We thank Tamas Bodai for thorough review, suggestions and comments. Our point by point answers to the comments are presented below. Referee comments are in bold and our replies in body text.

The authors model two geoengineering methods, the Solar Radiation Management (SRM) and Carbon Dioxide Removal (CDR), in two Earth system models, the CESM and the MPI-ESM, and run simulations under several scenarios and analyse the global mean and regional temperature and precipitation responses. The considered scenarios are the following: (1) RCP4.5 (2) RCP4.5 with SRM controlled to approximately maintain present-day global mean temperature throughout the 21st c. (3) RCP4.5 with SRM controlled to approximately maintain present-day global mean precipitation throughout the 21st c. (4) RCP4.5 with an ambitious CDR removing 1% of CO2 concentration per year They give some evidence in both ESMs that the global mean precipitation response has a global mean temperature-dependent component, which is based on the methodology proposed by Gregory et al. (2004). Besides this the precipitation can respond very fast, at least to the greenhouse co2 forcing, and this fast response is of opposite sign compared with the temperature-dependent part. To aerosol forcing the fast response is negligible. This is the reason why there is less precipitation in an SRM scenario when global warming is fully compensated. Conversely, the authors make the point and demonstrate in the models that maintaining global precipitation levels (3) requires less sulfate aerosol injection than maintaining temperature (2). The authors also point out that in the long run, without geoengineering, the temperature-dependent part of the precipitation response will dominate, and so the worst impacts of climate change we would be just yet to see. In the CDR scenario of net co2 reduction (4) they find a wettening in both ESMs and an overall very similar precipitation response, however, this turns out to be a coincidence, because the temperature response is not so similar, which drives the precipitation response, and these are the fast response components to other forcing agents that seem to "compensate for" the difference. The ESMs also differ in other aspects. The temperature-dependence of the precipitation is more sensitive in the MPI-ESM - while the fast response (per unit forcing) are surprisingly similar - which is why stronger SRM is needed in MPI-ESM to maintain the temperature (2). The regional responses, both temperature, and precipitation, are very different between the models; it seems hopeless to predict in any location even the sign of the side-effect!!! Although it would not be completely useless to be able to predict at least bounds on the magnitude of the possible change.

The paper is written fairly decently. Although there are some repetitions, and the mathematical symbols and notation should be consistent. I attach an annotated version of the manuscript with corrections, comments, and questions.

These are now corrected based on the referee's comments in the annotated version.

Overall, i found the paper a very worthwhile read. Even if i had my own experience with the drying under geoengineering and the fast components of the precipitation response, i have not been aware of the link of these two, and i did not know that the slow response can be put down approximately as a temperature-dependent component. The numerical work handling the EMSs was clearly a great effort too. I have just a few perhaps/hopefully minor points to make.

1. Why did you base your four scenarios (1)-(4) on the RCP4.5 emission scenario when it remains within the Paris Agreement as represented in both ESMs, even within 1.5 K warming? I mean why not consider a business as usual scenario when we would much rather need geoengineering?

Actually RCP4.5 does not remain within the Paris Agreement targets (less than 1.5 or 2 K warming compared to the preindustrial era). Figure 6 was misleading in this respect because the plotted temperature anomaly is calculated with respect to 2010-2020 and not with respect to the preindustrial era. We have now included text ("Temperature/precipitation anomalies relative to 2010-2020") to the y-axis labels to make this point more clear for readers. In addition we now rewrite Line 316 as: "*Under RCP45, the global mean temperature increased by 1.30 K and 1.20 K* <u>*over the 2010-2020 average*</u> *in MPI-ESM and CESM, respectively.*"

Likewise, L329-331 now reads: *"Thus, in both ESMs the SRM-PRECI global temperature increase* <u>*(~1.64 K in MPI-ESM and ~1.27 K in CESM compared to the preindustrial average)*</u> *stayed within the 2 C target of the Paris Agreement. For CESM, the SRM-PRECI temperature increase also stayed within the 1.5 C Paris target."*

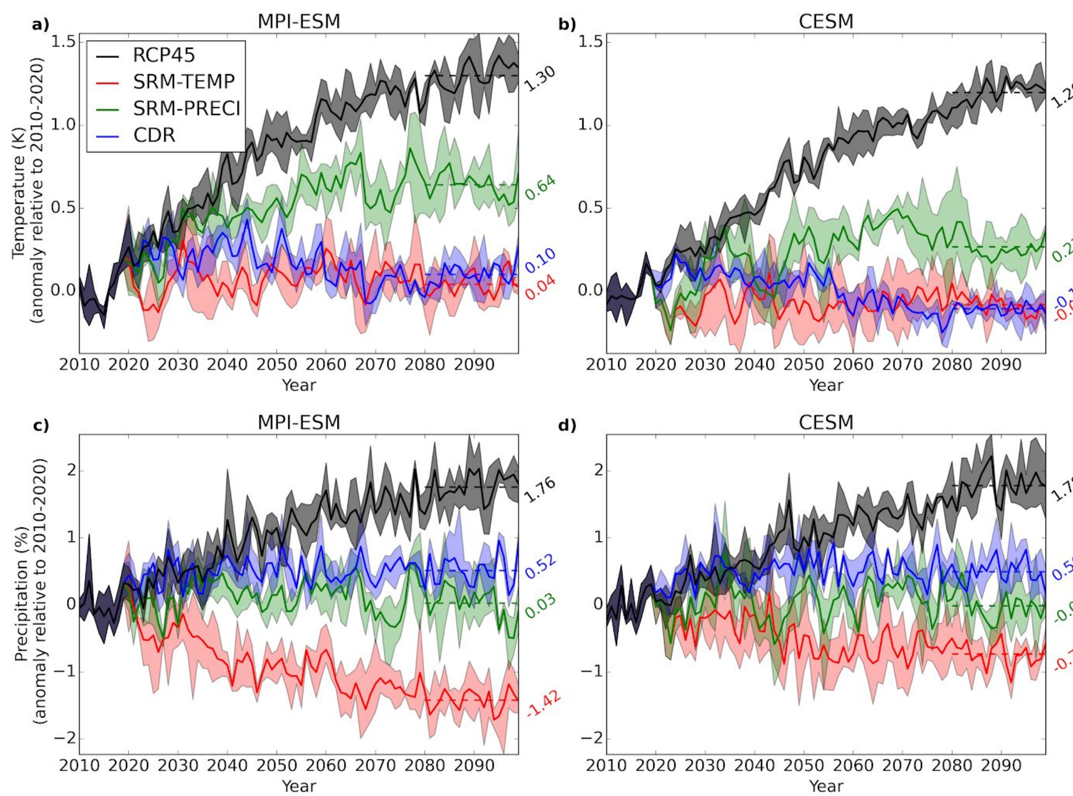

Figure 6: Global mean temperature anomalies in a) MPI-ESM and b) CESM, and global mean precipitation anomalies in c) MPI-ESM and d) CESM. Numbers to the right of each figure indicate the global mean difference between 2080-2100 and 2010-2020. Shaded areas show the maximum and minimum across three ensemble members.

2. Regarding the control method for (2) and (3): You change the forcing level in the ESMs year-to-year, if needed, but is this forcing realistic, would it be consistent with changing the sulfate injection rate year to year? Is there not a transient effect? You wrote that the aerosol model ECHAM-HAMMOZ took 2 years spinup runs.

There is indeed a transient effect which is not taken into account in the simulations. This was mentioned in section 2.2, but is now discussed in more detail as follows:

"*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. 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.*"

3. It seems to me that Fig. 7 does not verify that you have a temperature-dependent precip response component. But rather Fig. 4 does. In this respect, a more clear wording is needed at relevant points in the text.

Based on the referee's comments in the supplement, it appears that we were not sufficiently clear in stating that the temperature-dependent component depends only on temperature and not on the forcing driving the temperature change. This was mentioned on L57 in the introduction and at L309-L310 but is also now articulated more clearly in section 3.3. We have also clarified the rationale for our use of the component-based approach (i.e., in Fig 7). A new figure illustrating the dependence of the fast precipitation response on absorbed radiation has also been included in the supplement. Sect 4.2 it now reads:

"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):

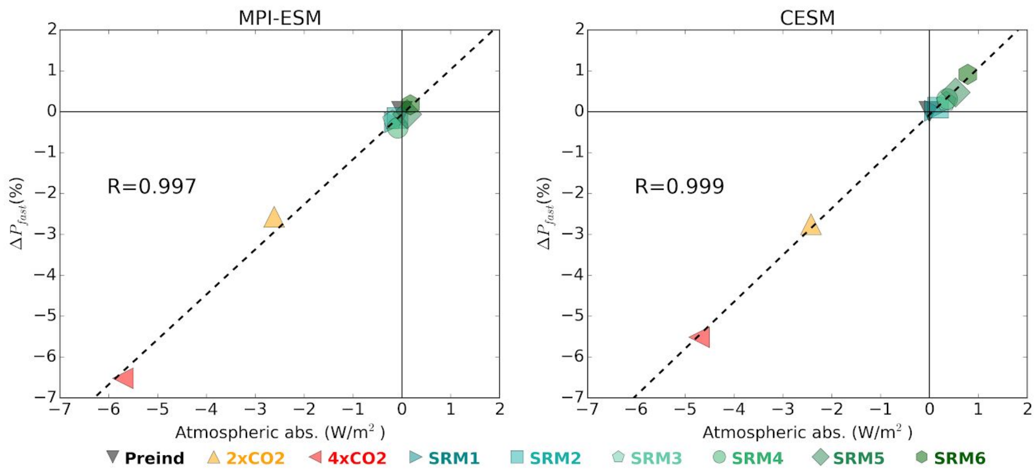

Figure S3. Regression of fast precipitation response versus atmospheric absorption in a) MPI-ESM and b) CESM. R is the Pearson correlation coefficient.

Along similar lines, we feel that inclusion of the BG component in Fig 7 could be misleading in giving the impression that the agreement between total modelled precipitation and the sum of the individual components arises from the BG component. While this is obviously true in the case of RCP 4.5 (from which the BG component is calculated as the residual between equation 2 (previously 1) and modelled precipitation), the same BG component (calculated based on RCP 4.5) is then used as a component in all geoengineering scenarios. Thus in those cases the precipitation agreement referred to above does not arise from the BG component.

To prevent any such confusion, we added a supplemental figure analogous to Fig. 7 but with yearly precipitation differences in the geoengineering scenarios compared to the corresponding year in RCP4.5. Doing the comparison in this way removes the BG component. As this figure shows, the component-based precipitation agrees well with the total modelled quantity in this framework as well.

The corresponding section has been rewritten to make this more clear for the reader:
"Figure 7 shows the precipitation component for each scenario in MPI-ESM and CESM. In general, the precipitation signal as estimated by the fast and slow components via Eq. (2) corresponds well to the actual model quantity in both ESMs for all scenarios. For RCP4.5 this is

an obvious result because the BG is derived as the residual between the modelled precipitation and the sum of the individual components for this very scenario. However, the component-based and full precipitation signals also agree well for the other scenarios, even though the BG component is calculated from the RCP45 case. From year-2020 to year-2100 the mean differences between the Eq. (2) results and the actual model quantities were ranged from -0.01% to 0.04% for MPI-ESM and from -0.16% to 0.05% for CESM. Fig. S5 in supplementary material shows the precipitation responses under the geoengineering scenarios as anomalies relative to the RCP4.5 case. The plotted precipitation differences in Fig. S5 are thus independent of the BG component. We see from this figure that the individual components can be reliably used to understand the drivers of precipitation change for each scenario."

We choose to include the BG component in figure 7 for the following reasons
1) For clarify we wish the modelled precipitation under the individual scenarios to be the same in both figures 6 and 7.
2) The BG component shows that there are several other significant fast precipitation components (in the background) which are affecting precipitation in RCP 4.5 scenario
3) The BG component was clearly different in same scenario modelled by MPI-ESM and CESM. This is one of the main reasons for the diversity in climate models precipitation results found here.
4) We wanted to show and highlight that component-based analyses are not significantly dependent on the background. For example, the precipitation difference between RCP4.5 and CDR can be represented by the fast CO2 component and the temperature-based component for years 2020 and 2100, even though the background atmospheric conditions have changed.