Reply to Editor Decision

Comments to the Author:

Dear Wei Li and co-authors,

I have now received the reports of the three referees. One referee suggested accepting the paper, another suggested minor revisions and another suggested major revisions. Reading their comments and suggestions and the text once again, I believe the manuscript needs another round of major revisions before it can be considered for publication in HESS.

Please consider carefully the following points in your revised manuscript and replies to the reviewers:

1. Justify the use of the 99.95th percentile of rainfall in your analysis.
2. Better explain the differences between the 20% forest cover scenario and the 50% forest cover scenario (especially address the comment made by the third reviewer - why would the 20% scenario produce larger changes than the 50% scenario?).
3. Why temperature was used for the comparison? Also, comment on what seems to be a systematic bias in the temperature.
4. What land cover changes happened between 1990 and 2010?
5. Explain the contradiction in LHF values between Fig. 13 and the text.

I summarized the above points based on the reports of the referees. Please, in your replies, address each of the points raised in their reports (not only the points summarized above). Also, consider having a native English speaker proofreading the manuscript. I am looking forward to receiving your revised manuscript.

Sincerely,

Nadav Peleg

Dear Editor:

We would like to thank the editor's and all reviewers' valuable suggestions and comments on the manuscript. These comments have further improved the quality of the current manuscript. All point-by-point responses are presented in our replies and we have carefully revised the manuscript based on these comments.

Sincerely,

Wei Li and co-authors

Reply to Referee comment 1

Emma Daniels (Referee)

Great to see you have improved the paper substantially. My main worry that remains is the thresholds of extreme precipitation that you choose to investigate. The 99.95th percentile should not be used in my opinion because amount of daily data is way too small to trust such a statistic. Instead go with the 90th percentile for example. Something you can probably not implement in the current research but might want to remember for future work is the sub-tiling (mosaic) option for Noah.

Thanks for the comments and suggestions. We have replaced the 99.95th percentile summer rainfall with the 90th percentile summer rainfall, and some of the results are displayed below (Fig. 4, Fig. 9 and Fig. 10). In Fig.4 and Fig.9, the spatial distributions of the changes in 90th percentile summer rainfall are more similar to those in average summer rainfall than those in 99th percentile summer rainfall. In Fig. 10, the area average changes in 90th percentile summer rainfall have same regulations as those in 99th percentile summer rainfall with smaller variation ranges. Overall, replacing the 99.95th percentile summer rainfall with the 90th percentile summer rainfall makes no differences to the conclusions. For the sub-tiling (mosaic) option for Noah, we will take it into consideration in our future researches. Thanks for the suggestion.

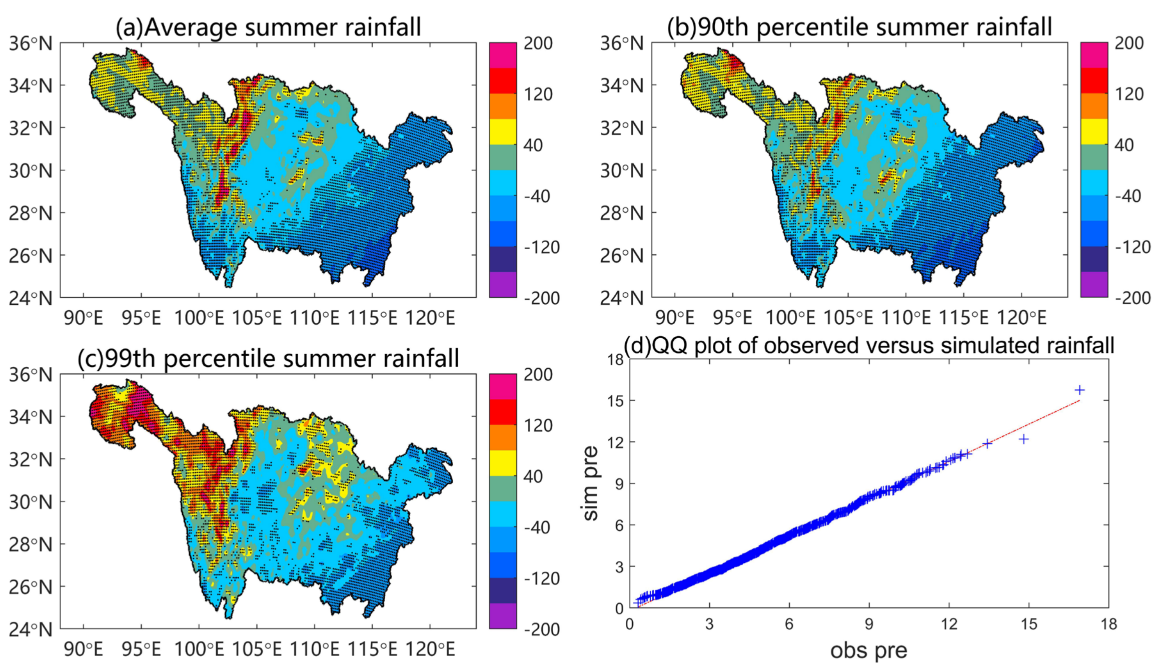

Figure 4. The bias of (a) average summer rainfall (%), (b) 90th percentile summer rainfall (%) and (c) 99th percentile summer rainfall (%) between the 2010 scenario and observed data, and (d) the qq-plot of observed rainfall versus simulated rainfall. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

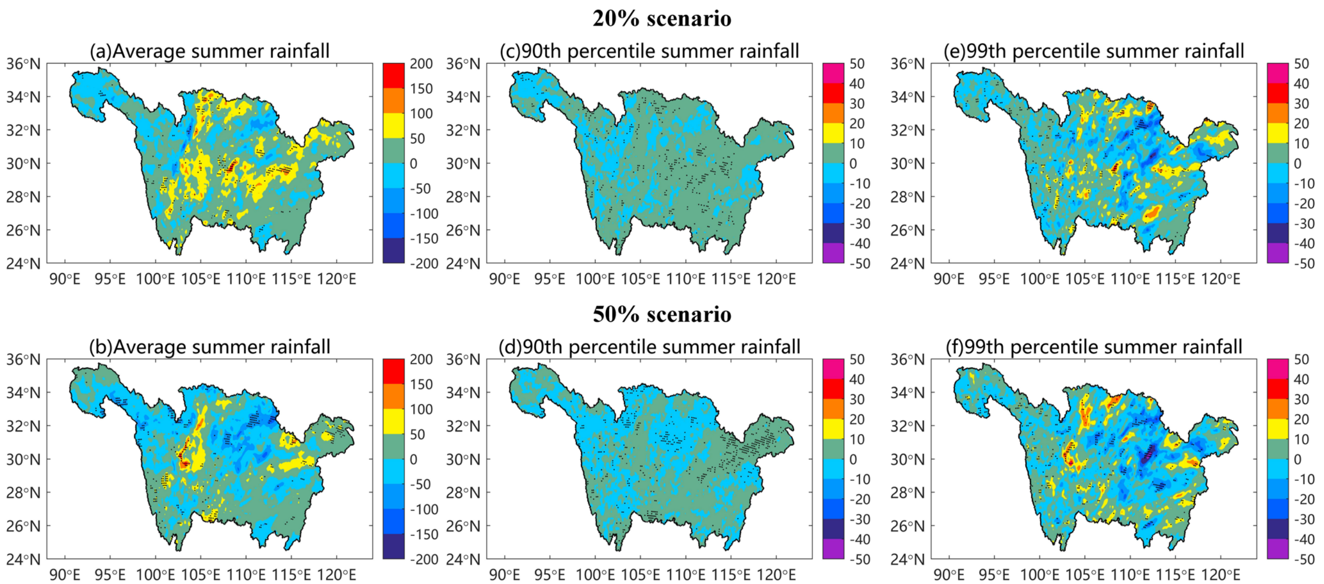

Figure 9. The changes in (a-b) average summer rainfall (mm), (c-d) 90th percentile summer rainfall (mm/day) and (e-f) 99th percentile summer rainfall (mm/day) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

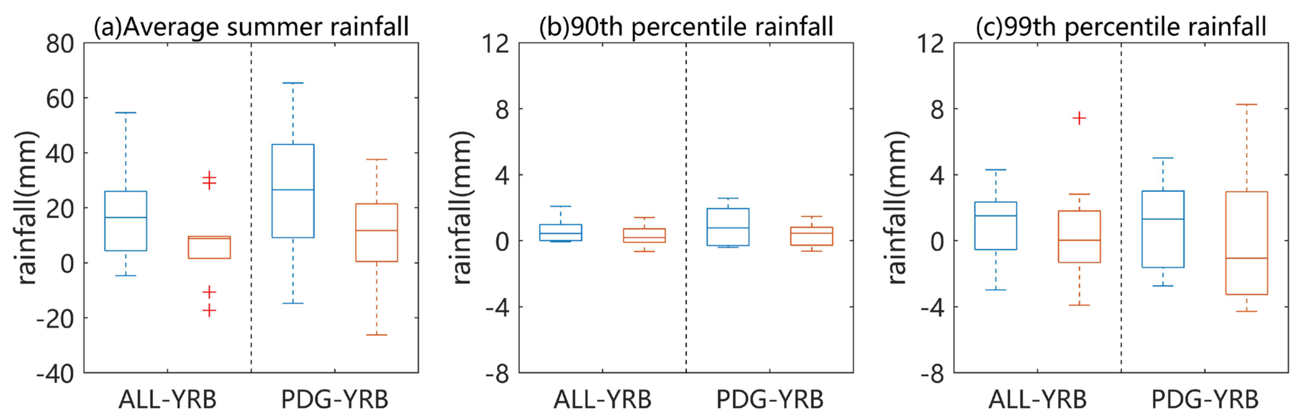

Figure 10. The changes in (a) average summer rainfall (mm), (b) 90th percentile summer rainfall (mm/day) and (c) 99th percentile summer rainfall (mm/day) between the two hypothesis scenarios (20% and 50% scenarios) and 2010 scenario in ALL-YRB and PDG-YRB area. The blue boxes represent the 20% scenario, while the red boxes represent the 50% scenario.

Reply to Referee comment 2

Anonymous Referee #2

The authors addressed most of my previous comments. But after reading the revised manuscript, there are still some issues related to unclear methodology which requires clarifications.

Thanks for the comments. All point-by-point responses are presented as follows and we have carefully revised the manuscript based on these comments. For clarity, all comments are given in the original version, while responses are marked in blue.

I am confused by the simulation length. In L150: they wrote that “The simulated period was 11 years from 2000 to 2010”; in L162 they wrote that “The simulation length was 3 months from June to August”. It is unclear to me is it a continuous simulation from Jan 2000 to Dec 2020, or just each JJA months from 2000 to 2020? For the former case, spin-up using the first year would be enough. For the latter case, it is effectively 11 independent experiments initialized from different years. It means the experiment needs to start earlier than June for spin up purpose (e.g., April or May).

Sorry for the confusion. In this study, we conducted a continuous simulation from Jan 2000 to Dec 2020, with the first year taken as spin-up time. In L162, the simulation length is the length of period we used to determine the most suitable parameterization schemes. As we have explained that the choice of parameterization schemes is based on the simulation of 2005 summer in L159, we have deleted the sentence in L162 “The simulation length was 3 months from June to August” to avoid redundancy and confusion.

L182: “The 99th percentile is the multiyear average value from the 99th percentile rainfall in each year”. Please justify this choice. What if one uses the 99% percentile calculated from the entire study period. Will these two different treatments produce similar results?

The choice that the 99th percentile is the multiyear average value from the 99th percentile rainfall in each year is based on previous studies (Zhai et al., 2003; Pan et al., 2010). We also calculated the 99th percentile from the entire study period and find that these two different treatments produce similar results (Fig. R1).

References:

Zhai, P., and Pan, X.: Change in Extreme Temperature and Precipitation over Northern China During the Second Half of the 20th Century, Acta Geographica Sinica, 58, 1-10, https://doi.org/10.11821/xb20037s001, 2003. (in Chinese)

Pan, A., Fan, S., and Chen, H.: Characteristic of extreme climate change over Jiangsu Province in the last 45a, Scientia Meteor Sinica, 30, 87-92, https://doi.org/10.3969/j.issn.1009-0827.2010.01.014, 2010. (in Chinese)

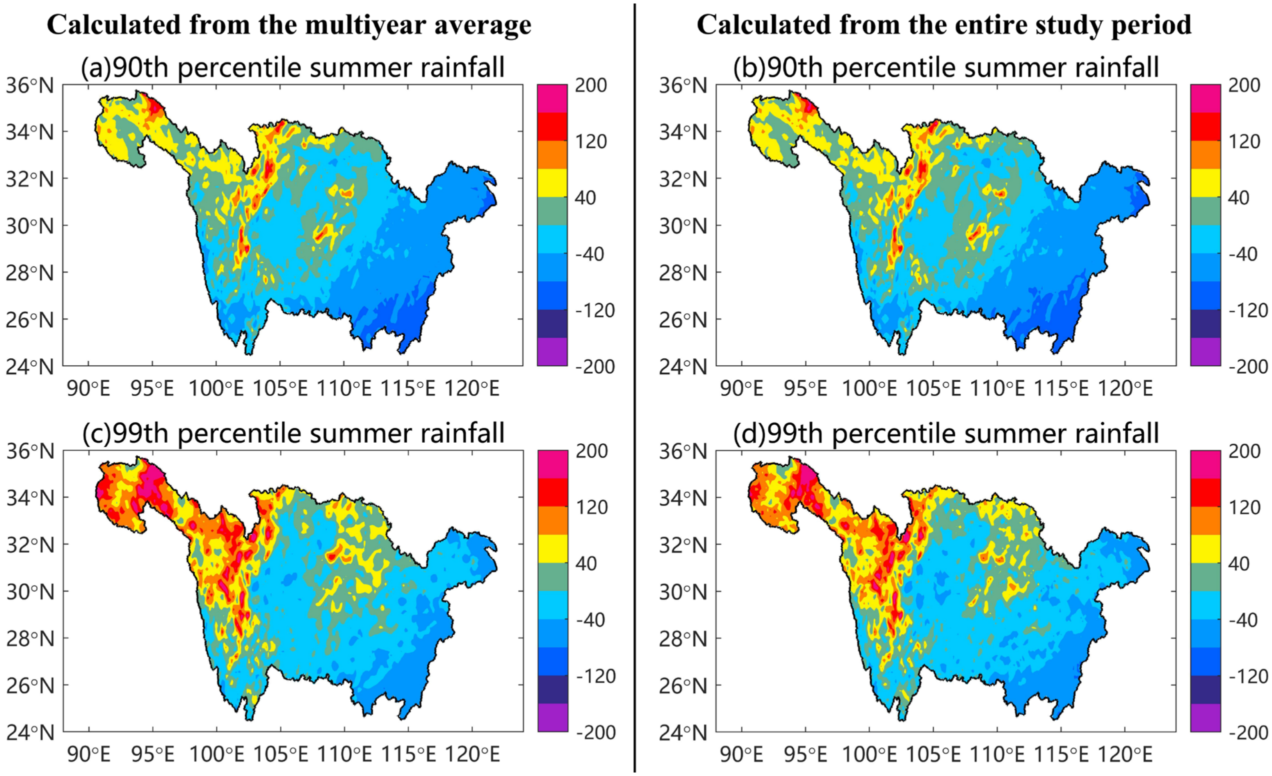

Figure R1. The bias of 90th and 99th percentile summer rainfall (%) between the 2010 scenario and observed data calculated from the multiyear average (Fig. R1a and R1c); the bias of 90th and 99th percentile summer rainfall (%) between the 2010 scenario and observed data calculated from the entire study period (Fig. R1b and R1d).

Figure 4: both the absolute and relative biases are useful. Please provide both and keep one in main text and the other one in SI.

Thanks for the comments. We have added the absolute biases for Fig.4, and put it in the Appendix (Fig. S1).

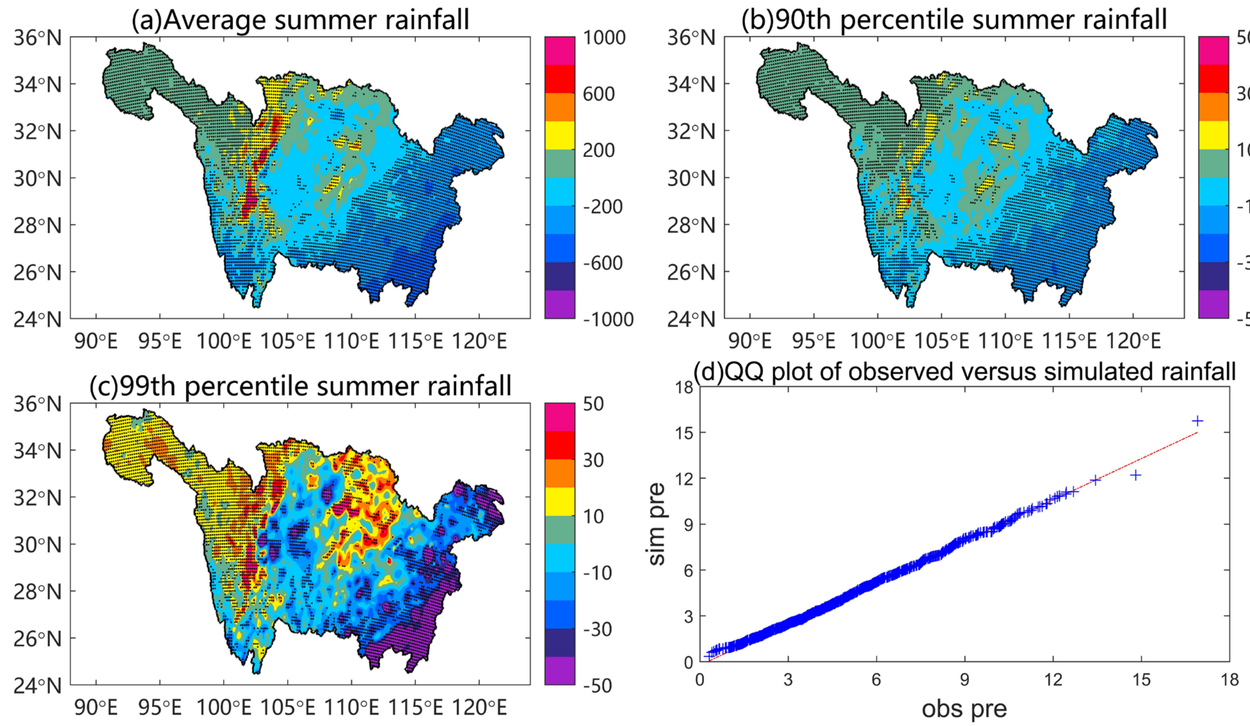

Figure S1. The bias of (a) average summer rainfall (mm), (b) 90th percentile summer rainfall (mm/day) and (c) 99th percentile summer rainfall (mm/day) between the 2010 scenario and observed data, and (d) the qq-plot of observed rainfall versus simulated rainfall. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

L203: what temperature variable from WRF was used to compare with what temperature variable from ERA-5 and what from observation? Is it all surface skin temperature or something else? There seems to be systematic bias in the temperature. The authors need to make sure they used the consistent and correct temperature variable for this comparison, as different temperature variables are not comparable.

The temperature variable used to validate the model performance was the 2m air temperature. We have checked that we used the consistent and correct temperature variable for this comparison. We also notice that the air temperature is systematically biased and similar results have been found in other studies. For example, Zhang et al. (2017) found that there was a cold bias in the eastern China when simulated by the WRF model, and the bias was < 5°C. Yan et al. (2021) also showed that the WRF model produced large cold bias over whole China expect for the northwestern Xinjiang. We have added the explanations in the manuscript.

References:

Zhang, X., Xiong, Z., Zhang, X., Shi, Y., Liu, J., Shao, Q., and Yan, X.: Simulation of the climatic effects of land use/land cover changes in eastern China using multi-model ensembles, Global and Planetary Change, 154, 1-9, https://doi.org/10.1016/j.gloplacha.2017.05.003, 2017.

Yan, Y., Tang, J., Wang, S., Niu, X., and Le, W.: Uncertainty of land surface model and land use data on WRF model simulations over China, Climate Dynamics, https://doi.org/10.1007/s00382-021-05778-w, 2021.

L225-226: I think the authors need also to tell us what land cover changes happened between 1990 and 2010. This helps to understand the cause of simulated precipitation change.

From Table 1, we can see that from 1990 to 2010, the area of cropland decreased from 29.15% to 28.48% for the whole basin, the area of forest increased from 42.82% to 43.60%, the area of grassland decreased slightly from 23.50% to 23.13%, the area of water and wetland increased slightly from 1.65% to 1.79%, the area of urban increased from 0.19% to 0.86%, and the unused land decreased from 2.69% to 2.14%. We have added the details of land cover changes between 1990 and 2010 in the section of Discussions.

L313: For “LHF increases by 2.08×10^3 and 4.82×10^3 W/m^2 for the 20% and 50% scenarios, respectively”, I think there might be a mistake here. According to Fig 13, the largest LHF change is from -20 to 20 W/m^2, how it is possible that this value become two magnitude larger in the multiyear average? Same issue for sensible heat numbers.

Sorry for the confusion. We summed up the changes of LHF and SHF over the whole basin without dividing by the number of model grids in the basin, that was the reason why the changes were so large. After dividing by the number of model grids in the basin, the LHF increases by 0.26 and 0.61 W/m^2 for the 20% and 50% scenarios, respectively. Meanwhile, the SHF decreases by 0.54 W/m^2 and increases by 0.54 W/m^2 for the 20% and 50% scenarios, respectively. We have revised the descriptions in the manuscript.

L326: what is the temperature before this part?

The temperature before this part is the 2m air temperature. To keep consistent, we have replaced the surface skin temperature in this part with 2m air temperature. As the changes in 2m air temperature are almost the same as changes in surface skin temperature, it makes no difference to the conclusions.

Reply to Referee comment 3

Anonymous Referee #3

My comments on the manuscript "Impacts of land use/cover change and reforestation on summer rainfall for the Yangtze River Basin" have largely been handled. There are still English mistakes, but it doesn't affect the readability of text much. The paper needs further English editing to aid readability.

Thanks for the comments. The manuscript has been further proofread, and we have carefully checked the whole paper to improve the readability.

I do believe that the paper would be more impactful if the authors made more of an attempt to understand the differences between the 20% forest cover scenario and the 50% forest cover scenario. Why would the 20% scenario produce larger changes than the 50% scenario? The surface fluxes look similar across the different scenarios, but the surface temperature and relative humidity are quite different (and even opposite in sign). The authors allude to perhaps changes to horizontal wind, but this would be very interesting if they chose to discuss this further. The authors might look to Eiras-Barca et al., 2020 (doi: 10.1111/nyas.14364) for inspiration as this study looks at deforestation and assesses moisture transport and surface roughness changes as a result.

Thanks for the advices. We agree with the reviewer that deeper analyses of the differences between the 20% and 50% reforestation scenarios are necessary. After reading Eiras-Barca et al., 2020 (doi: 10.1111/nyas.14364) recommended by the reviewer and other references, we analysed the spatial changes in the wind at 10m and short-wave radiation to further explore the potential reasons leading to the differences between two reforestation scenarios. Fig. A7 shows that the 10m wind decreases in most places of the Yangtze River Basin for both scenarios, which is as expected because reforestation increases the surface roughness. However, the10m wind increases around the reforested areas, accelerating the moisture export from the forest. It is worth noting that areas with an increase in 10m wind are more expansive for the 50% scenario than for the 20% scenario, which means that more moisture is transported from the forest to other places for the 50% scenario. In addition, from the changes in wind direction in Fig. A7, moisture exported from the forest is transported towards the southern regions and finally flow out the Yangtze River Basin. The above analyses further prove that the differences between the 20% and 50% reforestation scenarios are mainly caused by the changes of horizontal wind. Moreover, we notice that Eiras-Barca et al. (2020) used a water vapor traces tool to track the moisture. This tool can also be used in our future studies to better study the impacts of reforestation on moisture transportation. As for the short-wave radiation (Fig. S3), it has similar spatial changes with the surface skin temperature. The 2m air temperature decreases where the short-wave radiation decreases, further leading to the increases in 2m relative humidity and water vapor mixing ratio. We have added the analyses of 10m wind in the section of Discussions and kept the spatial changes of short-wave radiation in the Appendix.

References:

Eiras-Barca, J., Dominguez, F., Yang, Z., Chug, D., Nieto, R., Gimeno, L., and Miguez-Macho, G.: Changes in South American hydroclimate under projected Amazonian deforestation, Ann N Y Acad Sci, 1472, 104-122, 10.1111/nyas.14364, 2020.

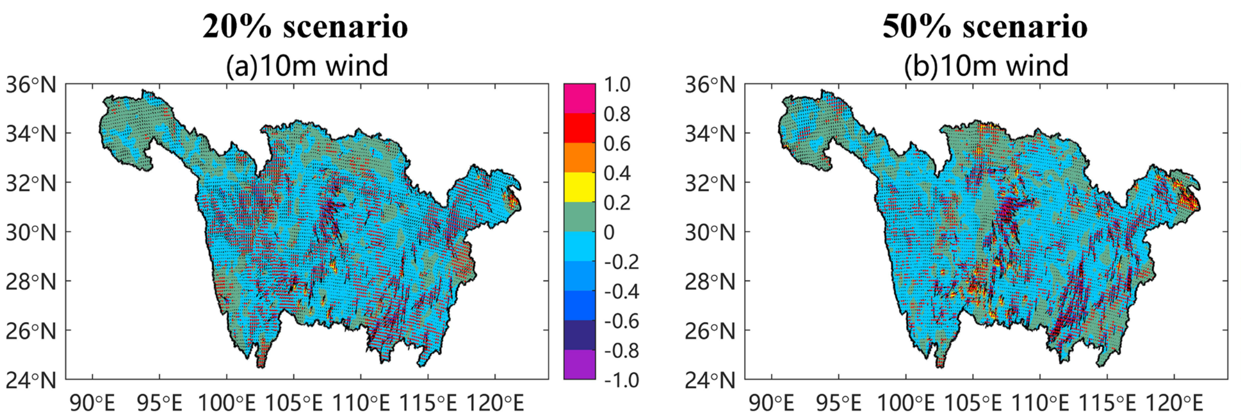

Figure A7. The changes in (a-b) 10m wind (m/s) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

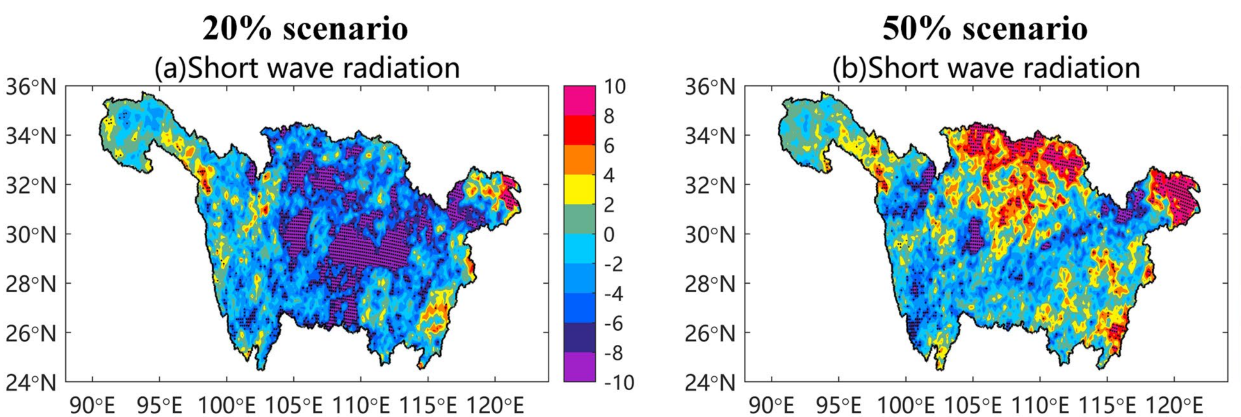

Figure S3. The changes in (a-b) short wave radiation (W/m^2) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.