Reviewer comments are in bold. Authors' responses are in blue.
Response to reviewer #3

This manuscript investigates the difference between simulations where solar radiation management is implemented as a reduction of incoming solar radiation and as an injection of stratospheric aerosols. The authors find that the two methods lead to a different response in the surface climate, and therefore are not equivalent in simulating geoengineering (as suggested in a few previous papers).
I have found this paper interesting, clear, and well written. I only have very minor comments.

Thank you for the kind words and for the suggestions! We have responded to each point below.

L45: I supposed with "baseline simulation" the author means a simulation of a desirable climate reached via emission mitigation. My first understanding, though, was that a baseline simulation was one without geoengineering, which would not be correct because I believe G4 also required a simulation without geoengineering.

Yes, we have corrected this to be more clear.

L49: The discriminant is not the presence of microphysics, but rather the presence of a sulfur cycle, or a way to represent the formation of new particles from sulfur dioxide. A microphysics representation allows for the evolution of particle size, which is one additional process beyond the formation of new particles.

Corrected.

L130: Shouldn't the reference be to Fig. 2f?

Yes. Fixed.

L132: what does it mean that the solar reduction and the AOD seem unrelated? In Fig. 2e you show a fit with R^2 of 0.72. The word "unrelated" should be replaced with a quantitative metric

We have changed this as suggested.

L142: eliminate "much"

Done.

L147: can you explain better how this is a feature of the current feedback controller? I understand that you refer to Tilmes et al. (2018) but I think it would be more complete if there was a not too specific explanation here without having to look for another paper

Of course. Done.

L152: I don't understand the connection between the sentences before and the conclusion that they explain why the AOD in 2050 is the same among all models.

We agree that it didn't sound very clear, and have removed the phrase.

L160: Does Solar produce any difference in the stratosphere? I imagine that reducing the solar flux would impact UV absorption by ozone (hence the temperature profile) as well as ozone concentrations

It produces minimal changes, as we have shown for CESM1 in Visioni et al. (2021). We plan to analyze some of the stratospheric changes in these models in the future.

L189: I'm really not sure how meaningful is the comparison between Pinatubo and geoengineering simulations, and I would not include it at all. As you write here, this is a sustained injection, and you already mentioned several times that Visioni et al. showed the importance of the injection seasonality on the transport. Jones et al. (2016, doi:10.1002/2016JD025001) showed the dependence of the Pinatubo dispersal from the initial conditions. What I find incorrect is that this is the only "real" point of comparison. The only real point of comparison is a simulation of Pinatubo (which all models could do - and probably have done already) vs observations of Pinatubo. That would be informative with respect to the ability of each models to simulate the transport of stratospheric aerosols.

Yes, the initial conditions are fundamental. This is also why looking at Pinatubo in i.e. the Historical simulations would also be pretty imperfect, as they did not check for the proper meteorological conditions in those simulations, for Pinatubo. There is an experimental protocol (ISA-MIP that is in the process of doing some more robust comparison, but the results are yet to come (see https://meetingorganizer.copernicus.org/EGU21/EGU21-13387.html). Our point here was just to have a pretty rough comparison between models considering the same achieved global AOD as Pinatubo. We've toned down parts of the discussion to be more clear.

L200: Is this true also if you calculate the spread as % of multimodel mean? I am not sure that the better agreement isn't simply a result of the AOD values being smaller in 4c than 4a

Yes, that remains true anyway.

L210: It would be interesting to show the optical properties (the extinction efficiency) used by each model and/or the simulated particle size distribution

We agree. Sadly, the variables needed for that were not made available by the modeling teams (as the microphysical properties of the aerosols were not the main focus of CMIP6, so they were not between the necessary variables to upload).

L214: Canty et al. (2013, https://doi.org/10.5194/acp-13-3997-2013) finds a decrease of 0.14°C globally.

Thank you for the valuable reference! We have updated the discussion accordingly.

Fig. 7: I am a bit confused by the statistics. The ensembles contain two, three at the most, ensemble members. Did you perform a T test with only 2 realizations per experiment?

We considered a set of 20 years X the number of ensemble members for the test. We fixed the caption for clarity

References

Visioni, D., MacMartin, D. G., and Kravitz, B.: Is Turning Down the Sun a Good Proxy for Stratospheric Sulfate Geoengineering?, Journal of Geophysical Research: Atmospheres, n/a, e2020JD033 952, https://doi.org/https://doi.org/10.1029/2020JD033952, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2020JD033952, e2020JD033952 2020JD033952, 2021.