We are grateful to Peter Irvine for his comprehensive review, insightful suggestions, and valuable comments, which have enhanced the quality of our manuscript and made it clearer and easier to understand. Our point by point answers to the comments are presented below. Referee comments are in bold and our replies in body text.

General comments

The authors evaluate the precipitation response to stratospheric aerosol injection (SAI) geoengineering, considering earth system model and aerosol microsphysical uncertainty. Prescribed aerosol fields were generated in an ESM with either a sectional or modal aerosol module, producing quite different aerosol properties and hence radiative forcings. These were fed into 3 different ESMs which simulated a range of combinations of CO2 and SAI injections. The fast, forcing-driven hydrological response was found to be quite different for the different aerosol modules as the modal module produced fewer, larger particles which absorbed more LW radiation. Despite being driven by the same aerosol field, the ESMs produced quite different radiative, temperature and precipitation responses. However, the largest differences in many respects arose from the microphysical representation. The study makes a detailed analysis of the various factors that shape the precipitation response to SAI, making clear that microphysical uncertainties are important.

This paper will make a substantial contribution to the literature, is generally well-written and has generally good quality analysis, and so I recommend that it be published after making relatively modest changes, outlined below.

The paper is generally clear and well-written, but the argument was a little hard to follow in places as the paper jumped back and forth between radiative forcing and precipitation several times. For example, section 4.4 is titled "simulated precipitation response…" but the opening page is about the reasons for a radiative mismatch. The authors may consider revising the order of analysis.

To make results section more clear, results section is divided into two subsection:

"4.1 Quantifying fast and slow responses from regression simulations"

"4.2 Results of climate equilibrium simulations"

First two paragraphs in prior section 4.4, which were describing climate equilibrium simulations and how SAI-CO2 pairs are chosen, are moved under new section 4.2.

Section 4.4 is now divided to two subsubsections:

"4.2.1 Global mean temperature change in climate equilibrium simulations"

and

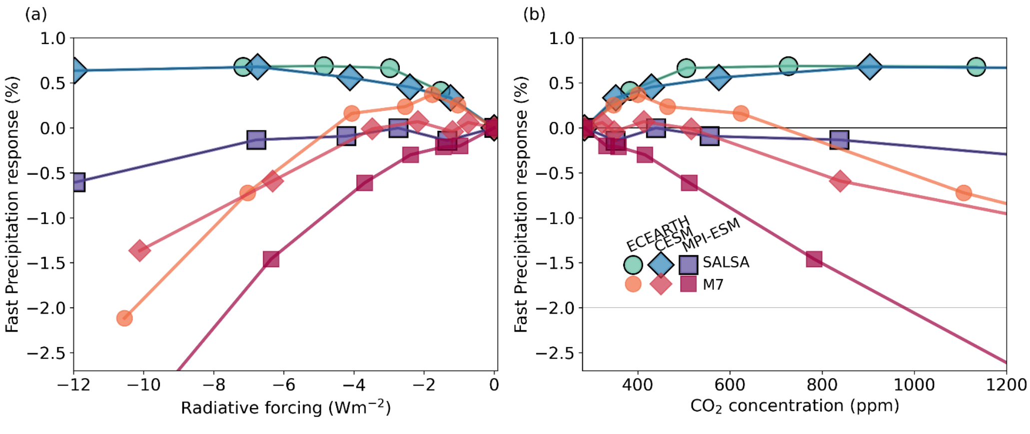
"4.2.2 Simulated global mean precipitation change in climate equilibrium simulations"

The figures and analysis are generally very good, but in places the analytical choices made things a little difficult to follow, e.g., Figure 6 was particularly challenging. I've made a series of suggestions for improvement in the specific comments below.

I was left not quite knowing the answer to a question that I think could help increase the impact of this study and I think that with a little work could be easily answered. There is a factor of ~2 difference in the SO2 amount needed to achieve the same cooling for the sectional and modal aerosol modules. This made me wonder: is the residual precipitation, or just fast precipitation, difference ~2x larger as well? Or does the fast effect of CO2 dominate this residual?

We have included two new figures in the supplement to answer these questions. In these figures we focus on the uncertainties caused by SAI and exclude the impact of CO2 in these figures, as the main focus of this study is on the impact of SAI.

Fig.6 results were for simulations where magnitude of SAI is adjusted to compensate for CO2 induced warming. There, CO2 fast response dominates the global mean precipitation, but the SAI precipitation component is significant in most cases. To show this, the following figure is added to the supplement, which shows the fast precipitation component of SAI in scenarios shown in Fig. 6b. So for example, the fast precipitation component of SAI varies from -0.9 to +0.7 % depending on the aerosol model and the ESM in cases where the warming is caused by 600 ppm CO2. The CO2 fast precipitation component for 600 ppm CO2 is -3.0 - 1.9 % depending on the ESM (see Fig 5). In addition we have included a figure showing the fast precipitation response of SAI with radiative forcing as x-axes (left Figure below). As this figure shows, the connection between fast precipitation response and radiative forcing is complex in individual models, but generally uncertainty (difference in results between models) increases with larger injections. This figure also justifies some of our analytical choices, which will be discussed in more detail later in the specific comment.



Related to the new supplement figure, the following text is added to manuscript in the first paragraph of section 4.2.2: *"...and the differences between aerosol models become more pronounced with higher injection rates. These differences among aerosol model results are even more apparent when the fast precipitation response is presented as a function of radiative forcing. For SALSA aerosols, a lower injection rate can achieve the same level of radiative forcing as M7, resulting in more significant differences in fast precipitation responses (see supplement figure Fig. S7a)."*

And in section 4.1.3 it now reads: “*As illustrated by Fig. 5b and Supplementary Fig. S6, the fast precipitation response to a quadrupling of CO2 levels varied significantly, ranging from a decrease of 3.38% in the EC-Earth simulations to an increase of 5.6% in the CESM simulations. However, the fast precipitation response to SAI accounts for differences of up to 3.5% in global mean precipitation, as illustrated by the fast precipitation response component in Supplementary Figure S7b.*”

Another new figure is related to the following comment and shows how uncertainties depend on injection rate or radiative forcing:

More generally, could the authors comments on the relative scale of the precipitation differences compared to this injection amount? RMSE difference might be a simple metric that could be calculated to test this. Some take-away claim that relates these 2 key elements would make the paper more memorable and useful to the community.

New figure (S8) on standard deviation is added to supplement:

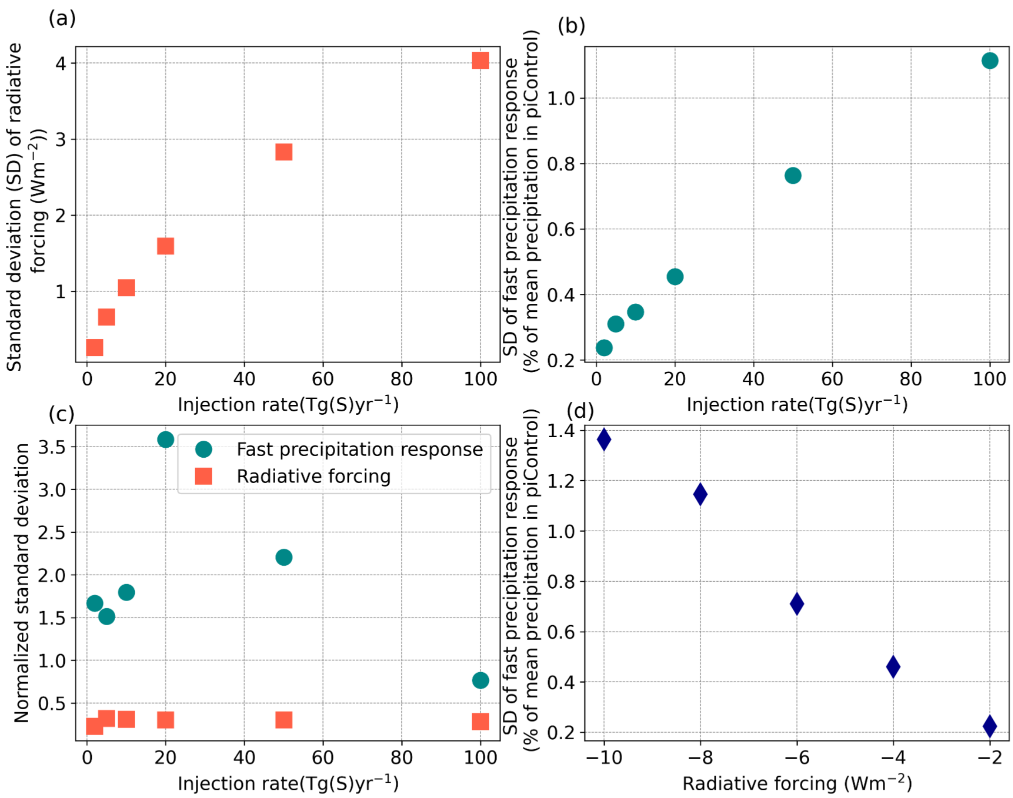


Figure S8. Standard deviation (SD) of simulated a) radiative forcing at TOA and b) fast precipitation response as a function of injection rate, c) the coefficient of variations of radiative forcings and d) fast precipitation responses and SD of fast precipitation responses as a function of injection rate. Since specific radiative forcings marked in d) were not simulated, the fast precipitation responses are estimated based on the two closest simulated radiative forcings for each model.

Following text is added to the manuscript to section 4.1.2:

However, the standard deviation of the simulated fast precipitation response between model combinations is rather linear with respect to the injection rate and the simulated radiative forcing (see Supplement Fig. S8). This means that the differences in the simulated fast precipitation response between models become larger with larger injections.

Specific comments

L14 – reduction relative to what?

Line now reads: "*In equilibrium simulations, where aerosol injections were utilized to offset the radiative forcing caused by an atmospheric CO_2 concentration of 500 ppm, the decrease in global mean precipitation varied among models, ranging from -0.7% to -2.4% compared to the preindustrial climate*."

L16 – "rather negatively correlated" – why not just negatively correlated? And could you clarify what is meant by "absorbed radiation" here? Is that a new finding or a widely established result that you are referencing?

Word *"rather"* is replaced by "i*s shown to be "*. The relation between fast precipitation response and atmospheric absorption is not a new finding. It is already shown e.g in here: Samset, B. H., G. Myhre, P. M. Forster, Ø. Hodnebrog, T. Andrews, G. Faluvegi, D. Fläschner, M. Kasoar, V. Kharin, A. Kirkevåg, et al. (2016), Fast and slow precipitation responses to individual climate forcers: A PDRMIP multimodel study, *Geophys. Res. Lett.*, 43, 2782–2791, doi:10.1002/2016GL068064

Good point related to *"absorbed radiation*", because this claim in the manuscript was actually wrong. It is not correlated with absorbed radiation, but absorbed radiation by the atmosphere. "absorbed radiation" is replaced by "*atmospheric absorption"*.

L30 – relative to what?

This is now rewritten as: *"Thus, this would lead to several consequences such as a decrease in global mean precipitation and unevenly distributed temperature chance compared to climate without increase in CO2 and SAI"*

L31-34 – review phrasing.

These lines and the next sentence are now rewritten as: *"The extent of these impacts is influenced not only by the level of GHG increase in the atmosphere but also by the interaction of aerosol fields with SW and LW radiation. This interaction is further dependent on the aerosols' optical properties, which, as demonstrated in Laakso et al. (2022) study, are closely associated with the modeling of aerosol microphysics in climate models."*

L43 – clarify whether the same 2 aerosol modules were used in the 3 different models.

This now reads: "In Laakso et al. (2022) (from now on referred as Part 1), we simulated different injection rates using both a sectional aerosol model SALSA and a modal model M7 within same climate model ECHAM-HAMMOZ, showing that…"

L50 – might be nice to indicate roughly the fractional changes here.

This now reads: “This means that in larger injection rates the contribution of LW radiation to total radiative forcing becomes larger: In SALSA simulations LW radiation forcing compensated for between 10% to 28% of the SW radiative forcing with injection rate of 1-100 Tg(S)yr-1 while M7 simulation the range was 24-57%.”

L23-50 – Might be worth indicating which aerosol scheme performs better at reproducing observed volcanic response if that can be determined, i.e., is the SALSA sectional model better but more expensive and M7 the poor-man’s alternative?

Following discussion is added to text:

The situation is further complicated by the lack of clear criteria for selecting the appropriate aerosol model. Observations following the 1991 Pinatubo eruption have frequently been utilized as a benchmark for evaluating models' ability to simulate stratospheric aerosols. However, a single sulfur injection, as in the case of Pinatubo, differs significantly from continuous injections in case of SAI. Notably, there is a minimal difference between the M7 and SALSA model results in the simulations of the Pinatubo eruption, as detailed in Kokkola et al. (2018). Simulations using the M7 model were 60% faster than those with SALSA, but, there were some numerical limitations associated with the modes in M7, which restricted the aerosols from achieving an optimal size range for effectively scattering radiation, as noted in Laakso et al., (2022). However, the performance of the M7 results is also sensitive to the configuration of the modes, making it difficult to predict which setup will perform well, as the performance depends on the simulated case (i.e volcanic eruption vs. SAI, different injection strategies for SAI).

L59-60 – Does this apply in the same way to stratospheric heating as it does to tropospheric? Is stratospheric heating as effective as tropospheric heating at suppressing precipitation? If the absorption occurred up in the mesosphere, I imagine it would have little effect on the hydrological cycle.

Based on Fig 4a, it does apply (e.g. CO_2 (affects both the troposphere and the stratosphere) and SAI (stratospheric) impact are similar - the dependence between absorption and precipitation is the same). This is how we see that it can be understood:

The atmosphere has a relatively low heat capacity, meaning it quickly adjusts to changes in energy fluxes. This adjustment ensures a balance where incoming and outgoing energy fluxes in and out from the atmosphere are equal. When some factor increases the absorption of energy in the atmosphere (i.e., the energy flux into the atmosphere), it rapidly adapts to this and will have a new equilibrium. This adaptation involves an increase in temperature, which leads to more longwave (LW) radiation being emitted, and/or a decrease in latent heat flux, seen as reduced precipitation.

Atmosphere can be separated into two layers: the stratosphere and the troposphere which both have to be in equilibrium in respect to energy in a relatively short time scale. In these simulations absorption of the stratosphere is increased due to aerosols. Since the stratosphere primarily exchanges energy with adjacent layers (and thus the troposphere) through radiative fluxes, it must warm up to emit more radiation and achieve a new equilibrium. This warming results in more radiation being emitted towards the troposphere, thereby increasing the incoming LW radiative flux to the troposphere. Consequently, the troposphere adjusts to this new influx by decreasing its latent heat flux, finding a new balance in the process.

Niemeier et al. (2013) investigated the impact of different SRM techniques acting at different altitudes. They made a similar comparison as we have done here, comparing predicted precipitation (mainly based on absorption) and simulated precipitation. As these results were in good agreement, this suggests that it does not matter at what height the absorption occurs.

In the introduction there now reads: *“Niemeier et al. (2013) investigated the impact of different SRM techniques applied at different altitudes. Their findings show that the precipitation predicted by Equation 1 aligns closely with the precipitation observed in simulations. Changes in sensible heat flux within their simulations were minimal, suggesting that the calculation of precipitation based on atmospheric absorption is not influenced by the altitude at which the absorption change occurs.”*

Please also note replies for another reviewer.

L64-66 – perhaps note T-driven intensification under GHG case?

These lines now read:

”In the case of solar radiation modification generally, the unambiguous impact of this is seen in model simulations, in cases where the GHG-induced radiative imbalance is fully compensated by SRM. Without SRM, there would be an increase in global mean precipitation, driven by a rise in temperature. However, if the temperature increase is offset by SRM, it results in overcompensation and decrease in global mean precipitation”

L75 – formatting of citations.

Fixed

L78 – in the consequent precipitation responses.

“followed” changed to “consequent”

L103 – add resolution in degrees.

Resolution in degrees is added to line: *“ The resolution of atmosphere used in MPI-ESM, CESM and EC-Earth simulations are T63L47 (1.9◦ x 1.9◦), finite volume 0.9◦x1.25◦ and 32 vertical levels and T255L91 (0.70◦ x 0.70◦) respectively”*

L149 – from a preindustrial baseline with GHG and SAI perturbations applied?

Line changed to: *“The regression simulations with ESMs were started from a preindustrial baseline with GHG and SAI perturbations applied”*

Figure 1 – Great figure! Small suggestion: 6x climate responses instead of impacts.

Thank you! *“impacts”* changed to *“responses”*

L162 – logarithmic fit

“logarithmical” changed to *“logarithmic”*

Figure 2 – Another great figure. Wondered if it might make sense to use the shape to match models, e.g., diamonds = CESM. This might help the colorblind to follow along. Looks like that was done in Figure 3, but I’d suggest adding the shapes to the legend or caption.

Shapes are now matching with ESMs, simulations where SALSA aerosols are used are now surrounded by black edges and legend is changed in Fig 2,3,4,6 (shapes are changed also in Fig3). Example of the new “style” is seen in the first figure above which were added to the supplement. This was a good suggestion and now it is much easier to distinguish different scenarios.

L206 – will have changed when it does settle down?

This line now reads: *“...it is possible to estimate how much global mean temperature will have changed when it does settle down in the new radiative balance after SAI is started…”*

L205 – 213 – a little repetitive.

We feel that this part of the text still provides information which has not been mentioned elsewhere in the manuscript.

L245-249 - phrasing a little unclear.

These lines are now rewritten as: *“Thus, when the effective climate sensitivity is calculated based on 150-year simulations, the sensitivity appears higher in the CESM model compared to the other two ESMs. However, this difference is not as pronounced when using 20-year simulations. This characteristic in the CESM results was discussed in Bjordal et al., 2020. It was identified that the increased sensitivity is due to a negative feedback mechanism, which involves a reduction in ice content within clouds in a warming climate. This feedback mechanism becomes less substantial when the climate has warmed sufficiently.”*

Figure 4 – Is it best to compare injection mass for Salsa and M7 directly in this way? I found myself a little confused until I remembered that 50Tg in Salsa has a much greater cooling effect than in M7. Perhaps some additional text or analysis could clarify this, e.g., normalizing the fast effect by the expected cooling magnitude or plotting against an x-axis that shows temperature or RF?

We think that this is dependent on the perspective of the reader. You are correct in saying that for certain types of "end-users," it might be more useful to understand the extent of precipitation changes associated with a specific degree of cooling (radiative forcing), which is often the premise for consideration of SAI. This was the perspective we chose to follow e.g in figure 6 where CO_2 concentration is chosen as x-axis. However, in this instance, we adopted a more of a climate modelers' perspective, where the same perturbation (specific injection rate) is applied to different models to compare the variations in their responses. Using injection rate as x-axes is also inline in earlier figures related to temperature and forcing as well as figures in Part 1.

Figure 4 is just for the explanation responses seen in Fig3b. Thus we see that x-axes should be the same here. However, as said in our earlier comment, we added a new figure in the supplement, where fast precipitation response is shown as a function of radiative forcing and this was commented on in the text.

L295-296 – Would this non-linearity disappear if the axis was RF instead?

Please see our earlier comment.

L347 – less precipitation = a greater reduction in precipitation relative to the baseline?

Changed as suggested

L350 – link back to earlier claim on reduced SO2 for same RF in EC-earth?

We added *“This is due to two factors in EC-Earth simulations: the smaller magnitude of the fast precipitation response to CO2 (as shown in Fig. 5b) compared to MPI-ESM and CESM, and the more positive fast precipitation response to SAI when the injection rate is adjusted to match the radiative forcing of CO2 (refer to Fig. S7b).”* to text.

Figure 6 – Is this the best way to get this information across? I’m very confused by some of the analytical choices and by how complex it is. Why aren’t the points falling on the precise CO2 ppm values used before? Can the analysis be flipped so that they do?

If these kind of simulations are done for certain CO2 ppm and SAI pairs, where forcings of these two compensate each other, there are two choices how to proceed:

a) Choose specific CO2 concentrations and adjust injection rate to correspond forcing of CO2

or b) Choose specific injection rates and adjust CO2 concentration to correspond forcing of injection rate

We have chosen to proceed with the second option. As Figure 4 demonstrates, both the radiative forcing and the fast precipitation response exhibit a logarithmic dependence on CO2 concentration. Therefore, it is straightforward to calculate values between simulated CO2 levels. However, adjusting SAI levels is not as simple, particularly for the fast precipitation response. In this case, fitting any simple function to the simulated points is challenging, as can be seen in Figure 2b or the new Figure S7 in the supplementary material.

This was commented earlier on in the first paragraph in section 4.4. The following text is also added now to caption of Fig6: *“Based on the logarithmic relationship between radiative forcing and fast precipitation response to CO2 concentration (as shown in Fig. 5), the CO2 concentration and the subsequent fast precipitation response can be determined from the logarithmic fit so that the radiative forcing aligns with the simulated radiative forcing for SAI.*”

(continuation related to fig6) **More information needed on c, to clarify modelled pairs. Panel d seems like it could have been a whole multi-panel figure of its own. I also**

wonder if a pure temperature adjustment is the best choice, couldn't you also scale up or down the fast effect of SAI by the fractional change in cooling that's needed? Presumably that would give a better fit.

In addition to the new line in above to caption fig6 we modified the caption: *"c) Simulated changes in a) global mean temperature and b) precipitation under SAI - CO2 pair scenarios (as illustrated in a), assuming a state of climate equilibrium."*

We acknowledge that d) includes a lot of information and takes some effort to comprehend. However, to compare fast responses, modeled precipitation, and temperature adjustments easily they need to be in the same panel. An alternative approach would have been to allocate separate panels for each Earth ESM, but our aim was to facilitate direct comparisons of actual simulated results between models.

You are correct about adjustments. A pure temperature adjustment would not be enough if we would like to estimate what would be "real" precipitation change in the case without temperature change at all. However, here we want to show that the assumption on total precipitation based on fast responses in Fig6 b) did not correspond to the modelled values mainly because of an unexpected temperature change. I.e precipitation change should be $\Delta P = a*\Delta T + P_{fast\text{-}CO2} + P_{fast\text{-}SA}$ste, where we did not take into account "slow response" ($a*\Delta T$). By removing "slow response" from the simulated precipitation values, there should be only fast responses of SAI and CO2 left and it should correspond to the calculated precipitation values in Fig 6b. Even though there was temperature change in actual simulation, the fast effect of SAI is still the same. Scaling might become a point of discussion if we aimed to estimate the precipitation levels in a hypothetical equilibrium state without temperature changes. However, that was not our intention here. In addition, as previously mentioned, scaling the fast precipitation response is not straightforward due to the nonlinear relationship between fast precipitation response and radiative forcing.

"Adjusted by hydrological sensitivity" might have been a bit misleading and now *"Adjusted by hydrological sensitivity"* in the legend to *"slow (temperature) response removed"*.

L355 – conversely? should that be Additionally?

Yes it should. Fixed

355-360 – this suggests switching axes on Figure 6, as CO2 is the dependent variable.

You're correct. However, our intention was to adopt a slightly different perspective and illustrate the significant uncertainties involved in mitigating CO2-induced climate change through SAI. We aim to highlight how these uncertainties, particularly the differences in simulated values, escalate with the magnitude of climate change.

4.4 – Given the first page is about the radiative mismatch, should this be 2 sub-sections? And should the radiative discussion come here or earlier? This might help with the flow of the article.

This section is now separated to two sections: “*Global mean temperature change in climate equilibrium simulations”* and “*Simulated global mean precipitation change in climate equilibrium simulations”*

L392 – global mean precipitation is more positive?

“larger than” changed *“more positive than”*

L393 – here you are referring to the effect after the fast effect, whereas in some studies it is meant to include the total effect.

“*…than the estimated ones (which did not take into account precipitation change due to the hydrological sensitivity and change in the temperature)”* is changed to: *“..than those estimated from the sum of fast precipitation responses”*

L398-400 – I think making the correction I suggested and noting that the forcing mismatch produced this precipitation mismatch might lead to a more useful conclusion here.

It was a good and justified suggestion. However we decided to keep the figure and these lines as it is. As replied to earlier comment, here we want to compare $\Delta P = a * \Delta T + P_{fast\text{-}CO2} + P_{fast\text{-}SAI}$ to actual simulated precipitation. Even though our assumption that there would not be temperature change was wrong for CESM and EC-EARTH, the fast precipitation components should still be the same.

L403-420 – Isn’t a big driver of the overcooling / residual warming seen in many stratospheric aerosol geoengineering experiments the distribution of aerosols? Might be useful to refer to that distribution here and remind the reader that it’s the same in each model (I may have forgotten myself by this point).

Yes, the distribution of aerosols significantly contributes, but overcooling or residual warming is also observed in experiments where the solar constant is reduced (e.g Schmidt, et al., (2013)). This outcome is somewhat anticipated when there's a fractional reduction in solar radiation coupled with an increase in well-mixed CO2 and because the average gradient between high and low latitudes is steeper for solar radiation than for thermal radiation. Although the overcooling/residual warming in the case of solar constant reduction was much smaller than the SAI based on e.g Visioni et al. (2021).

We include the following text to the manuscript:

“Laakso et al. (2022) demonstrated that the radiative forcing from SAI is primarily concentrated around the Equator for aerosols simulated using both SALSA and M7 models. There was also significant clear-sky zonal forcing observed at the latitudes of 50◦N and 50◦S. However, the presence of clouds in these regions reduced the aerosol all-sky radiative forcing. Aerosol optical properties were consistently applied across all three ESMs, but variations in cloud cover and properties among the ESMs can lead to differences in the actual radiative impact of aerosols.”

Schmidt, H., Alterskjær, K., Bou Karam, D., Boucher, O., Jones, A., Kristjánsson, J. E., Niemeier, U., Schulz, M., Aaheim, A., Benduhn, F., Lawrence, M., and Timmreck, C.: Solar

irradiance reduction to counteract radiative forcing from a quadrupling of CO_2: climate responses simulated by four earth system models, Earth Syst. Dynam., 3, 63–78, https://doi.org/10.5194/esd-3-63-2012, 2012.

Visioni, D., MacMartin, D. G., Kravitz, B., Boucher, O., Jones, A., Lurton, T., Martine, M., Mills, M. J., Nabat, P., Niemeier, U., Séférian, R., and Tilmes, S.: Identifying the sources of uncertainty in climate model simulations of solar radiation modification with the G6sulfur and G6solar Geoengineering Model Intercomparison Project (GeoMIP) simulations, Atmos. Chem. Phys., 21, 10039–10063, https://doi.org/10.5194/acp-21-10039-2021, 2021.

Figure 7 – maybe a note on how these pairings were chosen. It might be useful to extend the y axis and add a global mean temperature residual value to the legend.

Text *"In these simulations, the CO2 concentration was adjusted to counterbalance the radiative forcing from a specific injection rate, as determined by regression simulations."* was included in the caption of Figure 7.

Figure 7 was modified as suggested and in the caption it now reads: *"dT in the legends shows residual global mean temperature"*

Figure 8 – A bit difficult to read, would adding figure wide column and row labels make it easier to parse? You might also consider rearranging so that SALSA is as one block, M7 as another.

SALSA and M7 are moved to their own blocks and the aerosol model label is moved to the side of the figure. ESM is now the label for the whole row. The title of each panel includes only the injection rate and CO2 concentration and the residual global mean temperature. "-piControl" is removed from the panels.

Figure 9 – missing labels. Panel a is quite difficult to read, some for previous figure. Is there another way to show this?

We acknowledge that panel a is somewhat challenging to read. However, we have not come up with an idea of a better method of presentation, so the figure has been retained in its current form.

Figure 10, same comment as 8.

Modified as figure 8.

489-494 – not particularly clear or particularly logical flow at the end of this paragraph, consider revising.

We removed the line *"Thus it is important to bear in mind when interpreting these results, but also in general, not to assign excessive importance to the quantified effective climate sensitivity of individual models, as it is sensitive to external factors (e.g., forcing agent and simulation period)."*

from 489-> and end of this paragraph it now reads:"*Overall, drawing from these results and a comparison with the climate sensitivities reported in Zelinka et al (2020), it should be kept in* *mind that effective climate sensitivity is not a straightforward parameter. Its interpretation is complicated by its sensitivity to external factors, such as the type of forcing agent (which affects shortwave vs. longwave radiation) and the length of the simulation period (e.g., 20 years vs. 100 years). Moreover, sensitivity to these external factors varies across different models."*

504-505 – compared to what? Is the comparison to the baseline the most relevant? Should it be to the 500 ppm case? Given the amount of SO2 injected scales with CO2, this difference in injection amount should modulate that total precipitation response, which as a consequence shifts the net result.

Now it reads: *"In the aforementioned 500ppm atmospheric CO2 concentration and SAI scenario the resulting reduction compared to preindustrial climate in global mean precipitation ranged from 0.7% to 2.\% between different model combinations."*

We agree that it probably would be more convenient to compare a situation without SAI (500 ppm) than a pre industrial climate. However to simulate precipitation change in 500 ppm would need a very long simulation where climate was near new climate equilibrium.

L513-514 – See my earlier comment about making a full adjustment, i.e., what would have occurred if the correct amount had been chosen to keep temperature constant, rather than just the temperature adjustment (which excludes the change in fast forcing effect).

In addition to our previous response regarding the complexity of scaling the fast precipitation response to correct for the necessary radiative forcing to eliminate residual temperature change, estimating the required radiative forcing is not straightforward. As demonstrated by these simulations, it was not feasible to counteract warming by adjusting CO2 radiative forcing to offset the effects of SAI. Furthermore, the Gregory plots provided in the supplementary materials for these 'climate equilibrium simulations' do not show a distinct 'residual radiative forcing'. Therefore, making a comprehensive adjustment would likely necessitate the use of some form of feedback function in the simulations to correct the CO2 levels (or SAI).

L495-513 – Here or elsewhere some comment on the relative scale of the precipitation differences compared to the required injection amounts would be useful. M7 suggests ~2x greater sulphate required, is the gross or net precipitation difference 2x greater too?

There is now a new figure in the supplement (Fig. S8, see our earlier reply) showing the standard deviation of the fast precipitation response as a function of radiative forcing. It does indeed show that the differences in fast precipitation responses increase relatively linearly as a function of injection rate. Comment on this is added to section 4.1.2 (see our earlier reply).

518 – more negative?

"lower" changed to *"more negative"*

530 – consistently more negative?

“lower” changed to *“more negative”*

538 – perhaps remind reader that they faced the same change in aerosol optical properties

Now it reads: *“Relatively minor differences in the radiative forcing of SAI in Fig. 2, in spite of the implementation of identical optical properties, and small differences..”*

543-547 – a little hard to follow.

Now it reads: *“For example, while the variation in LW ESMs was minimal, the reduction in SW absorption was 0.12-1.57 W/m2 smalle in simulations using MPI-ESM compared to those conducted with CESM and EC-Earth. Consequently, the total absorption in MPI-ESM simulations was greater than in the other two ESMs, particularly at higher injection rates. This led to a more pronounced negative fast precipitation response in MPI-ESM relative to the other two models”*