Anonymous Referee #3

In this study, the authors compared model-simulated hydrological cycle change in two scenarios: a scenario in which global mean temperature, equator-to-pole temperature gradient, and interhemispheric temperature gradient, are all stabilized at present day level under the RCP8.5 background scenario through stratospheric sulphate aerosol injection (GLENS ensemble simulations); and a scenario in which atmospheric CO2 is reduced to achieve the temperature stabilization goal of 1.5 degree (carbon capture and storage). The stratospheric sulphate injection simulations are done with CESMWACCM, and the carbon capture and storage simulations are done using CESM1- CAM5. The main metrics used in the analysis are precipitation (P), potential evapotranspiration (PET), and the ratio of P to PET. The regions focused on is North and South America.

Response:
Thanks for the excellent summary.

My biggest concern is to what extend hydrological cycle change in these two scenarios can be compared with each other. By experiment design, compared to the present-day climate, the global mean temperature is near zero in the GELENS, and 1.5 degree warming in carbon capture and storage simulation. Different amount of temperature change would certainly be one of the major factors responsible for different hydrological cycle change.

Response:
Very quick clarification. The 1.5C warming is relative to the pre-industrial era, so the stabilized warming is less than 0.5C compared with present-day in the CCS simulation. Also, our comparison is mainly based on the "avoided warming" relative to RCP8.5 at the end of the 21st century. We did not directly analyze the change between now and the end of the 21st century, because, as the reviewer correctly pointed out, the GLENS experiment is designed to minimize such a change.

The authors also presented changes that are normalized by global mean temperature change, but to what extend these hydrological metrics, in particular PET, scales with global mean temperature at the regional scale?

Response:
Yes. The normalization by global mean temperature change (Section 5) is conducted to mitigate the difference in "avoided warming". The scalability of PET to temperature appears to be strong, even at regional level. In Figure 9 (scatter plot), the correlation is strong with R>0.95.

CESM-WACCM and CESM1-CAM5 also has different model configuration and climate sensitivity, which further complicates the comparison between two sets of simulations.

Response:

This is a good point, also brought up by another reviewer. The different impacts of these two geoengineering schemes are what we set out to quantify. Therefore, we must address the limitation of the current experiment set up – two large ensembles are from two related but different climate models.
As we now increasingly emphasized in this revision, we highlight four approaches to minimize this limitation:
(a) bias correction (see more technical description in this revision),
(b) normalization (Section 5 as the reviewer acknowledged, also heavily revised),
(c) interpretation of physical mechanism by breaking down PET, P/PET changes to individual climatic drivers, to highlight the role of solar dimming at the ground surface, and the shift of tropical rainfall out of deep tropics (Amazon), both of which are only strongly operating in the sulfate injection case (Section 4)
(d) further corroboration of the physical mechanisms at play, using other previously published simulations including volcanic eruption and tropospheric aerosols (Section 5).

In the conclusion, the author states that 'As a result, we emphasize that the main purpose of this paper is not to examine the effectiveness of these two climate engineering schemes in the sense of absolute values. Instead, we aim to highlight the physical mechanisms at play, especially when distinct between the two approaches'. But most of the study is actually devoted to the comparison of these two scenarios quantitatively, and I really don't see a clear presentation of the fundamental physical mechanisms gained from this study.

Response:
Most of the quantitative comparison is done thru relative values (fractional changes avoided by these two types of geoengineering).
We have tried to improve the presentation of the physical mechanisms in this revision (related to bullet points (c) and (d) in the response above), including the revised Table 5, Figure 4, Figure 7, Figure 8 and Figure 9. Further suggestions are certainly welcome.

Specific comments:

Abstract: 'these two leading geoengineering schemes have not been carefully examined under a consistent numerical modelling framework.' Does it imply that this study is the first to carefully examine these two schemes in a consistent modelling framework? This is clearly not true. 'Here we present a comprehensive analysis of climate impacts . . .' This is not true. This study only analyzes some hydrological metrics for some specific regions. This is not a comprehensive analysis.

Response:
Some previous work has studied the difference between sulfate injection and carbon dioxide increase/decrease. While the carbon capture impact should be similar to deep emission cut, the current design puts that to extreme by introducing a large amount of negative emission towards the later half of the 21st century, much higher than assumed in RCP2.6 (Figure 4e, solid green vs. dash green) .
We have tuned down the language to be
"...these two leading geoengineering schemes have not been directly compared under a consistent analytical framework using global climate models."
"Here we present the explicit analysis of hydroclimate impacts of these two geoengineering approaches at global and regional level"

Introduction:

Line 64: The reference of Xu and Ramanathan, 2017 and Miller et al., 2017 is missing in the reference list. (please also check other references. Quite a few are missing in the reference list)

Response:
Thanks, and we apologize for those mistakes and will check the reference list thoroughly.

Line 64-65: Climate engineering is proposed as a potential method to mitigate global warming, but it is a too strong statement saying that climate engineering is needed in climate mitigation. In fact, in the abstract of Lawrence et al. (2018), as cited here, it states: "Based on present knowledge, climate geoengineering techniques cannot be relied on to significantly contribute to meeting the Paris Agreement temperature goals"

Response:
Yes. We have now clarified what we meant:
"aggressive climate engineering schemes are required <u>in order to meet these low-warming targets"</u>.
This is in line with Laweren et al, which basically said climate geoengineering is not sufficient in itself.

Line 65: Please provide reference for these approaches. In particular, what is 'spraying sea water over sea ice' and 'oceanic evaporation enhancement' approaches?

Response:
Yes, these are two less commonly discussed proposals. Both are highly controversial and are included for completeness.
Field, L., Ivanova, D., Bhattacharyya, S., Mlaker, V., Sholtz, A., Decca, R., et al. (2018). Increasing Arctic sea ice albedo using localized reversible geoengineering. Earth's Future, 6, 882–901. https:// doi.org/10.1029/2018EF000820
Salter, Stephen. "Spray turbines to increase rain by enhanced evaporation from the sea." Tenth Congress of International Maritime Association of the Mediterranean, Crete. 2002

Line 67-70: It is confusing to state that they are global-scale schemes. In theory, each of the schemes described here can be implemented at either global or local scales.

Response:
Right. In terms of impact, these two approaches, even when implemented at local scale, are less confined to regional levels, such as cloud brightening or land albedo modification. We change it to "two schemes that can have global impact …"

Line 70: This sentence needs some rewriting. Stratospheric sulphate injection is usually considered to be relatively inexpensive.

Response:
We change it to "Both approaches, especially the first one (carbon capture), can be ~~massively~~ expensive... "

Method: In addition to fundamental difference between GLENS and CO2 mitigation, these two sets of simulations use different versions of CESM, which adds another uncertainty to the results presented here.

Response:
The difference of the two mitigation is what this analysis is aiming for, so we do not see that as a weakness. But in this revision, we have fully acknowledged the two similar but different versions of GCMs is a limitation of this pilot work. We have tried to mitigate this caveat (see responses previously to the general comments) and also call for future analysis using more models.

By just reading the paragraph of the carbon capture and storage experiment, it's not clear to me whether this is emission-driven or concentration driven. It says net emission is reached at year 2050, and then says the corresponding CO2 concentration is prescribed rather than simulated.

Response:
Sorry for the confusion. The CESM itself is concentration driven. The concentration is simulated using a simpler climate model (MiCES), prior to CESM simulation, which is emission driven. This is now clarified.

Also, a figure showing the emission (or concentration) pathway for the carbon capture and storage experiment should be presented.

Response:
We had shown the emission pathway (emission reduced) in Figure 4e.
We also add a comparison with RCP2.6 (which is well studied and compared with solar geoengineering) to show that the CCS scenario here is much more aggressive and contains a significant amount of carbon capture.

2.4 Hydroclimate variables examined: The authors state: "we focus on climate quantities over land due to their close relevance to agriculture, ecosystems, and the carbon cycle ..". Why not also analyse some variables directly related to agriculture and carbon cycle, such as terrestrial gross and net primary production? They are available from CLM output.

Response:
Good point, but we have tried to limit the metric in discussion to P/PET by comparing it with a few other drought indicators. This is to avoid too much complexity in the presentation.
We agree that CLM land output should be further analyzed to make a stronger link with the agriculture and carbon cycle. We considered it carefully but decided not to pursue that route at this stage, considering since the CLM version is also slightly different between the two models which will bring further complexity.

Page 6, line 71: "Climate model output cannot be taken at its face value". This statement is not true. It depends on purpose. For climate modelling studies that aim to understand fundamental mechanisms, no bias correction is needed at all.

Response:
True. We adjust it to be "Climate model output cannot be taken at its face value, especially for future projection."
Another benefit of bias correction is that it mitigates the model differences.

3. Mitigation at the global scale
Most of this section is devoted to the presentation of numerical values and the characteristics for temporal evolutions of temperature, precipitation, and potential evapotranspiration. This kind of discussion should be shortened and replaced by scientific discussions of the underlying mechanisms.

Response:
We will shorten Section 3 as suggested.

It is not surprising that temperature response to SAI is quicker than that to carbon capture because of the long timescale associated with CO_2 forcing, but I think more analyse should be done for the change in hydrological cycle.

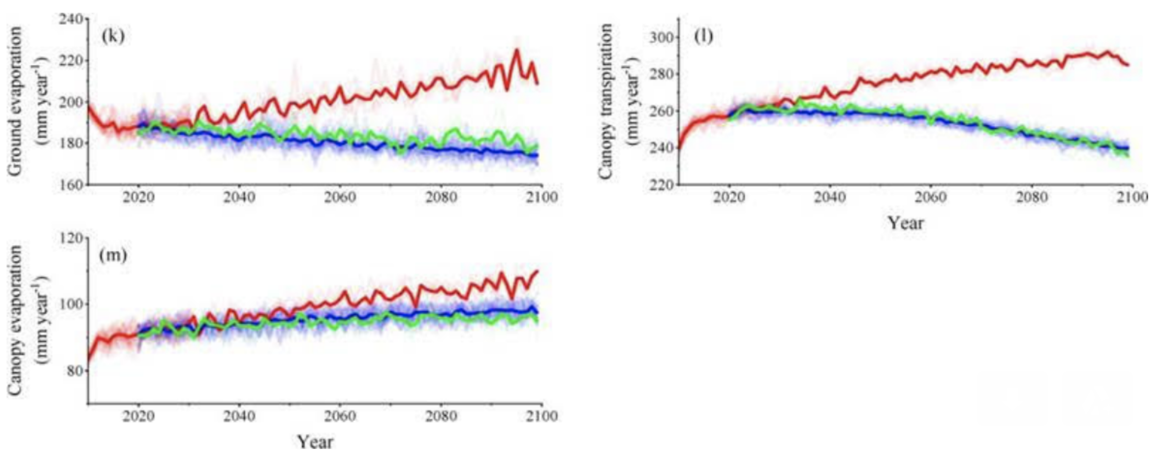
Response:
Thanks for recognizing a key message of Section 3. The long time scale of CO_2 forcing is now presented in an improved version of Fig 4 (bottom row).

How does the change in PET compare the change in soil moisture as presented by Cheng et al. (2019)?

Response:
In Figures 1 and 2, we had compared P/PET change with soil moisture as studied in detail by Cheng et al., (2019). They are generally consistent but with notable regional differences.

For carbon capture simulation, what is the role of the direct CO2 effect on land through the influence on stomatal opening, leaf area index, and vegetation dynamics (if any)?

Response:
The Ball-Berry stomatal conductance model was utilized in both CLM4.0 and CLM4.5. though there is an improvement of the iterative solution in the stomatal conductance model under CLM4.5 relative to CLM4. The stomatal conductance is directly influenced by the relative humidity, the CO_2 concentration at the leaf surface, and the soil water.
The competition of all these terms are all systemically studied in Cheng et al., (2019), which found that under a warming climate, increases in all column soil liquid water and canopy intercepted water overwhelm the direct physiological effect of CO_2 (which tends to suppress the transpiration), and thus lead to an overall increase of canopy transpiration under RCP8.5 (Red line in the next figure).



Anyway, just to present numbers does not help much to improve our scientific understanding.

Response:
Again, we very much appreciate the suggestions of digging into land component model output and carbon cycle response. But that's really beyond the scope of this analysis which focuses on hydroclimate metrics. We do have a project in planning to look at land response more closely in agriculture regions with a focus on crop yields under various mitigation scenarios.
As for the scientific mechanisms, we now enhance the discussion of P/PET changes due to five governing drivers in Section 5 (Figure 10) and also contrasting that to previous model analogs of tropospheric GHGs, aerosols, and volcanic eruption. To our knowledge, this type of synthesis is not done before.

3.1 temperature The lengthy discussion of global temperature change does not really provide any scientific insight. All it says it that SAI stabilize temperature change and carbon capture maintains 1.5 degree warming by the end of this century, both of which are achieved by experiment design. We can just use a few sentences to cover this info.

Response:
We have shortened the temperature results in Section 3.1 to be two paragraphs.

Page 9 line 43: What is 'carefully introduced'?

Response:
We clarified it to be :
when the amount of Sulfur Injection is carefully adjusted to balance the CO2 warming

Page 9, line 47: "Because of the careful experiment design here'. Does it imply that previous experiments are not carefully designed? Please rephrase.

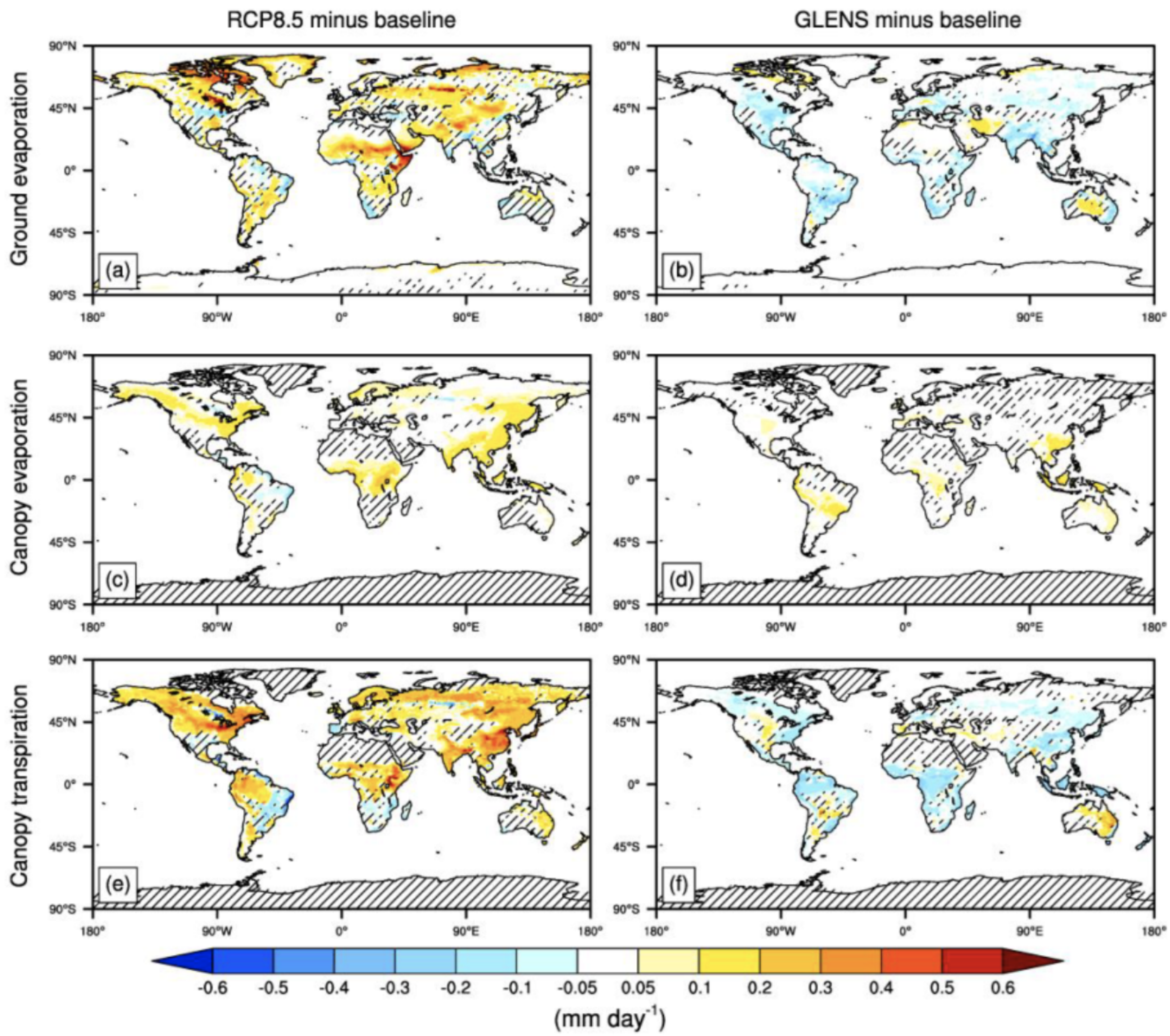
Response:
Change it to "In the current experiment design.."

Page, 10, lines 87-88 "For example, the mid-century projected PET increase is 102.2 mm/year, but the Sulfur Injection can lower that by 127.8 mm/year, which drops the absolute value of PET by 25.6 mm/year" 102.2 mm/year is lowed by 127.8 mm/year? Please check the math and expression here.

Response:
The math is correct and actually a point we wanted to make. Sulfur Injection can flip the sign of PET change from increase to decrease.
Clarified as "the mid-century projected PET increase is 102.2 mm/year, but the Sulfur Injection can reduce that by 127.8 mm/year, which actually leads to a drop of the absolute value of PET by 25.6 mm/year."

Regional change: How does the presented PET change for GLENS compare with soil moisture change presented by Cheng et al. (2019)? This should be discussed.

Response:
PET itself depicts the evaporation demand of the atmosphere and has no direct bearing on soil moisture. P/PET is more related to soil moisture and vegetation growth as shown by many studies previously. Thus, we only showed P/PET compared with soil moisture in Fig 1 and 2.
PET change is also similar to the ET changes as studied in Cheng (2019), not surprisingly because they are based on the same dataset.

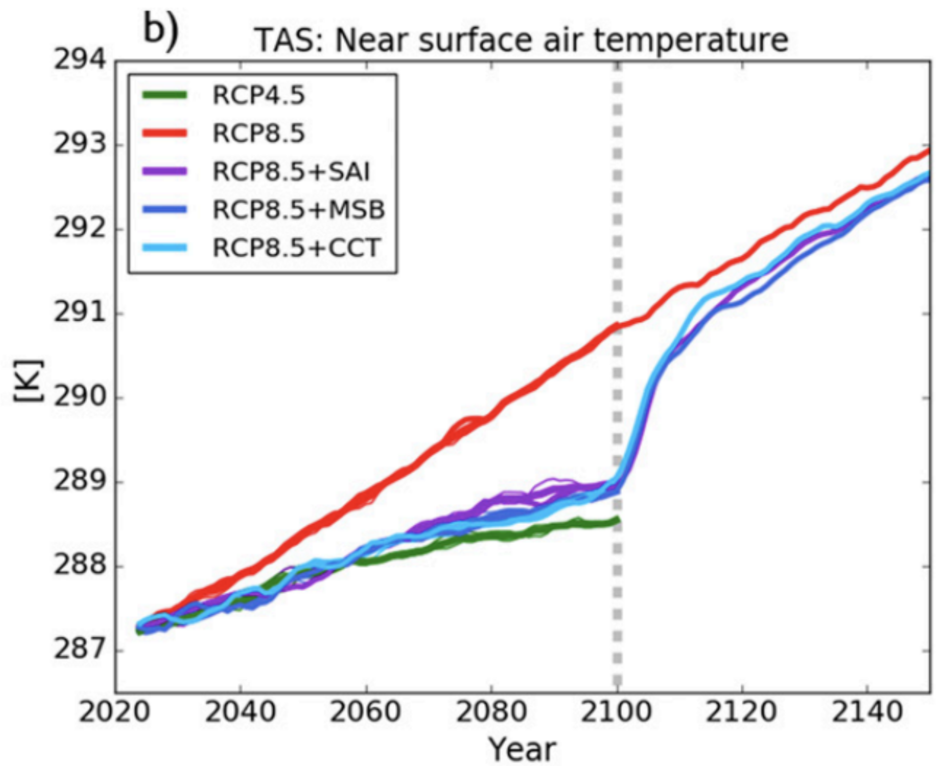


Page 15, lines 50-53: As clearly stated here, there is no direct comparison between GLENS and carbon capture experiment here. First of all, temperature change is not the same, which masks the usefulness of this study.

Response:

True. If the experiment were perfectly designed, the temperature change should be maintained the same for both cases to facilitate a fair comparison. We acknowledge the caveat and call for more work into it.

However, retrospectively, we do not really see the temperature level difference of 0.5-1C as a major limiting factor. Several other comparison studies based on climate models are not producing perfecting aligned temperature trajectory either, for example Muri et al., (2018) with Figure 2b copied below. Also see Figure 3 of Niemeier et al., (2013).



Niemeier, U., Schmidt, H., Alterskjær, K., and Kristjánsson, J. E. (2013), Solar irradiance reduction via climate engineering: Impact of different techniques on the energy balance and the hydrological cycle, J. Geophys. Res. Atmos., 118, 11,905– 11,917

Muri, H., J. Tjiputra, O.H. Otterå, M. Adakudlu, S.K. Lauvset, A. Grini, M. Schulz, U. Niemeier, and J.E. Kristjánsson, 2018: Climate Response to Aerosol Geoengineering: A Multimethod Comparison. J. Climate, 31, 6319–6340, https://doi.org/10.1175/JCLI-D-17-0620.1

5. Normalized change To what extend PET change scales with global mean temperature change at regional scale?

Response:
See the response to a general comment.

Page 16, lines 71-75: There are many studies on the different precipitation sensitivity to CO2 and aerosol forcing. The authors should discuss some of them, in addition to their own study (Lin et al., 2016)

Response:
Right. We included a few more studies on precipitation response to both sulfur injection (Muri et al., 2019; Niemeier et al., 2013; Cao et al., 2017) and to troposphere SO2 forcing (Ming et al., 2007)

Page 16, lines 76-77: I don't understand how this conclusion is drawn. In this paragraph, only CESM model is mentioned.

Response:
The earlier argument is flawed and we have removed the sentence in question.

Page 16, lines 83-84: I just don't understand this sentence.

Response:
Thanks for catching that. The related sentences are revised to be "Different from the precipitation sensitivity, the PET sensitivity to Carbon Capture and Sulfur Injection are similar (3.8 %/ºC vs. 4 %/ºC). A similar PET change, when combined with the greater precipitation decrease, will lead to a smaller increase in P/PET in response to Sulfur Injection. In other words, the almost identical PET sensitivity in response to Sulfur Injection and Carbon Capture is the main reason that Sulfur Injection has a smaller control over P/PET by a factor of two (Figure 9, -1.1 %/ºC vs. -2.2 %/ºC)."

Page 17, lines 9-10: Does the contribution from different climate variables to PET add linearly to their combined effect?

Response:
Yes. Because the composition is done with partial differentiation, they add up linearly to the total PET change.

Page 17, line 18: What does the weaker sensitivity of sulphur injection mean? It should be 'the sensitivity of XX to sulphur injection is weaker'.

Response:
Changed to "weaker sensitivity of P/PET in response to Sulfur Injection".

Page 19, lines 56-60: "Instead, we aim to highlight the physical mechanisms at play, especially when distinct between the two approaches (e.g., radiative balances and dynamic response)." I really don't see what insightful physical mechanisms are highlighted in this study.

Response:
We add a summary sentence here, supported by more revisions in the text. "The notable distinction between the two approaches include response time scales, the role of solar dimming at the surface, the shift of deep tropical rainfall. The direct physiological role of CO_2 is potentially important because the CO_2 level is greatly reduced in only one of the two mitigation approaches. But this study, focusing on meteorological drivers of land aridity using P/PET, did not delve into CO_2's suppression on plant transpiration via stomatal closure, which also appears to be weak in these two models compared with other climate models (Swann et al., 2018)."