.5K-SAI - is SAI modulating ENSO?

There has been some research suggesting that SAI can affect ENSO. Given that this warming pattern is present in the warming (non-SAI) scenario, it is more likely in this case that SAI is not fully offsetting ENSO-related changes under global warming, at least not to an extent visible in 20-year temperature maps. While a detailed analysis would likely be a separate study in itself, we have added some citations to this discussion.

4. L315-L317: Authors used the precipitation centroid metric to test for ITCZ meridional shifts and find no significant displacement, then suggest other factors (Hadley/Walker circulation, ENSO) might explain tropical precipitation changes. However, this analysis examines only ITCZ position, not changes in ITCZ width or intensity. Some studies (e.g., Byrne et al. 2018) demonstrate that the ITCZ can narrow and intensify with stronger precipitation in the core and suppression at the edges without significant meridional displacement. I suggest analyzing and discussing ITCZ width and intensity to better explain the tropical precipitation responses.

This is a reasonable suggestion. As with ENSO above, a thorough analysis of ITCZ behavior might be its own study, but we agree that our analysis was extremely cursory; we have added additional diagnostics to the supporting information.

5. L32: typo error – “temepratures”

Fixed

6. Figure 5 caption: typo error – “bottom” should be “right”

Fixed

Reference:

Byrne, M.P., Pendergrass, A.G., Rapp, A.D. et al. Response of the Intertropical Convergence Zone to Climate Change: Location, Width, and Strength. Curr Clim Change Rep 4, 355–370 (2018). https://doi.org/10.1007/s40641-018-0110-5

Author response to comments of Anonymous Reviewer #3

Lee et al. present a new set of SAI simulations under the GeoMIP project, G6-1.5K-SAI as carried out in four earth system models, and contrast the results of these simulations with the earlier GeoMIP experiment G6sulfur. They assess cooling and AOD efficiency, aerosol distributions and sizes, and impacts on regional temperature and precipitation, all in significant detail and with helpful discussion of contributions to differences between models and scenarios. This is an excellent paper. The quality of figures is exceptional, the writing is clear, and the results will be of significant interest to the community. I support publication, but have a few minor comments.

We thank the reviewer for their feedback and kind words. We have revised our paper appropriately; we respond to individual comments below.

Minor comments

Line 3 - "..model, and features" rather than "..model, features"

Changed

Line 120 - consider perhaps putting these figures in a table (or supplementary).

Another reviewer said the same; we have moved these figures to the supporting information and added a figure.

Line 127 - "*we treat all of the simulations as approximately maintaining a GMSAT of 1.5◦C above preindustrial using SAI*" I'm not sure exactly what this means here? What does it mean to assume they all achieve 1.5K? Does this assumption have implications for the analysis?

This language was confusing; the message we intended to convey is that we consider all the models to have successfully maintained the target temperature, and that any deviation from the temperature target is small relative to the amount of warming being offset. As per the previous comment, most of this content has been moved to the supporting information, and the remaining text is now more clear.

Figure 2 - this is a really useful figure, and is nicely presented. It's a little difficult to make out all the lines in the MIROC case, perhaps shading rather than plotting all individual members might be useful? But I defer to the authors on design here. It would further complicate the figure, but I think differentiating the G6sulfur models here would be valuable.

Another reviewer said the same; we have modified the line properties in panels c, d, and e to make the figure easier to interpret.

Line 133 - "*the latter is governed by the models' respective aerosol microphysics, chemistry and transport representations*" .. and also by the strength of climate feedbacks in a given model, which determine the °C change per W/m2?

Agreed; added

Line 140 - "are trained on the same volcanic eruptions" - this is an important point, but is only mentioned here and as an aside, could you expand on it (including references) - perhaps in the introduction?

Yes - we have added more text and citations; we felt the discussion section at the end was the most fitting place.

Line 153 - I'm not sure about the argument here - AOD is linear so more aerosols over a small surface area vs fewer spread out over a larger area should give the same global mean? (This isn't a critical point in any case, given that it's the convolution of AOD with underlying albedo and insolation that matters)

This is a good point - we have removed this argument.

Lines 195-200 the authors might consider putting these equations in supplementary, given they are a standard calculation.

Good suggestion; we have moved these to the supplementary.

Line 211 - "optimal radius" - it would be useful to state approximately what this is here.

Added

Figure 5 - "the bottom column" should read "the right-hand column"

Fixed

This, and figures 6,7,8 are really nice, I think the clear demonstration of which comparisons are being is very useful. The variation in font sizes between the diferent parts of the figure is large though. Is not essential, but the authors might consider reducing the size of the schematic diagrams at the top of each figure. I also would lean towards labelling all subplots so people can refer to individual panels, but again, this is an aesthetic judgement which the authors should feel free to ignore.

Thank you for the suggestion; we have added individual figure panel labels, and we have made the font sizes more consistent across different parts of the figure.

Line 228 - "*Cooling is stronger over land than over the ocean, which can be attributed to the smaller heat capacity of air compared to water (Duan et al., 2019).*" Its not just the variation in heat capacity that contributes to the land/sea warming differential - see Sutton et al., 2007; Joshi et al. 2008.

Good point; we have added these citations.

Figure 9 (and associated discussion) - it might would be worth discussing potential impacts of the rate of warming in the baseline state in the different scenarios as suggested by Duffey et al. (2024) here. Their arguments could offer an explanation for the higher hydrological sensitivity under G6sulfur shown in Fig 9b. Similarly for the greater cooling over land under G6-1.5K-SAI described in the abstract.

Thank you for the recommendation; we have added more analysis here, including the recommended citation.

References

Sutton et al., 2007; Land/sea warming ratio in response to climate change: IPCC AR4 model results and comparison with observations; GRL; doi:10.1029/2006GL028164

Joshi et al., 2008; Mechanisms for the land/sea warming contrast exhibited by simulations of climate change; Climate Dynamics; https://doi.org/10.1007/s00382-007-0306-1

Duffey et al., 2024; Accounting for transience in the baseline climate state changes the surface climate response attributed to stratospheric aerosol injection; Environ. Res.: Climate; DOI 10.1088/2752-5295/ad9f91