We thank Anonymous Referee #1 for suggestions and comments which improved the manuscript. 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 studied the climate responses to two different geoengineering approaches: sulfur injections and carbon dioxide removal. Using the Gregory regression approach, the authors separated temperature-independent (fast adjustment) and temperature-dependent (slow responses) components and then quantified their contributions to the total precipitation change in an RCP based scenario. The authors also compared the total amount of aerosols needed under various scenarios and the spatial pattern changes. The paper is well written and contributes to our understandings of the hydrological cycle responses to different geoengineering forcing. I would recommend publication after the authors addressed the following comments.

Specific comments

My understanding is that during the simulations using MPI-ESM and CESM, you directly used the "equilibrium" aerosol distributions patterns estimated from mECHAMHAMMOZ under the 1, 2, 3, 4, 5, and 6 Tg(S)/yr emission rates (in Fig.8 you used discrete injected sulfur rates). In the real world, if you change the emission rate of aerosols in year N, it takes time to adjust to a new equilibrium state. Therefore, there is a difference between your simulations and the real-world implementations (maybe it's worth to point out?). Do you have any idea how much the difference could be?

This is true and was briefly mentioned in P7 L195, but we agree that this needs to be discussed more.

Thus in section 2.2 the original sentence :

"*An approximation inherent in this approach is that transitory ramp-up and ramp-down periods in the stratospheric aerosol burden with 1 Tg(S)/yr changes in SRM are not taken into account*."'

is followed by:

"Thus the simulated SRM changes take place faster than would occur in the real world. For example, the ECHAM-HAMMOZ simulation with 5 Tg(S)/yr injections requires 6 months to achieve 70% of the ultimate steady state aerosol optical depth (AOD) (533nm) after starting from background conditions.. When sulfur injections are suspended in the ECHAM-HAMMOZ simulation, the AOD decreases by roughly by 40% over the course of the first year. However, since the sulfur changes in our ESM simulations are only ±1 (Tg(S)/yr)) and do not usually occur in consecutive years we can assume that neglecting this time lag does not significantly alter our overall results."

The discussion of regression in Section 3 is good. But in general, I think that the authors could add more information to support their statements and help the reader to better understand. For example, it would be very helpful if the author could show numbers at least for those regression slopes. It's hard to identify and compare these regression lines

with statements in the paper by eyes. Another example is shown in lines 258 to 260: the authors stated that "There was no large difference in surface albedo change between models" and "clear-sky SW absorption was linearly dependent on surface temperature". Can you remind me where I can find figures or data for such statements?

We have now added the regression slopes to figures 2-4. In addition, we have now added similar regression figures for clear sky shortwave absorption and albedo changes to the supplemental material.

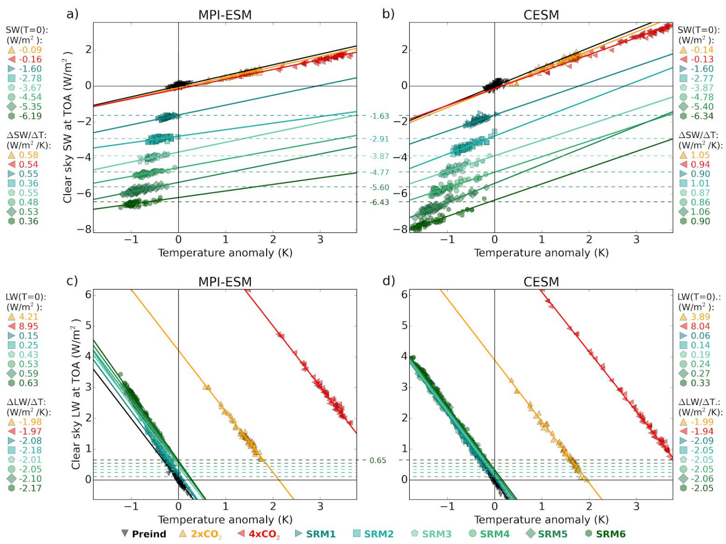

Figure 2: Gregory plots of the...Corresponding radiative forcing (intersection of linear fit and the y-axes (T=0)) in MPI-ESM and in CESM are shown in top and slope of the linear fit in bottom of legends next to the figure...

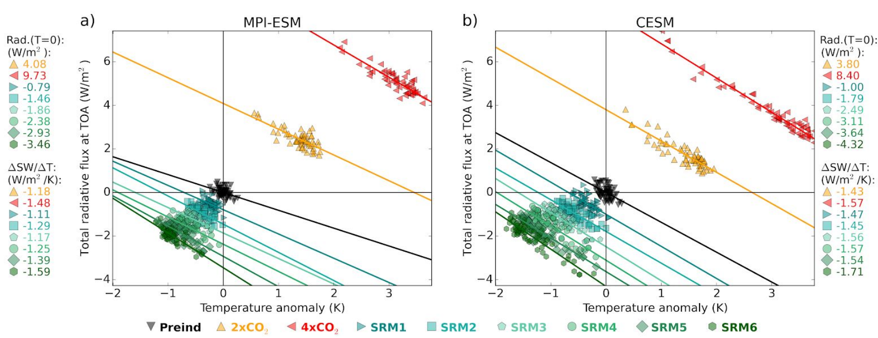

Figure 3: Gregory plots of the...Corresponding all-sky radiative forcing (intersection of linear fit and the y-axes (T=0)) in MPI-ESM and in CESM are shown in top and slope of the linear fit in bottom of legends next to the figure...

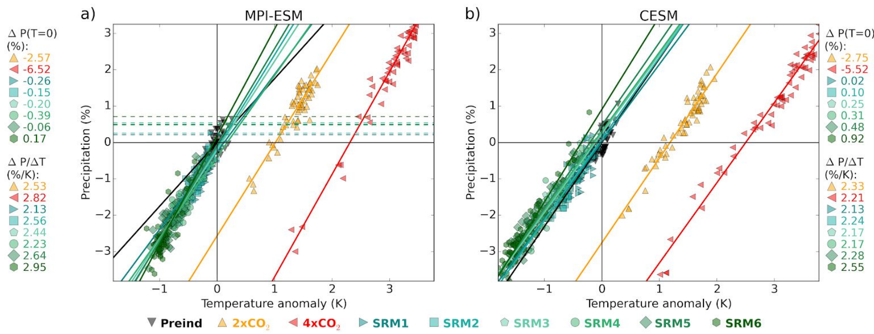

Figure 4: Gregory plots of the...and slope of the linear fit (shown in bottom of the legends) corresponds the slow response due to the temperature change...

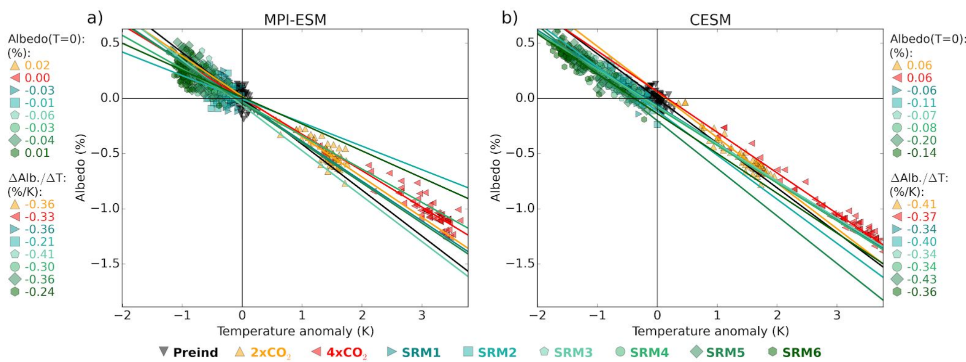

Figure S1: Gregory plots of albedo change for a) MPI-ESM and b) CESM. Markers indicate a single-year global mean value for one ensemble member and solid lines are linear fits. Origin represents zero temperature and albedo anomaly compared to the Preind simulation.

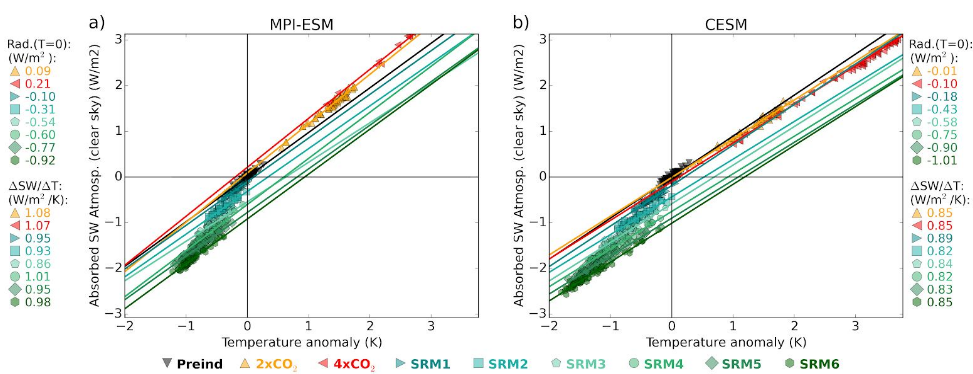

Figure S2: Gregory plots of clear sky atmospheric absorption change for a) MPI-ESM and b) CESM. Markers indicate a single-year global mean value for one ensemble member and solid lines are linear fits. Origin represents zero temperature and atmospheric absorption anomaly compared to the Preind simulation.

For each of the two models, the authors used regression results from three 20-year simulations (preindustrial, 2xCO2, and 4xCO2) and applied a logarithmical fit to estimate the coefficient "c" (the fast CO2 effect to precipitation) (lines 362 to 364). You have only three data points during the curve fit, so I am wondering how good the curve fit is? The fast precipitation response to CO2 change not only relates to the CO2 radiative effect but is also partly attributed to the physiological effect. It's fine to use the logarithmical form (I think the first paper used this relationship is in Cao et al., (2015)), but I think you might want to point it out here.

This is a good comment and we agree that this should be pointed out. Using the logarithmical form requires the assumption that fast response depends linearly on absorbed radiation, but this was not shown or discussed. Based on e.g figure 2 in Samset et al 2016 this dependence is fairly linear, but some deviation occurs which is caused by, as the reviewer pointed out, physiological effects and also sensible heat flux. To address this comment we have now added a figure similar to that in Samset et al 2016, but using data from our simulations:.

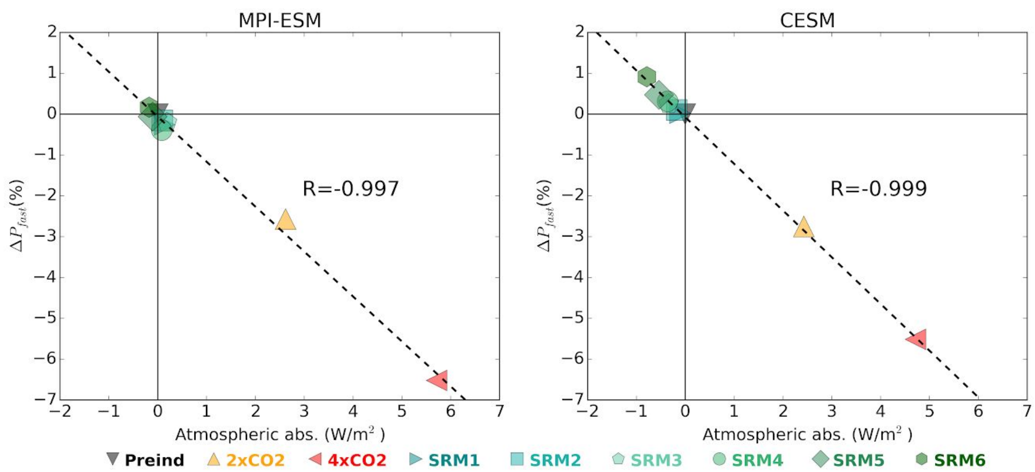

Figure S3. Regression of fast precipitation response versus atmospheric absorption in a)MPI-ESM and b) CESM. R is the Pearson correlation coefficient.

Lines 352-363 have now been rewritten to discuss this further:

"Based on our component analysis simulations we see that the fast precipitation response varies fairly linearly with absorbed radiation (See Fig. 3 in supplementary), but some deviation occurs due to changes to sensible heat flux and physiological responses of vegetation (DeAngelis et al., 2016). This result is consistent with that of Samset et al. (2016) and Myhre et al. (2017). The higher correlations in our simulations compared to Samset et al. (2016) may be due to the use of the fixed Sea Surface Temperature (SST) method to define the fast response in Samset et al. (2016): fast responses quantified with fixed-SST methods include land temperature adjustments.

Radiative forcings are generally assumed to be additive (Marvel et al., 2015). If we assume based on supplementary Fig. S3 that the overall fast response depends only on absorbed *radiation, it follows that the fast responses of individual forcing agents are also additive. In Sect. 3.3 we also showed that the slow temperature-dependent component does not depend on the applied forcing. We can thus describe the global mean precipitation change as the sum of the temperature-dependent slow component (a×ΔT) and all fast components (Fläschner et al., 2016):*

$$\Delta \mathrm{P} = a \times a\Delta \mathrm{T} + b(\mathrm{SRM}) + c \times \ln \frac{\mathrm{CO_{2\ preind}} + \Delta \mathrm{CO_2}}{\mathrm{CO_{2\ preind}}} + \mathrm{BG},$$

where a, and c are model-specific coefficients, b is a function of the SRM level, T is the simulated global mean surface temperature, $CO_{2\ preind}$ is the preindustrial CO2 concentration, ΔCO_2 is the atmospheric CO_2 change relative to the preindustrial value, and BG is the background fast component, assumed to be the same for all scenarios. Coefficient a is obtained from the scenario-ensemble mean slope in Fig 4 (2.53 %/K for MPI-ESM and 2.27 % for CESM), while b is the fast component (intercept) from simulations of the corresponding SRM scenario (see ΔP(T=0) values in Fig 4). To calculate coefficient c, we again assume that fast precipitation response is linearly dependent on absorbed radiation. Radiative forcing due to CO_2 varies logarithmically with concentration (Etminan et al., 2016) and thus the fast precipitation response for CO_2 is also assumed to be logarithmically dependent on CO2 concentrations (see supplementary material Fig. S4)"

We have also added a figure demonstrating this logarithmical fit to supplementary material (Fig 4).

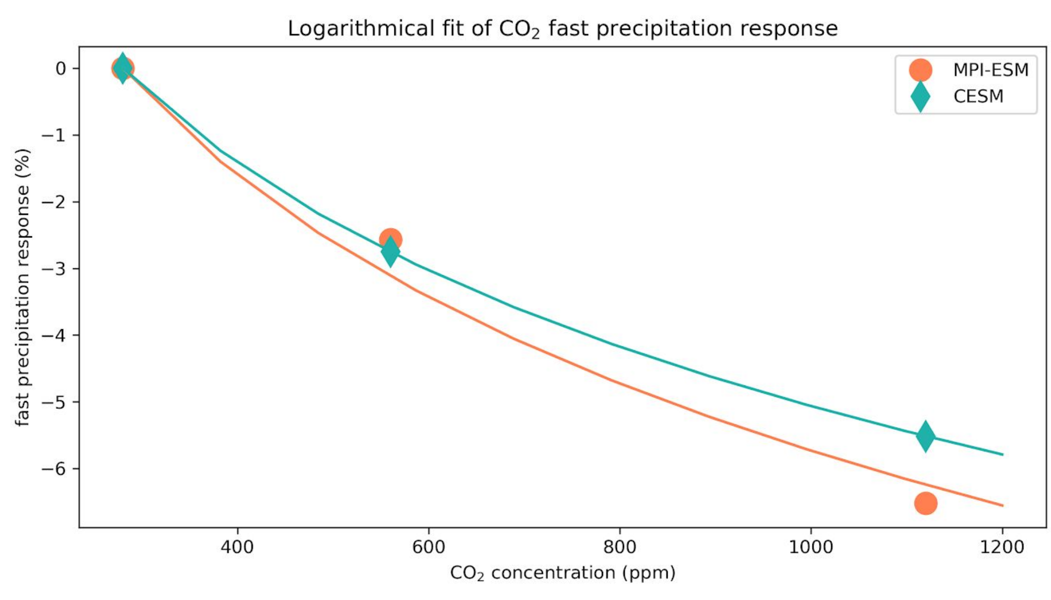

Figure S4. Logarithmical fit of CO_2 fast precipitation calculated from preind, $2xCO_2$ and $4xCO_2$ scenarios.

Technical corrections

The following technical corrections have been made.

Line 34: Add "increase" to "maintain the global mean temperature increase within 2◦C . . ."

Line 39: When the author says "a reduction of energy use", do you mean energy use associated with fossil fuel burning only? Because energy consumption provided by renewables and biofuels will not deteriorate the climate warming target.

We rephrased the sentence to "*would require fast and significant reduction in use of fossil fuels complemented with carbon dioxide removal.*"

Line 47: Missing ")" here: "(Luderer et al., 2018, IPCC 2018)"

Line 67: ". . . SRM would decrease global mean precipitation through the direct radiative effect described above". The hydrological cycle responses to solar geoengineering depend on magnitudes of geoengineering deployments and which case you are comparing with (the pre-industrial or high-CO2 world without SRM). I think the statement here should be more specific.

This line is now rewritten:

"*One fundamental problem is that compensating GHG-induced warming with SRM would decrease global mean precipitation through the direct radiative effect described above*."

Line 80: add "for" to: ". . . continue to change for decades. . ."

Line 89: "separate" to "separates", "totally" to "total"

Line 94: "a" to "the"

Line 134: delete "the"

Line 182: what do you mean of "business as usual scenarios" here?

"business as usual scenarios" is changed to "high emission scenario (RCP 8.5)"

Line 196: add "into" to "taken into account"

Line 217: add "as"

Line 224: "were" to "was"

Line 232: Please define "Preind" before using it

Rewritten as:

"First, we calculated the clear-sky shortwave flux and temperature anomaly compared to the stable preindustrial conditions (Preind simulation) for each year individually, and performed a linear regression between the two variables"

Line 293: "warned" to "warm"

Line 294 and 325: the font is different

Line 315: "Fig.1" should be "Fig.6"

Line 324: "2010-2020" should be "2080-2100"

Line 355: there is an additional "a" in Eq. (1)