Reply to Editor Decision

Comments to the Author:

Dear Wei Li and co-author,

Thank you for posting your responses to the three referees' reports. The reviewers raised some important comments and suggestions - especially considering the model setup and validation, the reforestation scenarios and the interpretation of the results. From reading your responses, I can see that you seriously considered their critiques, which I have confidence that will improve the quality of the manuscript. In addition to the comments made by the reviewers, I would like to suggest another analysis that I believe will contribute to the scientific quality of the paper: many recent studies tie between rainfall intensification, air temperature and humidity increase (i.e. the Clausius–Clapeyron relation); in your work, I see the potential of exploring how this relation is changing following the change in vegetation cover. Please consider this point, as it might add another interesting scientific perspective to your work. Based on my reading of the original manuscript and your replies to the referees, I find this to be a potentially interesting paper that might fit the scope of HESS and could be of interest to the hydrological community. Therefore, I invite you to upload a revised manuscript, incorporating the proposed changes and additions, and making any other modifications where you see fit. In your response, please provide a point-to-point answer to the comments made by the reviews, and a track-changed version of the manuscript. I look forward to receiving the revised manuscript.

Sincerely,

Nadav Peleg

Dear Editor:

We would like to appreciate the editor's and all reviewers' valuable suggestions and comments on the manuscript. These comments have not only improved the quality of the current manuscript but also are beneficial to our future research in general. All point-by-point responses are presented in our replies and we have carefully revised the manuscript based on these comments. Moreover, considering the editor's suggestions in the comment, we have done some analysis and here is the reply:

We try to find the relations between rainfall and temperature under different scenarios using the linear regression method, which is recommended in the previous study (Zhou et al., 2016). Then, whether and how the relation between rainfall and temperature changes following the change in vegetation cover is explored by comparing the regression coefficients. First, the average daily rainfall and temperature for the 2010 scenario and two hypothetical reforestation scenarios (20% scenario and 50% scenario) are calculated for each year at the grid-scale. Considering the simulation period is from 2001 to 2010, there are 10 values for rainfall and temperature for each grid, respectively. The relation between rainfall and temperature is then established using the linear regression method for each grid cell. Fig. R1 shows the spatial distributions and boxplots of the regression coefficients in terms of the percentage. From the figures, it can be seen that there are not many differences in the regression coefficients among the three scenarios. Some reasons may explain this result. (1) There are only 10 points for each grid to calculate the regression coefficient, which may bring large uncertainties. It is hard to determine whether the regression equation can well represent the relation between rainfall and temperature as the correlation between rainfall and temperature is insignificant at a 5% significance level in many places. (2) Although the vegetation cover changes, the rainfall and temperature among ten years are not monotonic, which means no apparent trends for rainfall and temperature. That is why the regression coefficients between rainfall and temperature are close to zero for most grids. (3) The simulation biases further enlarge the uncertainties of the relation between rainfall and temperature. From the analyses above, we do not find the change in the relation between rainfall and temperature in terms of the vegetation cover change in this study. We would like not to include this result in the revised manuscript regarding the length of revised manuscript, which is already pretty long (13 figures and 6 figures in appendix), although we think this is a useful testing and checking. Thank you all the same for this comment and suggestion.

References:

Zhou, Y., Luo, M., and Leung, Y.: On the detection of precipitation dependence on temperature, Geophysical Research Letters, 43, 4555-4565, https://doi.org/10.1002/2016gl068811, 2016.

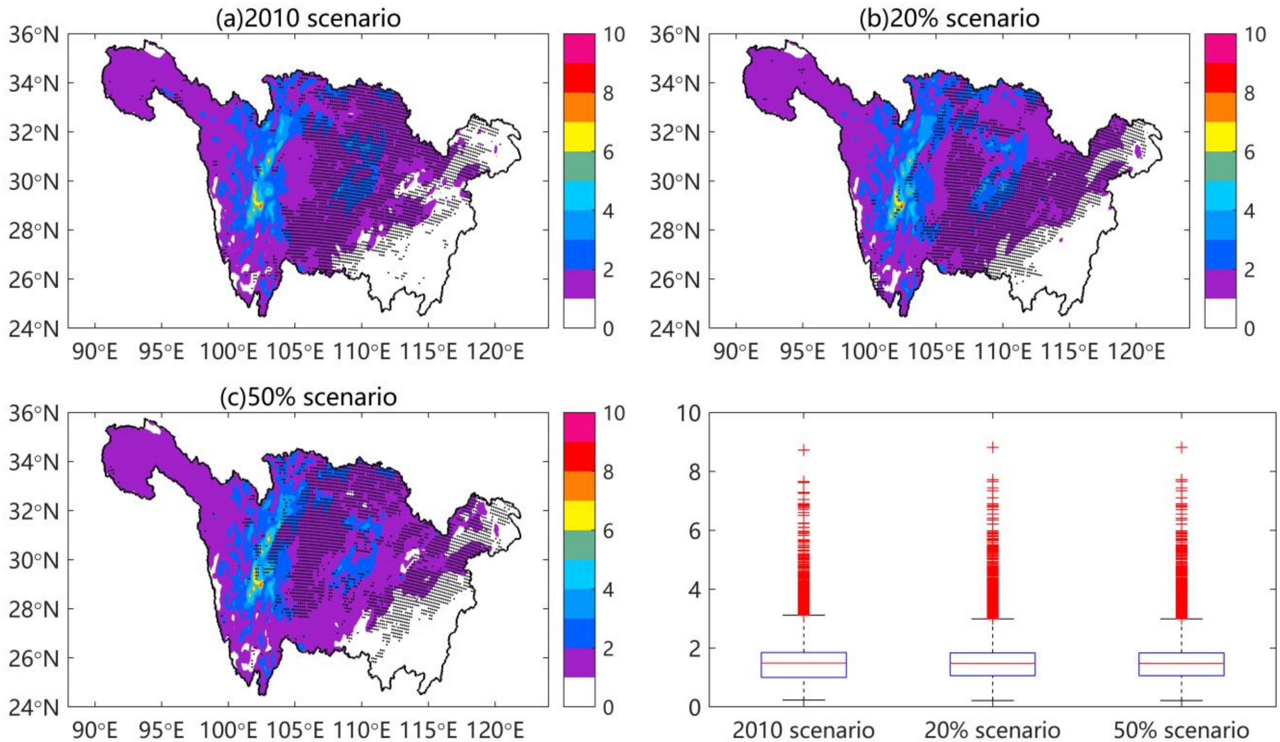

Figure R1. The spatial distributions and boxplots of the regression coefficients (%) for the 2010 scenario and two hypothetical reforestation scenarios. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

Reply to Referee comment 1

Dear Editors and Reviewers:

We would like to thank the editor and all reviewers for their valuable suggestions and comments on the manuscript. These comments have not only improved the quality of the current manuscript, but also are beneficial to our future research in general. 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.

Emma Daniels (Referee)

General comments:

In general, I think the paper has interesting results and could be published. However, the quality of English needs to be improved in some parts (mainly abstract, introduction and methods). Moreover, I miss details in your method such as your definition of summer (i.e. which months are analyzed) and details on the land use maps (e.g. a table with percentages) and how they are included. I think the paper would benefit from analysis of an additional parameter for extreme precipitation, such as rainfall above the 90th percent as with 10 years of data (i.e. 900 data points assuming a summer of 3 months) the 99th percentile alone might be misleading. Furthermore, I miss an explanation why precipitation is thought to increase with further reforestation but decreased between 1990 and 2010 though forest cover increased. Also, I wonder why Shrubland (USGS code 8) and Savanna (USGS code 10) are chosen as a type of forest? Judging from the LANDUSE.TBL these classes are much more similar to Cropland and Pasture than forest, so I wonder if expanding these makes a difference or if you are mainly looking at the effect of the additional Broadleaf forest. I think the figures need work and should become more informative than mainly barplots and spatial difference plots.

Thanks for the comments. We are sorry for the grammar problems in the manuscript. The manuscript has been proofread by a native English speaker. We have added the definition of summer in the introduction, and the summer defined in this manuscript is from June to August. Table 1 has been added in the revised manuscript to explain the percentages of different land use types in the whole basin. Moreover, the land use changes were included in the WRF model by modifying the geographical static data used in the model which further changed the simulation of subprocesses such as the vegetation phenology, canopy stomatal resistance, runoff and groundwater in the land surface model Noah-MP (Li et al. 2018). Many parameters were used in Noah-MP to describe the characteristics of different land use types, such as albedo, HVT (Top of canopy), LAI (Monthly leaf area index), and VCMX25 (Maximum rate of carboxylation at 25 °C). When the land use changed, these parameters changed accordingly which finally led to the changes in substance and energy exchanges between atmosphere and land surface. In the study, we used the U.S. Geological Survey (USGS) land cover with 30s resolution (~ 1km resolution;

“landuse_30s_with_lakes”) in the WRF Preprocessing System (WPS). The new land use data of 1990 and 2010 derived from the Landsat TM digital images at 1km resolution, was then used to replace the USGS land cover data in the WRF simulation in YRB. Finally, we randomly changed 20% and 50% of the croplands to be forests using the 2010 scenario as a baseline to produce 20% and 50% reforestation scenarios.

Given the comments from other reviewers, the 99.95th percent summer rainfall has been chosen to further analyze the extreme rainfall. Furthermore, the land use changes from 1990 to 2010 were not only attributed to the increase of forests, but also the change of other land uses. Therefore, although the forests increased between 1990 and 2010, the precipitation decreased with the joint impacts of all other land use changes.

Moreover, the land use categories of the 1990 and 2010 land use data from Landsat TM digital images were defined by Liu et al. (2002, 2005), which were commonly used in China; while, the USGS data for WRF modelling has 24 land use categories (including lake). Thus, we used the method of land use type conversions based on the study of Hu et al. (2015). According to this method, the four classes of land use in the Liu’s category from Landsat TM digital images, including the Forest (Liu code 21), Shrub (Liu code 22), Sparse woodland (Liu code 23), and Cut over land (Liu code 24), were converted to four classes of USGS land use category, including the Deciduous broadleaf forest (USGS code 11), Shrubland (USGS 45 code 8), Savanna (USGS code 10), and Savanna (USGS code 10), respectively. That was why Shrubland (USGS code 8) and Savanna (USGS code 10) were chosen as a type of forest.

All above information and more clarifications have been added in the method section of the revised manuscript. The figures in the revised manuscript have been improved, and we have also added more informative figures such as qq-plot, boxplots and significance test in the revised manuscript. Please see the revised Fig. 4 and Fig. 10 as follows; other revised figures can be found in the revised manuscript.

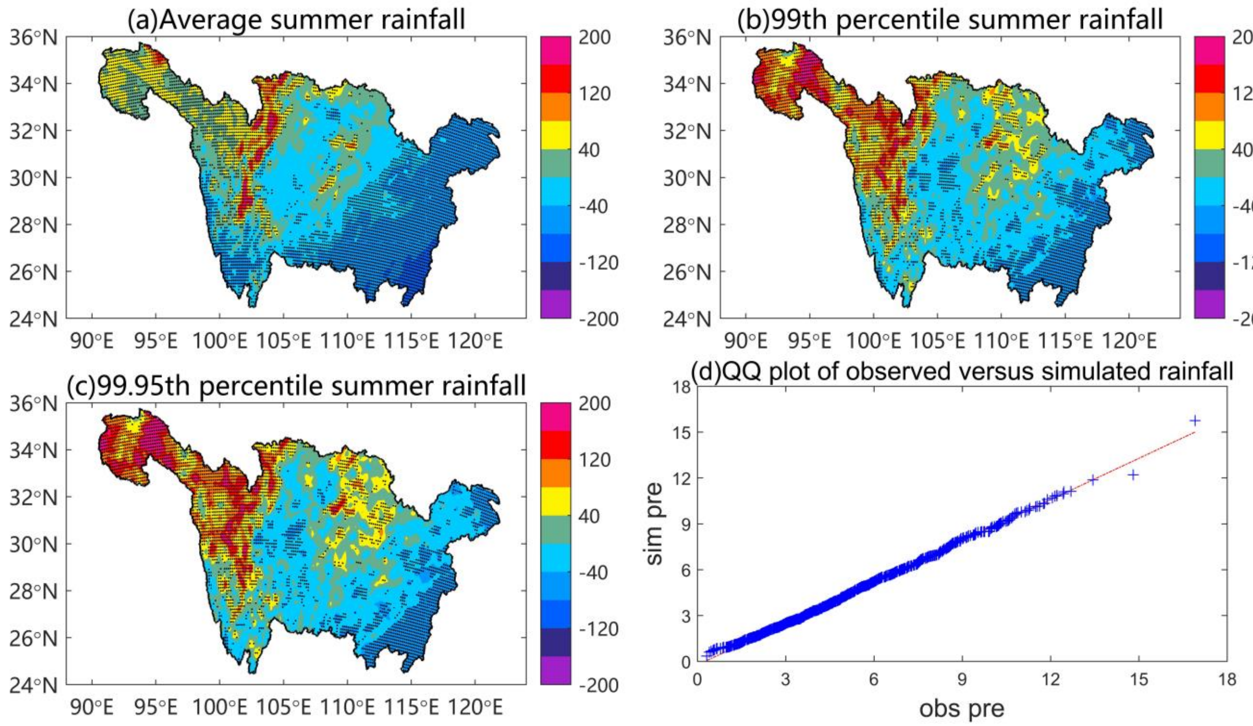

Figure 4. The bias of (a) average summer rainfall (%), (b) 99th percentile summer rainfall (%) and (c) 99.95th percentile summer rainfall (%) between the 2010 scenario and observed data, and (d) the qq-plot of observed rainfall versus simulated rainfall. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

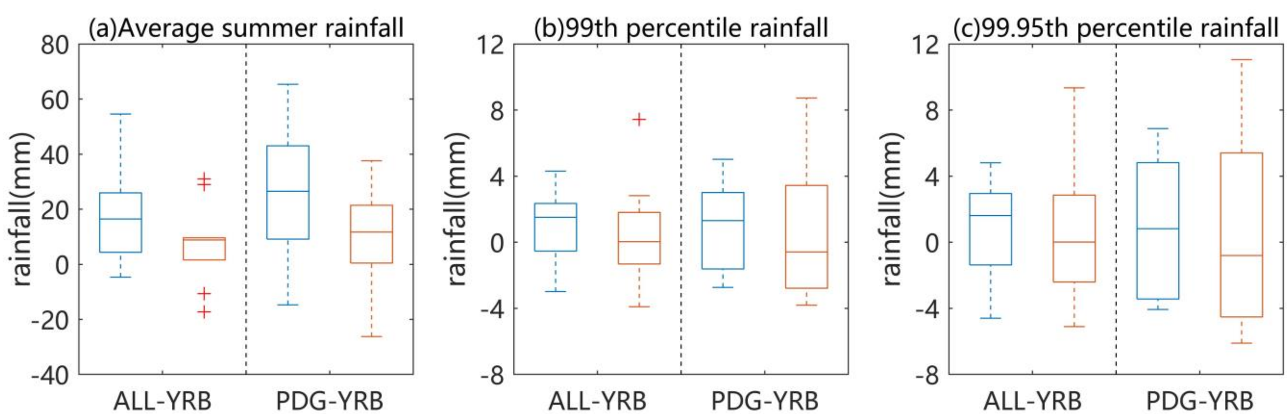

Figure 10. The changes in (a) average summer rainfall (mm), (b) 99th percentile summer rainfall (mm/day) and (c) 99.95th percentile summer rainfall (mm/day) between the two hypothesis scenarios (20% and 50% scenarios) and 2010

scenario in ALL-YRB and PDG-YRB area. The blue boxes represent the 20% scenario, while the red boxes represent the 50% scenario.

Table 1. The percentages of land use and cover under four scenarios.

Scenarios	Cropland	Forest	Grassland	Water and wetland	Urban	Unused land
1990 scenario	29.15	42.82	23.50	1.65	0.19	2.69
2010 scenario	28.48	43.60	23.13	1.79	0.86	2.14
20% scenario	22.80	49.28	23.13	1.79	0.86	2.14
50% scenario	14.58	57.50	23.13	1.79	0.86	2.14

Reference:

Li, J., Chen, F., Zhang, G., Barlage, M., Gan, Y., Xin, Y., and Wang, C.: Impacts of Land Cover and Soil Texture Uncertainty on Land Model Simulations Over the Central Tibetan Plateau, Journal of Advances in Modeling Earth Systems, 10, 2121-2146, https://doi.org/10.1029/2018ms001377, 2018.

Liu, J., Liu, M., Deng, X., Zhuang, D., Zhang, Z., and Luo, D.: The land use and land cover change database and its relative studies in China, Journal of Geographical Sciences, 12, 275-282, https://doi.org/10.1007/BF02837545, 2002.

Liu, J., Liu, M., Tian, H., Zhuang, D., Zhang, Z., Zhang, W., Tang, X., and Deng, X.: Spatial and temporal patterns of China's cropland during 1990–2000: An analysis based on Landsat TM data, Remote Sensing of Environment, 98, 442-456, https://doi.org/10.1016/j.rse.2005.08.012, 2005.

Specific comments:

The number and quality of references in the first section of the Introduction is poor. I am sure there is more work done on LUCC changes that is more relevant to your work than done in Burkina Faso and Scandinavia. You can also leave these out as you mention more relevant ones later on.

Thanks for the comment. We have removed these two references and added two more relevant references in the first section of the introduction: Furthermore, Yu et al. (2020) found that the recent greening in China inferred a country-averaged surface cooling of 0.11 ℃. The study of Lin et al. (2020) showed that the urbanization tended to weak extreme precipitation events in urban agglomerations over coastal regions and intensify the influences on those in central/west China.

References:

Yu, L., Liu, Y., Liu, T., and Yan, F.: Impact of recent vegetation greening on temperature and precipitation over China, Agricultural and Forest Meteorology, 295, 10.1016/j.agrformet.2020.108197, 2020.

Lin, L., Gao, T., Luo, M., Ge, E., Yang, Y., Liu, Z., Zhao, Y., and Ning, G.: Contribution of urbanization to the changes in extreme climate events in urban agglomerations across China, Sci Total Environ, 744, 140264, https://doi.org/10.1016/j.scitotenv.2020.140264, 2020.

Adding a table to figure 4 with the percentages of LU classes would be more informative.

We agree with this comment and have added the Table 1 with the percentages of land use classes. Please see it in the reply to the general comment above.

How are the 32 vertical levels of the model spread? Are there enough layers near the bottom to trust the surface values you are evaluating such as skin surface temperature and 2-m relative humidity?

There were 32 eta levels of the model, and the top was at 50 hpa. We acknowledged that we did not test whether there were enough layers near the bottom to trust the surface values. However, there were many relevant studies which used similar or less vertical levels to study the changes of these surface variables (e.g., Hu et al., 2015; Yu et al., 2020). Moreover, Gallus et al. (2009) found that doubling the number of vertical levels from 31 to 62 did not result in a consistent improvement in the precipitation forecasts and the skill might not be improved much by refining the number of levels, although we acknowledge that the finding from Gallus's study may be different if it is in a different study area. On the other hand, adding the number of levels requires more computing resources and running time, which will limit what we can achieve in the study regarding it, since it is already quite heavy to finish around 40 years WRF simulations with such a big nested domain. So, we decided to keep 32 vertical levels in this study but may look at it in the future work. At the meantime, we have clarified this point in the Discussion part of the revised manuscript.

References:

Hu, Y., Zhang, X.-Z., Mao, R., Gong, D.-Y., Liu, H.-b., and Yang, J.: Modeled responses of summer climate to realistic land use/cover changes from the 1980s to the 2000s over eastern China, Journal of Geophysical Research: Atmospheres, 120, 167-179, https://doi.org/10.1002/2014jd022288, 2015.

Yu, L., Liu, Y., Liu, T., and Yan, F.: Impact of recent vegetation greening on temperature and precipitation over China, Agricultural and Forest Meteorology, 295, 10.1016/j.agrformet.2020.108197, 2020.

Gallus, W. A., Aligo, E. A., and Segal, M.: On the Impact of WRF Model Vertical Grid Resolution on Midwest Summer Rainfall Forecasts, Weather and Forecasting, 24, 575-594, 10.1175/2008waf2007101.1, 2009.

In Figure 5c (and others), why not show a qq-plot of model and observed rainfall instead? 50th percentile is not interesting to show and analyze.

Thanks for the comment. In the revised manuscript, we have shown a qq-plot of observed and simulated rainfall instead of the 50th percentile rainfall (Fig. 4). We have also added the analysis of 99.95th percentile rainfall to further analyze the changes of extreme rainfall. Please see the revised Fig. 4 in our earlier reply to the general comments.

It seems urbanizations plays a role in the precipitation decrease between 1990 and 2010. Please consider using an urban scheme in WRF.

We acknowledge that the urbanization scheme may play a role in the WRF simulation for investigating the rainfall changes. However, the urban area was only 0.19 % of the total area in 1990 and increased to be 0.86% in 2010. In this case, the impact of urbanization in YRB is ignorable regarding the increased urban area is only around 0.67% of total area between 1990 and 2010. In addition, it is difficult to re-run the simulation with urban scheme for this study, because it is very computing expensive and the time of running long-term simulations for such a big and nested domain is quite long. We have added this in the discussion of the revised manuscript and will take the urban scheme into consideration in future researches.

Why are the two areas (ALL-YRB) and (CTF-YRB) analyzed separately? Is there a rational in being interested in the converted areas specifically? Is analyzing more populated areas separately more interesting perhaps? As that is where the impact will be felt, not in the new forests.

Thanks for the comment. The reason we analyzed the two areas (ALL-YRB) and (CTF-YRB) separately was to investigate whether the land use changes at local scale (CTF-YRB) influenced regional climate over a larger domain, e.g., whole basin (ALL_YRB). We agree that analysing more populated areas separately is more interesting and we have added the relevant results in the Section 4.3.2 of the revised manuscript (Fig. 10, Fig. 11 and Fig. 12). From the results, we find that the effects of reforestation are more pronounced in the populated area than over the whole basin.

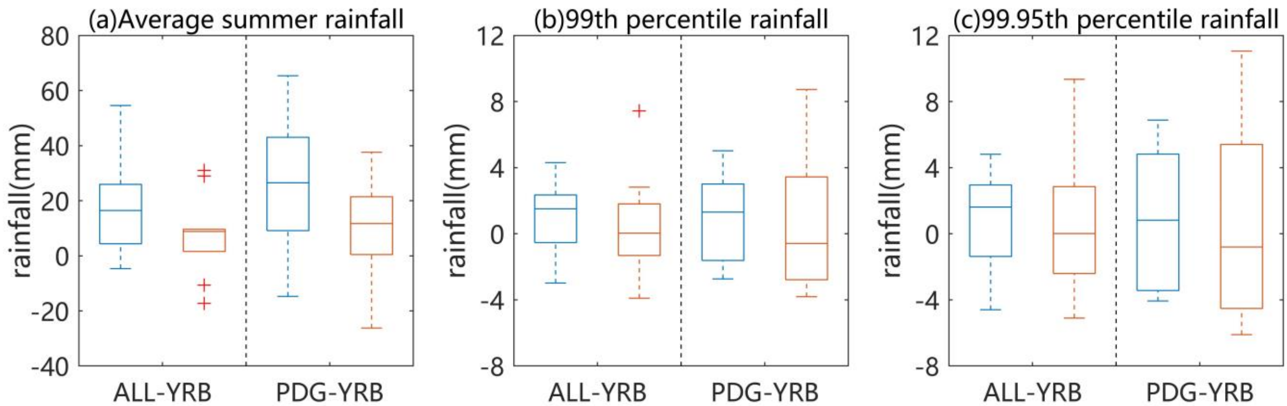

Figure 10. The changes in (a) average summer rainfall (mm), (b) 99th percentile summer rainfall (mm/day) and (c) 99.95th percentile summer rainfall (mm/day) between the two hypothesis scenarios (20% and 50% scenarios) and 2010 scenario in ALL-YRB and PDG-YRB area. The blue boxes represent the 20% scenario, while the red boxes represent the 50% scenario.

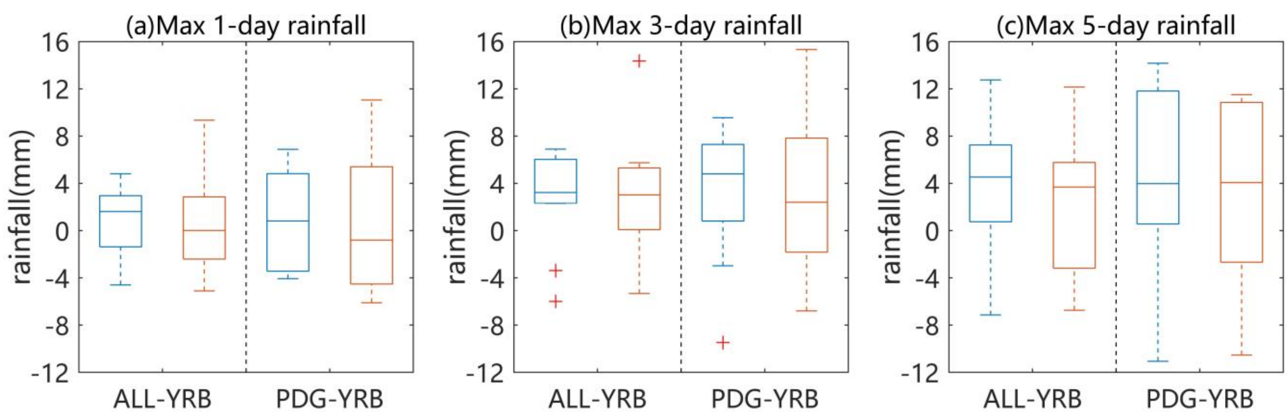

Figure 11. The changes in maximum 1-, 3-, 5-day rainfall between the two hypothesis scenarios (20% and 50% scenarios) and 2010 scenario in ALL-YRB and PDG-YRB area. The blue boxes represent the 20% scenario, while the red boxes represent the 50% scenario.

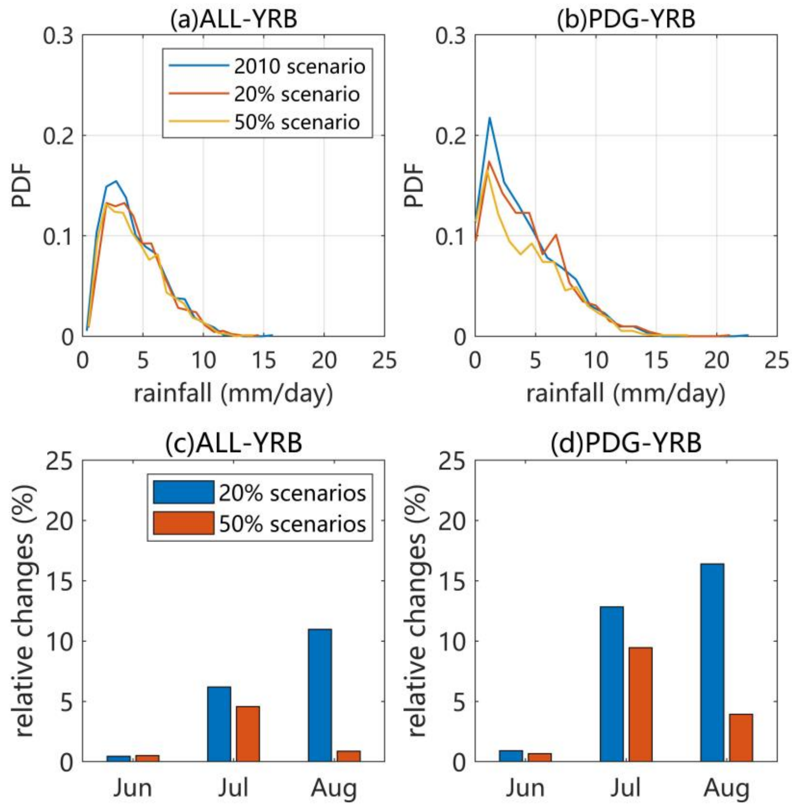

Figure 12. The probability distribution functions of summer rainfall in 2010, 20% and 50% scenarios in (a) ALL-YRB and (b) PDG-YRB; The changes in multiyear-averaged summer monthly rainfall between the two hypothesis scenarios (20% and 50% scenarios) and 2010 scenario in (c) ALL-YRB and (d) PDG-YRB.

Line 275-277 please reconsider/rewrite.

Thanks for the comment. We have added the changes in water vapor mixing ratio in this section (Fig. 14) and rewritten Line 275-277: “From the changes in the surface skin temperature and 2m relative humidity under reforestation, it can be seen that the 2m relative humidity decreases where the surface skin temperature increases. Besides, the water vapor mixing ratio in the atmosphere increases, which finally provides conditions for the increases of summer rainfall amount and extremes.”

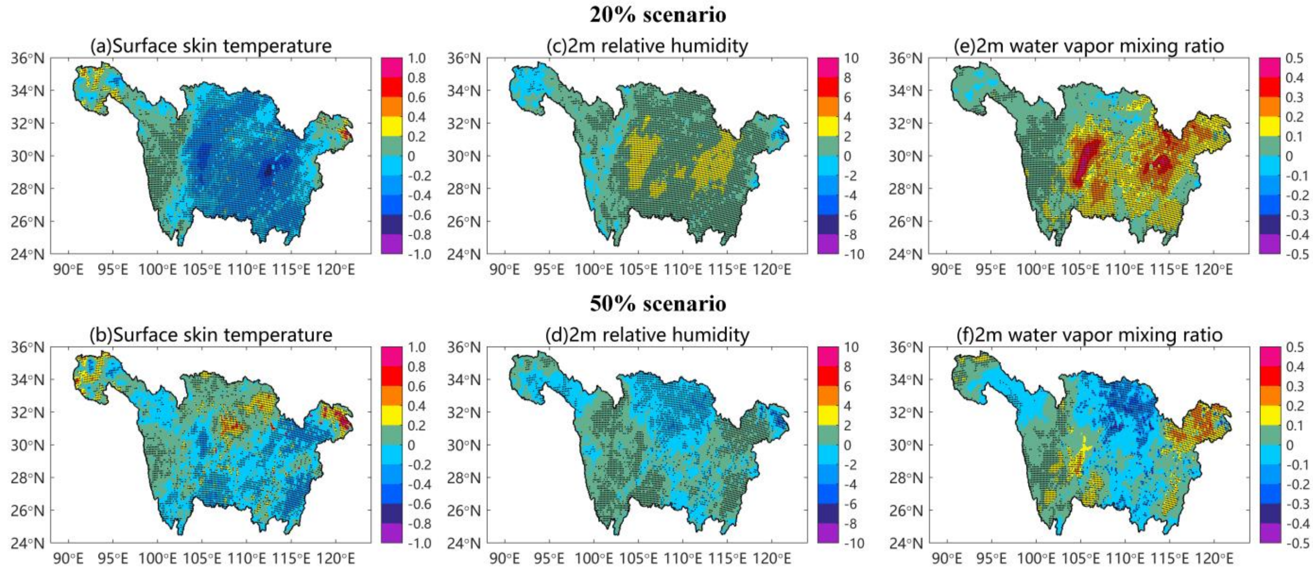

Figure 14. The changes in (a-b) surface skin temperature (°C), (c-d) 2m relative humidity (%) and (e-f) 2m water vapor mixing ratio (g/kg) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

Reply to Referee comment 2

Dear Editors and Reviewers:

We would like to thank the editor and all reviewers for their valuable suggestions and comments on the manuscript. These comments have not only improved the quality of the current manuscript, but also are beneficial to our future research in general. 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.

Anonymous Referee #2

The manuscript by Li et al "Impacts of land use/cover change and reforestation on summer rainfall for the Yangtze River Basin" used the WRF model to investigate how land cover changes and reforestation affect summer rainfall. The research topic is important given the massive ecological projects in China and its climate impact is worth studying. The manuscript is generally well-written, but I also have major comments for the authors.

Thanks for the positive evaluations and comments, all comments and suggestions have been addressed and incorporated into the revised manuscript.

1. For WRF model simulation, how land cover changes were implemented in the model needs more detailed explanations as different land surface models have different representations of land cover. It is still unclear what surface conditions/variables had been modified for the Noah-MP model to correctly reflect the intended land cover changes. I also have questions about the rationality of the randomly chosen crops for the two restoration scenarios.

The land use changes were included in the WRF model by modifying the geographical static data used in the model which further changed the simulation of subprocesses such as the vegetation phenology, canopy stomatal resistance, runoff and groundwater in the land surface model Noah-MP (Li et al. 2018). Many parameters were used in Noah-MP to describe the characteristics of different land use types, such as albedo, HVT (Top of canopy), LAI (Monthly leaf area index), and VCMX25 (Maximum rate of carboxylation at 25 °C). When the land use changed, these parameters changed accordingly which finally led to changes in substance and energy exchanges between atmosphere and land surface. In the study, we used the U.S. Geological Survey (USGS) land cover with 30s resolution (~ 1km resolution; "landuse_30s_with_lakes") in the WRF Preprocessing System (WPS). The new land use data of 1990 and 2010 derived from the Landsat TM digital images at 1km resolution, was then used to replace the USGS land cover data in the WRF simulation in YRB. Then we randomly changed 20% and 50% of the croplands to be forests using the 2010 scenario as a baseline to produce 20% and 50% reforestation scenarios. Both land cover datasets (i.e., downloaded from the WRF website and derived from the digital images) have a resolution of 1km. As the resolutions of outer and inner WRF domain were set as 75km and 15km, respectively, the post-processed land cover data was resampled from 1km to 75km and 15km by the WPS (the WRF Preprocessing System). The percentages of land cover under four scenarios after resampled to 15km are presented in Table A1 below. The dominant land use categories in model grids were used for the Noah-MP model to correctly reflect the intended land cover changes. We acknowledge that the randomly chosen crops for the two restoration scenarios may result in uncertainty in the study, as we have stated in the discussion section, the restoration processes usually happen in specific areas that are related to local policy. However, it is very challenging to gather the related policies from all of the local governments in such a big river basin. It can be also noticed that the crops are mainly located in specific areas such as Sichuan Basin and the middle- and down-stream of the YRB. Although we chose the crop grids randomly in this study, the restoration grids concentrated in these specific areas which was similar to the actual reforestation processes.

Table A1. The percentages of land use and cover under four scenarios after resampling.

Scenarios	Cropland	Forest	Grassland	Water and wetland	Urban	Unused land
1990 scenario	28.67	44.37	24.63	0.58	0.06	1.69
2010 scenario	28.12	45.02	24.60	0.69	0.45	1.12
20% scenario	22.97	49.85	24.83	0.69	0.54	1.12
50% scenario	14.76	57.53	25.32	0.69	0.58	1.12

Reference:

Li, J., Chen, F., Zhang, G., Barlage, M., Gan, Y., Xin, Y., and Wang, C.: Impacts of Land Cover and Soil Texture Uncertainty on Land Model Simulations Over the Central Tibetan Plateau, Journal of Advances in Modeling Earth Systems, 10, 2121-2146, https://doi.org/10.1029/2018ms001377, 2018.

2. When comparing simulation results between different experiments, the authors need to conduct statistical significance tests to determine whether the signal is robust while excluding any noise and random changes which may lead to misinterpretation.

We agree with this comment. We have conducted statistical significance tests to determine whether the signal is robust when comparing simulation results between different experiments. Please see the revised Fig.4 and Fig. 7 as follows; other results of significance tests have been added into the revised manuscript.

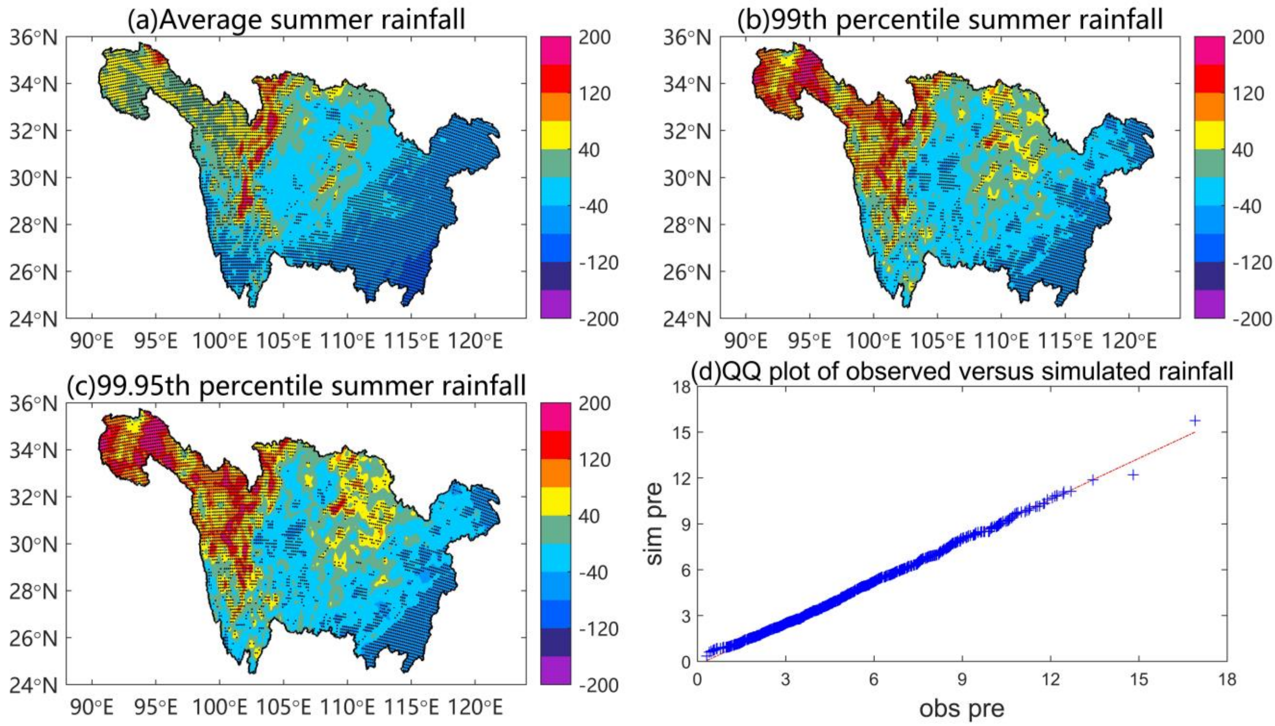

Figure 4. The bias of (a) average summer rainfall (%), (b) 99th percentile summer rainfall (%) and (c) 99.95th percentile summer rainfall (%) between the 2010 scenario and observed data, and (d) the qq-plot of observed rainfall versus simulated rainfall. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

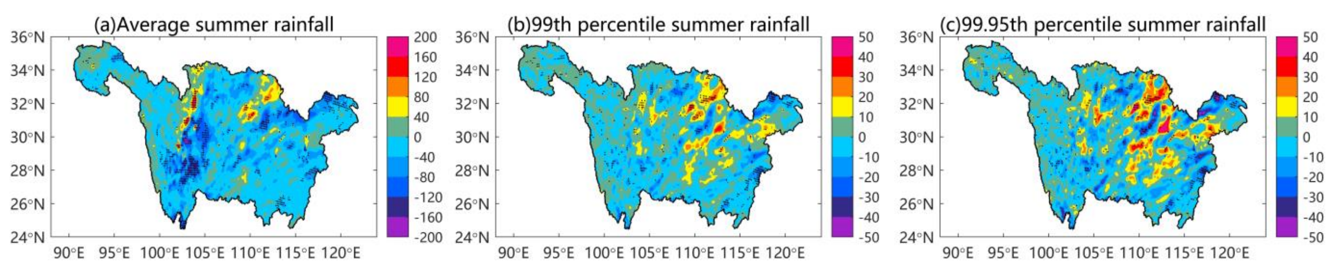

Figure 7. The changes in (a) average summer rainfall (mm), (b) 99th percentile summer rainfall (mm/day) and (c) 99.95th percentile summer rainfall (mm/day) between the 2010 scenario and 1990 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

3. I hope the authors could provide more mechanistic explanations of the results. For example, why did the 20% reforestation result in more precipitation changes than the 50% reforestation scenario?

Thanks for the comment. We have provided more mechanistic explanations of the results in the revised manuscript. As for the 20% reforestation resulted in more precipitation changes than the 50% reforestation scenario, after analysing the changes in the water vapor mixing ratio at 2m (Fig. 14) and upward moisture flux at the surface (Fig. A6), we found that the number of grids showing increased upward moisture flux in the 50% scenario slightly exceeded that in the 20% scenario. In contrast, the 2m water vapor mixing ratio increased over almost all basin in the 20% scenario while showed large decreases in the midstream of the basin in the 50% scenarios. From the surface level to the 2m level, the moisture kept increased in the 20% scenarios while decreased in the 50% scenarios. This suggested that the distribution of moisture may be changed by the horizontal transportation process. Moreover, Yu et al. (2020) found that the vegetation greening reduced rainfall in some region in the southern China which may be caused by the East Asian monsoon, as the East Asian monsoon significantly influenced the summer precipitation patterns in China (Ding et al., 2007). All these information and explanations have been incorporated into the revised manuscript.

Ding, Y., Ren, G., Zhao, Z., Xu, Y., Luo, Y., Li, Q., and Zhang, J.: Detection, causes and projection of climate change over China: An overview of recent progress, Advances in Atmospheric Sciences, 24, 954-971, https://doi.org/10.1007/s00376-007-0954-4, 2007.

Yu, L., Liu, Y., Liu, T., and Yan, F.: Impact of recent vegetation greening on temperature and precipitation over China, Agricultural and Forest Meteorology, 295, https://doi.org/10.1016/j.agrformet.2020.108197, 2020.

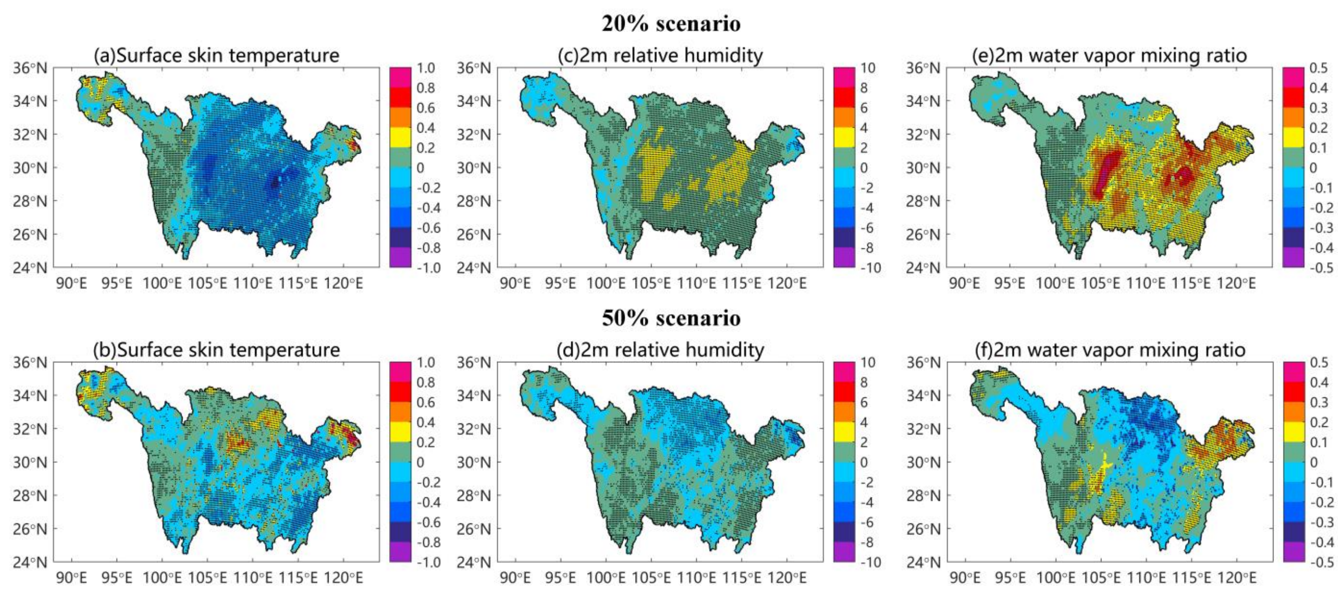

Figure 14. The changes in (a-b) surface skin temperature (℃), (c-d) 2m relative humidity (%) and (e-f) 2m water vapor mixing ratio (g/kg) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

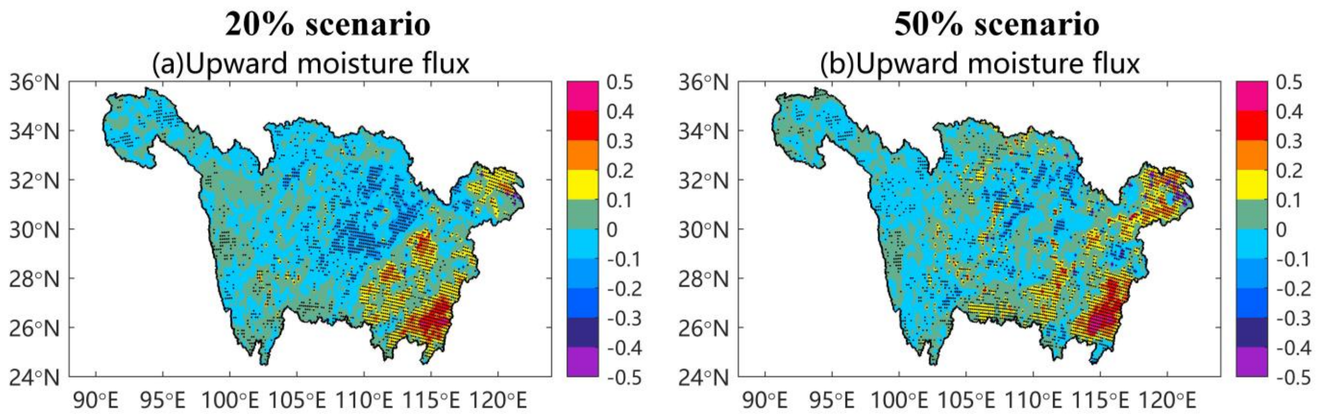

Figure A6. The changes in (a-b) upward moisture flux at the surface (kg/m2) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

Specific comments:

L9: There is another terminology “Grain for Green” frequently used in the literature for “Returning Farmland to Forest Program”. Which one is better acknowledged?

Thanks for the comment. Both the terminologies are correct, and the “Grain for Green” may be more widely used. We have changed all the “Returning Farmland to Forest Program” to “Grain for Green” in the revised manuscript.

L130-140: What kinds of WRF experiments have been conducted to compare different schemes/parameterizations, what domain and simulation length was used for the comparison experiments?

According to previous studies in China (e.g., Hu et al., 2015; Zhang et al., 2019; Feng et al., 2012; Xue et al., 2017), we chose three microphysical schemes (i.e., Purdue Lin Scheme (Lin), WRF Single-moment 5-class Scheme (WSM5), and Eta Scheme (Ferrier)) and two cumulus parameterization (i.e., Kain-Fritsch Scheme (KFN) and Grell–Devenyi Ensemble Scheme (GD)) in the WRF experiments. Five parameterization scheme combinations (i.e., Lin-KFN, WSM5-KFN, Ferrier-KFN, Lin-GD and WSM5-GD) were used to simulate the rainfall and temperature for the Yangtze River basin for 2005 summer, as there were several rainstorm events during this period for this basin. The most suitable parameterization schemes were chosen by comparing the performance of these five combinations. The domain setting was same as the whole experiment which can be seen in Fig. 2. The simulation length of the experiments was 3 months from June to August. We have added these information and explanations in the method and results of the revised manuscript.

References:

Feng, J.-M., Wang, Y.-L., Ma, Z.-G., and Liu, Y.-H.: Simulating the Regional Impacts of Urbanization and Anthropogenic Heat Release on Climate across China, Journal of Climate, 25, 7187-7203, https://doi.org/10.1175/JCLI-D-11-00333.1, 2012.

Hu, Y., Zhang, X.-Z., Mao, R., Gong, D.-Y., Liu, H.-b., and Yang, J.: Modeled responses of summer climate to realistic land use/cover changes from the 1980s to the 2000s over eastern China, Journal of Geophysical Research: Atmospheres, 120, 167-179, https://doi.org/10.1002/2014jd022288, 2015.

Xue, H., Jin, Q., Yi, B., Mullendore, G. L., Zheng, X., and Jin, H.: Modulation of Soil Initial State on WRF Model Performance Over China, Journal of Geophysical Research: Atmospheres, 122, 11,278-211,300, https://doi.org/10.1002/2017JD027023, 2017.

Zhang, H., Wu, C., Chen, W., and Huang, G.: Effect of urban expansion on summer rainfall in the Pearl River Delta, South China, Journal of Hydrology, 568, 747-757, https://doi.org/10.1016/j.jhydrol.2018.11.036, 2019b.

L145-149: It is better to also report the quantities of land cover changes between 1990 and 2010.

Thanks for the comment. We have added the Table 1 with the quantities of land cover under four scenarios. The quantities of land cover changes between 1990 and 2010 can be seen in it.

Table 1. The percentages of land use and cover under four scenarios.

Scenarios	Cropland	Forest	Grassland	Water and wetland	Urban	Unused land
1990 scenario	29.15	42.82	23.50	1.65	0.19	2.69
2010 scenario	28.48	43.60	23.13	1.79	0.86	2.14
20% scenario	22.80	49.28	23.13	1.79	0.86	2.14
50% scenario	14.58	57.50	23.13	1.79	0.86	2.14

L148: How did the random changes from cropland to forest being incorporated in the model surface land condition at 15 km resolution? I am not sure whether this choice is necessary. What land variables had been modified to represent the land cover change in WRF model and what are their changes? What types of forest were used in the reforestation experiment? How many grid boxes experienced land cover change?

The land use changes were included in the WRF model by modifying the geographical static data used in the model which further changed the simulation of subprocesses such as the vegetation phenology, canopy stomatal resistance, runoff and groundwater in the land surface model Noah-MP (Li et al. 2018). Many parameters were used in Noah-MP to describe the characteristics of different land use types, such as albedo, HVT (Top of canopy), LAI (Monthly leaf area index), and VCMX25 (Maximum rate of carboxylation at 25 °C). When the land use changed, these parameters changed accordingly which finally led to the changes in substance and energy exchanges between atmosphere and land surface. In the study, we used the U.S. Geological Survey (USGS) land cover with 30s resolution (~ 1km resolution; "landuse_30s_with_lakes") in the WRF Preprocessing System (WPS). The new land use data of 1990 and 2010 derived from the Landsat TM digital images at 1km resolution, was then used to replace the USGS land cover data in the WRF simulation in YRB. Finally, we randomly changed 20% and 50% of the croplands to be forests using the 2010 scenario as a baseline to produce 20% and 50% reforestation scenarios. Both the land use data (downloaded from the WRF website and derived from the digital images) have a resolution of 1km. As the resolution of inner domain of the WRF model was set as 15km, the post-processed land cover data were resampled from 1km to 15km by the WPS (the WRF Preprocessing System). Then, the dominant land use categories in model grids were used for the Noah-MP model to correctly reflect the intended land cover changes. There were two main types of croplands, i.e., dry cropland and pasture (USGS code 2), and irrigated cropland and pasture (USGS code 3), and three main types of forest, i.e., shrubland (USGS code 8), savanna (USGS code 10) and deciduous broadleaf forest (USGS code 11). For the 20% and 50% scenarios, there were 408 and 1060 cropland grids experienced land cover changes, while the total grids of cropland in the 2010 scenarios was 2231. This has been clarified in the revised manuscript.

Reference:

Li, J., Chen, F., Zhang, G., Barlage, M., Gan, Y., Xin, Y., and Wang, C.: Impacts of Land Cover and Soil Texture Uncertainty on Land Model Simulations Over the Central Tibetan Plateau, Journal of Advances in Modeling Earth Systems, 10, 2121-2146, https://doi.org/10.1029/2018ms001377, 2018.

L170: What about statistical significance levels of these precipitation changes? This needs to be reported for this and other figures as well.

Thanks for the comment. We have conducted statistical significance tests with t-test at the 5% significance level for all spatial difference plots to determine whether the signals are robust when comparing simulation results between different experiments. Please see the revised Fig. 7 in our earlier reply to the general comments; other significance test results have been incorporated into the revised manuscript.

L224: Why did 20% and 50% reforestation grids at the model resolution are different?

For the 20% reforestation scenario, only 20% cropland grids of the 2010 scenario were changed to forest grids, while for the 50% reforestation scenario, the proportion of cropland grids changed to forest grids was 50%. Moreover, the two reforestation scenarios were independently produced using random sampling. Thus, the 20% and 50% reforestation grids are different. This have been clarified in the revised manuscript.

L241: For this section, the overall decreases in both LHF and SHF after reforestation were unexpected to me. Not sure if these changes are robust enough. Typical, ET would increase after reforestation, as described in the introduction, so how to explain this result?

Thanks for the comment. From the results of significance test in the revised Fig. 13, we found that the increases of LHF were more significant than decreases after reforestation. Moreover, we added a quantitative investigation on the changes in LHF and SHF over the whole basin and found that the multiyear average summer daily LHF increased by 2.08×10^3 and 4.82×10^3 W/m^2 for the 20% and 50% scenarios, respectively, while the multiyear average summer daily SHF decreased by 4.30×10^3 and increased by 4.25×10^3 W/m^2 for the 20% and 50% scenarios, respectively. Therefore, the ET did increases after reforestation. We have added these results in the revised manuscript.

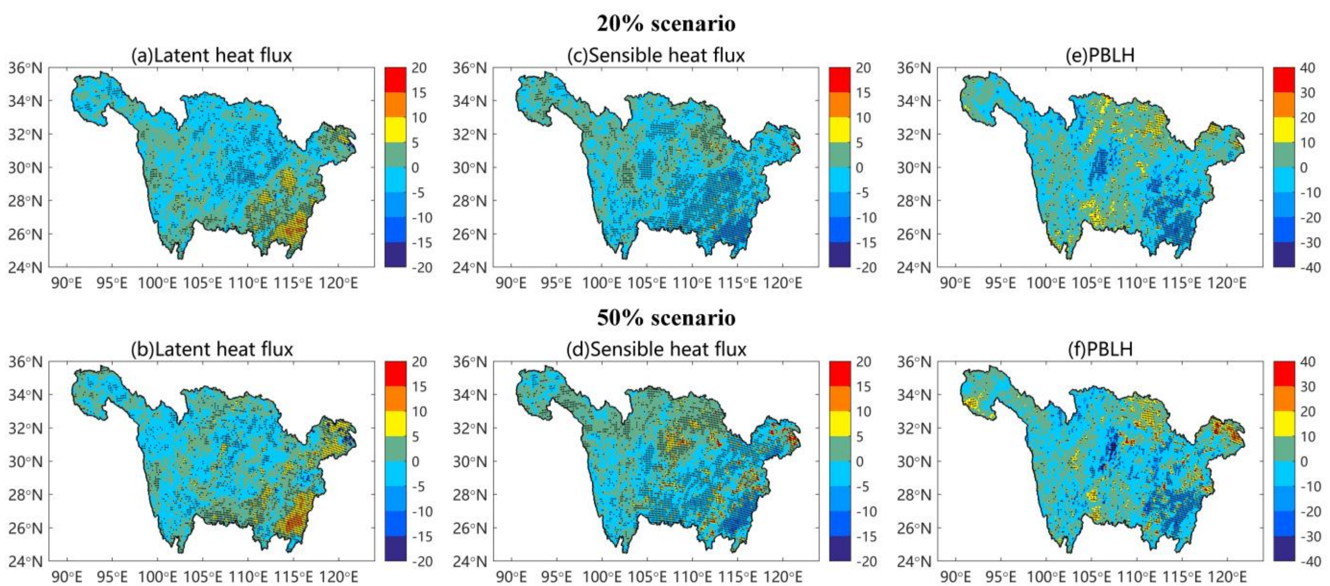

Figure 13. The changes in (a-b) latent heat flux (LHF, W/m2), (c-d) sensible heat flux (SHF, W/m2) and (e-f) PBL height (PBLH, m) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

L259: What about the changes in near-surface air temperature? For example, 2m air temperature.

Thanks for the comment. We actually analysed the changes in 2m air temperature which were not showed in the manuscript. The results were almost the same as the changes in surface skin temperature. We display the changes in 2m air temperature in Fig. A5 below. Considering that the length of the paper is too long, we show it in the appendix.

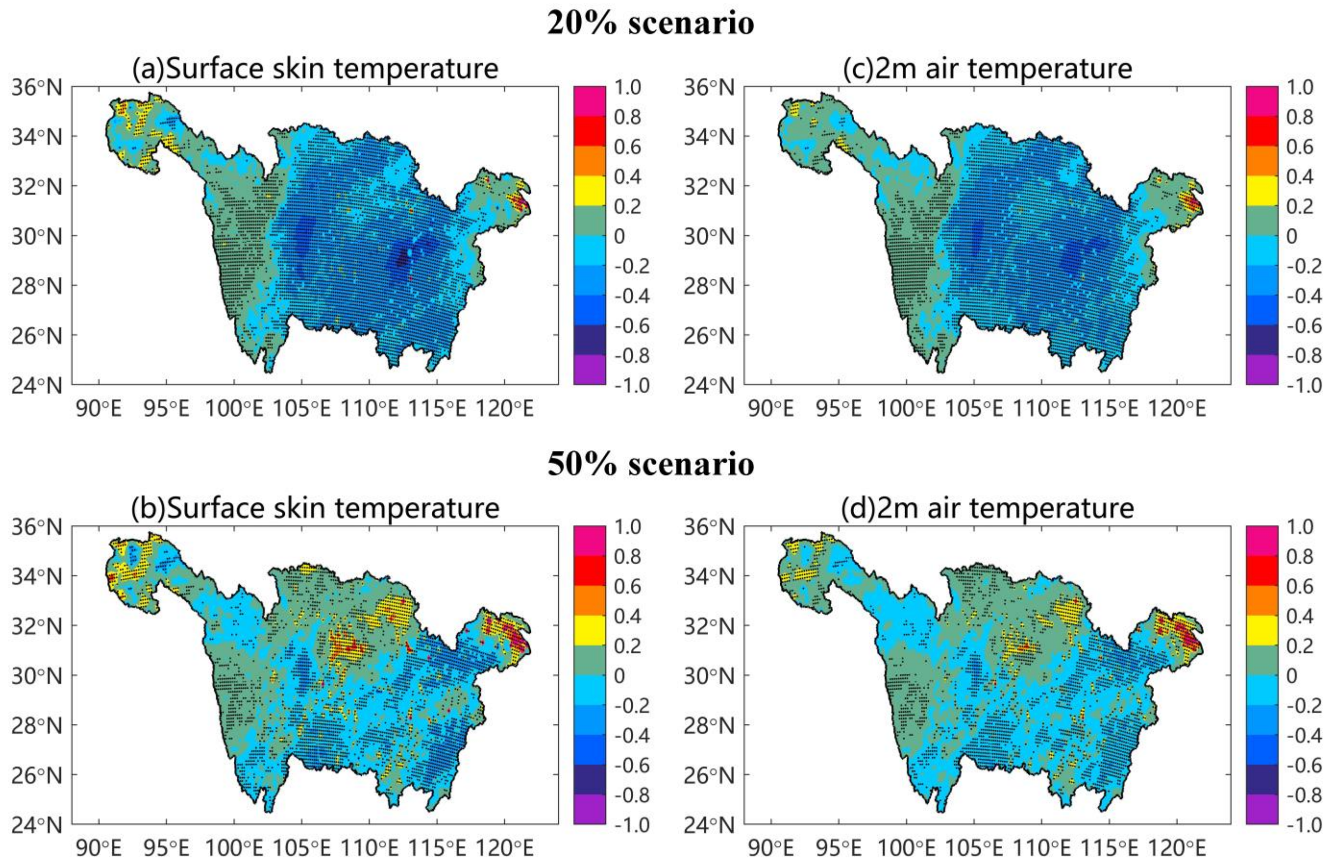

Figure A5. The changes in (a-b) surface skin temperature (℃) and (c-d) 2m air temperature (℃) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

L276-277: Any evidence to support this argument, given the latent heat flux decreased?

We have done the quantitively analysis of the changes of LHF over the whole basin and found that the latent heat flux increases after reforestation. We have revised this part of results and rewritten this argument: From the changes in the surface skin temperature and 2m relative humidity under reforestation, it can be seen that the 2m relative humidity decreases where the surface skin temperature increases. Besides, the water vapor mixing ratio in the atmosphere increases, which finally provides conditions for the increases of summer rainfall amount and extremes.

L296-297: How many urban grids had changed between 1990 and 2010? Whether urban expansion will affect the entire Yangtze river basin?

There were 32 urban grids out of the total of 7935 grids in Yangtze river basin, had been changed between 1990 and 2010. As the urban expansion mainly concentrated in the midstream and downstream of Yangtze River basin with less than 0.5% of the total area, it has a negligible impact over entire Yangtze river basin.

L332: Is there actual data to support the increased water vapor mixing?

From the model data, it can be found that the water vapor mixing increased at the 2m, especially for the 20% scenario. For the 50% scenario, areas with the significant water vapor mixing ratio increased were more than areas with significant water vapor mixing ratio decreased (Fig. 14). Please see the revised Fig. 14 in our earlier reply to the general comments.

L335: Why is the precipitation response larger in 20% than in the 50% scenario? There is no related explanation or discussion.

After analysing the changes in the water vapor mixing ratio at 2m (Fig. 14) and upward moisture flux at the surface (Fig. A6), we found that the number of grids showing increased upward moisture flux in the 50% scenario slightly exceeded that in the 20% scenario. In contrast, the 2m water vapor mixing ratio increased over almost all basin in the 20% scenario while showed large decreases in the midstream of the basin in the 50% scenarios. From the surface level to the 2m level, the moisture kept increased in the 20% scenarios while decreased in the 50% scenarios. This suggested that the distribution of moisture may be changed by the horizontal transportation process. Moreover, Yu et al. (2020) found that the vegetation greening reduced rainfall in some region in the southern China which may be caused by the East Asian monsoon, as the East Asian monsoon significantly influenced the summer precipitation patterns in China (Ding et al., 2007). All these information and explanations have been incorporated into the revised manuscript. Please see the revised Fig. 14 and Fig. A6 in our earlier reply to the general comments.

Ding, Y., Ren, G., Zhao, Z., Xu, Y., Luo, Y., Li, Q., and Zhang, J.: Detection, causes and projection of climate change over China: An overview of recent progress, Advances in Atmospheric Sciences, 24, 954-971, https://doi.org/10.1007/s00376-007-0954-4, 2007.

Yu, L., Liu, Y., Liu, T., and Yan, F.: Impact of recent vegetation greening on temperature and precipitation over China, Agricultural and Forest Meteorology, 295, https://doi.org/10.1016/j.agrformet.2020.108197, 2020.

Reply to Referee comment 3

Dear Editors and Reviewer:

We would like to thank the editor and all reviewers for their valuable suggestions and comments on the manuscript. These comments have not only improved the quality of the current manuscript, but also are beneficial to our future research in general. 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.

Anonymous Referee #3

The authors of the manuscript “Impacts of land use/cover change and reforestation on summer rainfall for the Yangtze River Basin” present work that show the effects of land use and land cover change on regional climate processes including summer rainfall. The manuscript shows the importance of better understanding these effects and has some interesting discussion points. These types of studies are difficult to do and this is a great start. However, in my opinion, the points outlined in this review need to be addressed for this work to have scientific merit.

Thanks for the positive evaluations and comments, all the comments and suggestions have been addressed and incorporated into the revised manuscript.

General comments:

1. The methods used to change land cover need to be discussed further as other reviewers have mentioned. The land surface model (Noah-MP) is complex and offers many options to better represent land surface processes. The land surface model is only mentioned once in the text. Noah-MP contains too many options that need to be carefully chosen for this to be glossed over. Additionally, Noah-MP uses only the dominant land use category when calculating surface fluxes, so at 15km an increase in forest will not matter if it doesn’t become the dominant category. This may help explain the inconsistent results between the 20% and 50% reforestation but without more information, it’s hard to say.

Thanks for the comment. The land use changes were included in the WRF model by modifying the geographical static data used in the model which further changed the simulation of subprocesses such as the vegetation phenology, canopy stomatal resistance, runoff and groundwater in the land surface model Noah-MP (Li et al. 2018). Many parameters were used in Noah-MP to describe the characteristics of different land use types, such as albedo, HVT (Top of canopy), LAI (Monthly leaf area index), and VCMX25 (Maximum rate of carboxylation at 25 °C). When the land use changed, these parameters changed accordingly which finally led to the changes in substance and energy exchanges between atmosphere and land surface. In the study, we used the U.S. Geological Survey (USGS) land cover with 30s resolution (~ 1km resolution; “landuse_30s_with_lakes”) in the WRF Preprocessing System (WPS). The new land use data of 1990 and 2010 derived from the Landsat TM digital images at 1km resolution, was then used to replace the USGS land cover data in the WRF simulation in YRB. Finally, we randomly changed 20% and 50% of the croplands to be forests using the 2010 scenario as a baseline to produce 20% and 50% reforestation scenarios. Both the land use data (downloaded from the WRF website and derived from the digital images) have a resolution of 1km. As the resolution of inner domain of WRF model was set as 15km, the post-processed land cover data was resampled from 1km to 15km by the WPS (the WRF Preprocessing System). Then, the dominant land cover categories in model grids were used for the Noah-MP model to correctly reflect the intended land cover changes. For 20% and 50% scenarios, there were 408 and 1060 cropland grids experience land cover changes, while the total grids of cropland in 2010 scenarios was 2231.

Reference:

Li, J., Chen, F., Zhang, G., Barlage, M., Gan, Y., Xin, Y., and Wang, C.: Impacts of Land Cover and Soil Texture Uncertainty on Land Model Simulations Over the Central Tibetan Plateau, Journal of Advances in Modeling Earth Systems, 10, 2121-2146, https://doi.org/10.1029/2018ms001377, 2018.

2. The limitations of using a convective parameterization when investigating rainfall extremes needs to be discussed. In a region with large vertical relief, the choice to use a course resolution for this study should be justified. Convection permitting scales (<4km) not only allow for better representation of precipitation processes, but also better land surface representation (including topography).

Thanks for the comment. We realize that convective parameterizations differ greatly in their treatments of the cloud up draughts and down draughts, mass-flux closure and triggering, often assuming that one is averaging over both cloud up draughts and the subsiding environment. As a result, all these schemes are better at predicting the area-average rainfall (Clark et al., 2016). Additionally, the cumulus parameterizations also introduce uncertainties to the model results (Liu et al., 2017). We also agree that higher model resolution can better represent precipitation processes and land surface. However, this study focused on the Yangtze River basin, which had a total area of ~1.8×10^6 km^2. Considering the huge area, it would take too much time and computing resources if running the WRF model at convection permitting scale. To find a balance between the simulation performance and consume, we think 15 km is an acceptable resolution to study the impacts of land use over such a huge basin. Moreover, in some other studies which evaluated the impacts of land use/cover changes on climate over such a big region, the resolution of model was usually similar or even coarser (e.g., Zhang et al., 2017; Zha et al., 2019; Zhang et al., 2021). We have added above clarification and the relevant information and discussion in the Discussion section of the revised manuscript.

References:

Clark, P., Roberts, N., Lean, H., Ballard, S. P., and Charlton-Perez, C.: Convection-permitting models: a step-change in rainfall forecasting, Meteorological Applications, 23, 165-181, https://doi.org/10.1002/met.1538, 2016.

Liu, C., Ikeda, K., Rasmussen, R., Barlage, M., Newman, A. J., Prein, A. F., Chen, F., Chen, L., Clark, M., Dai, A., Dudhia, J., Eidhammer, T., Gochis, D., Gutmann, E., Kurkute, S., Li, Y., Thompson, G., and Yates, D.: Continental-scale convection-permitting modeling of the current and future climate of North America, Climate Dynamics, 49, 71-95, https://doi.org/10.1007/s00382-016-3327-9, 2016.

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.

Zha, J., Zhao, D., Wu, J., and Zhang, P.: Numerical simulation of the effects of land use and cover change on the near-surface wind speed over Eastern China, Climate Dynamics, 53, 1783-1803, https://doi.org/10.1007/s00382-019-04737-w, 2019.

Zhang, X., Chen, J., and Song, S.: Divergent impacts of land use/cover change on summer precipitation in eastern China from 1980 to 2000, International Journal of Climatology, 41, 2360-2374, https://doi.org/10.1002/joc.6963, 2021.

3. The model validation is insufficient. Look to Liu et al., 2017, for an example of full model validation. To be specific, I would like to see the figures reworked to show the spatial patterns of rainfall on a seasonal and annual basis in the observations and in the control simulations. Furthermore, the figures should include a representation of percent change in rainfall. A bias of 600mm of rainfall during the summer months is a lot if the average summer rainfall is only 1000mm. This information isn't shown so it's hard to know if the bias is significant. Statistical testing should also be included where appropriate. Additionally, validation of other climatic components that contribute to rainfall (such as the vertical structure of the atmosphere, PBLH, CAPE, CIN) would aid this study. Validation of surface fluxes would also help build a better picture of how well the model can represent this region. There are several eddy-covariance towers in the eastern part of the domain and a comparison of sensible and latent heat flux to those towers would be interesting. Any change that is presented should have an accompanying discussion of validation for that component. Showing Figure 10 but compared to observations would be necessary to see if WRF can capture extreme rainfall.

Thanks for the comments. We have added the figures to show the spatial patterns of rainfall on a seasonal (Fig. 4) and annual basis (Fig. A7) for the observations and in the control simulations. The figures have included a representation of percent change and the results of statistical testing in the revised manuscript. In addition, we are aware of that the validation of other climate components and surface fluxes can be helpful. Actually, we have already looked for the data from the several eddy-covariance towers in the eastern part of the domain. However, we can only get flux data of one of these towers from the China Nation Science and Technology Infrastructure (http://www.cnern.org.cn/index.jsp) and the data are from 2003 to 2010 which is mismatch with our simulation period. So unfortunately, we don't have such observation data to validate our results. However, we have added ERA5 dataset in the revised manuscript for a further model evaluation, including the surface fluxes variables, such as sensible and latent heat fluxes and PBLH (Fig. A1). Although the absolute percent biases of these three variables between WRF simulated data and ERA5 data are large than 20% in some places of the Yangtze River basin, it does not mean that the model is not properly configured, as biases exist between observed data and ERA5 data and sometimes the biases are large (Gleixner et al., 2020; Tarek et al., 2020). For example, Al-Falahi et al. (2020) showed that the percent bias of average annual precipitation of ERA5 and ground stations was -88.97% over the Al Mahwit governorate in Yemen. Besides, we have also added the temperature evaluation based on the observed temperature data, which is the only available observation data we have besides observed precipitation in the study (Fig. 6). Furthermore, we have also shown the probability distribution functions of rainfall in 2010 scenarios compared to observations and find that WRF can capture extreme rainfall well (Fig. 5).

References:

Gleixner, S., Demissie, T., and Diro, G. T.: Did ERA5 Improve Temperature and Precipitation Reanalysis over East Africa?, Atmosphere, 11, https://doi.org/10.3390/atmos11090996, 2020.

Tarek, M., Brissette, F. P., and Arsenault, R.: Evaluation of the ERA5 reanalysis as a potential reference dataset for hydrological modelling over North America, Hydrol. Earth Syst. Sci., 24, 2527-2544, https://doi.org/10.5194/hess-24-2527-2020, 2020.

Al-Falahi, A. H., Saddique, N., Spank, U., Gebrechorkos, S. H., and Bernhofer, C.: Evaluation the Performance of Several Gridded Precipitation Products over the Highland Region of Yemen for Water Resources Management, Remote Sensing, 12, https://doi.org/10.3390/rs12182984, 2020.

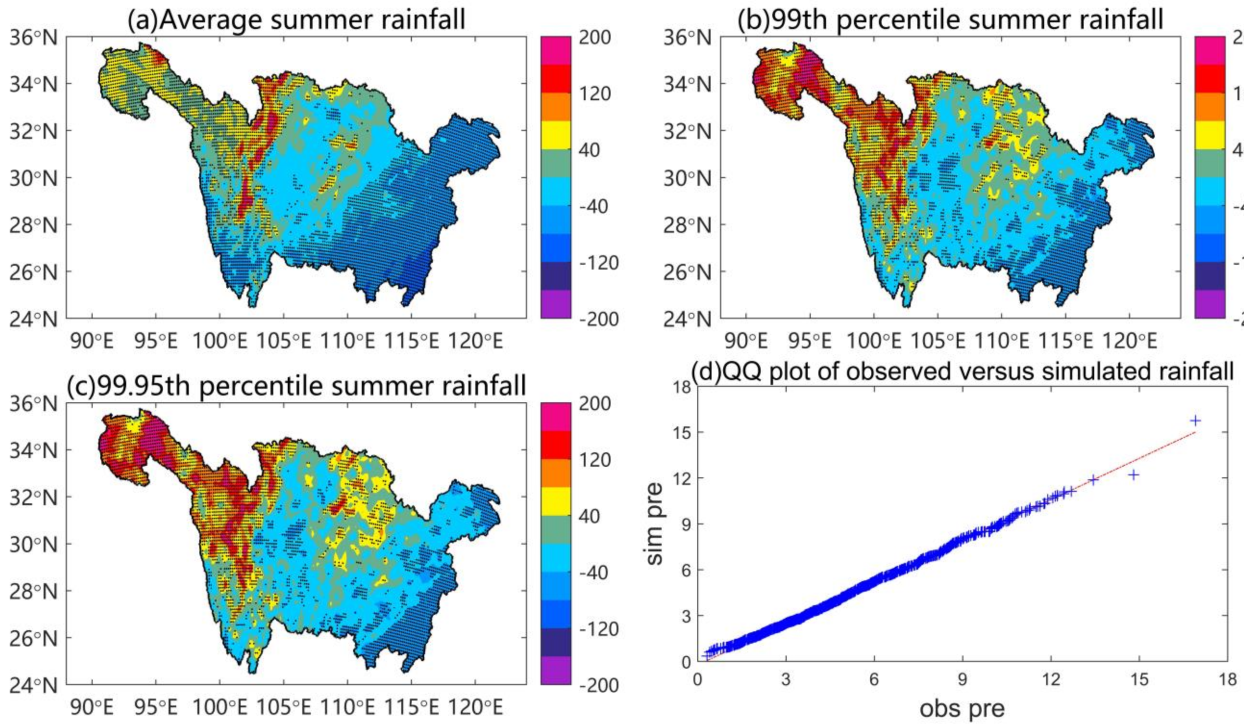

Figure 4. The bias of (a) average summer rainfall (%), (b) 99th percentile summer rainfall (%) and (c) 99.95th percentile summer rainfall (%) between the 2010 scenario and observed data, and (d) the qq-plot of observed rainfall versus simulated rainfall. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

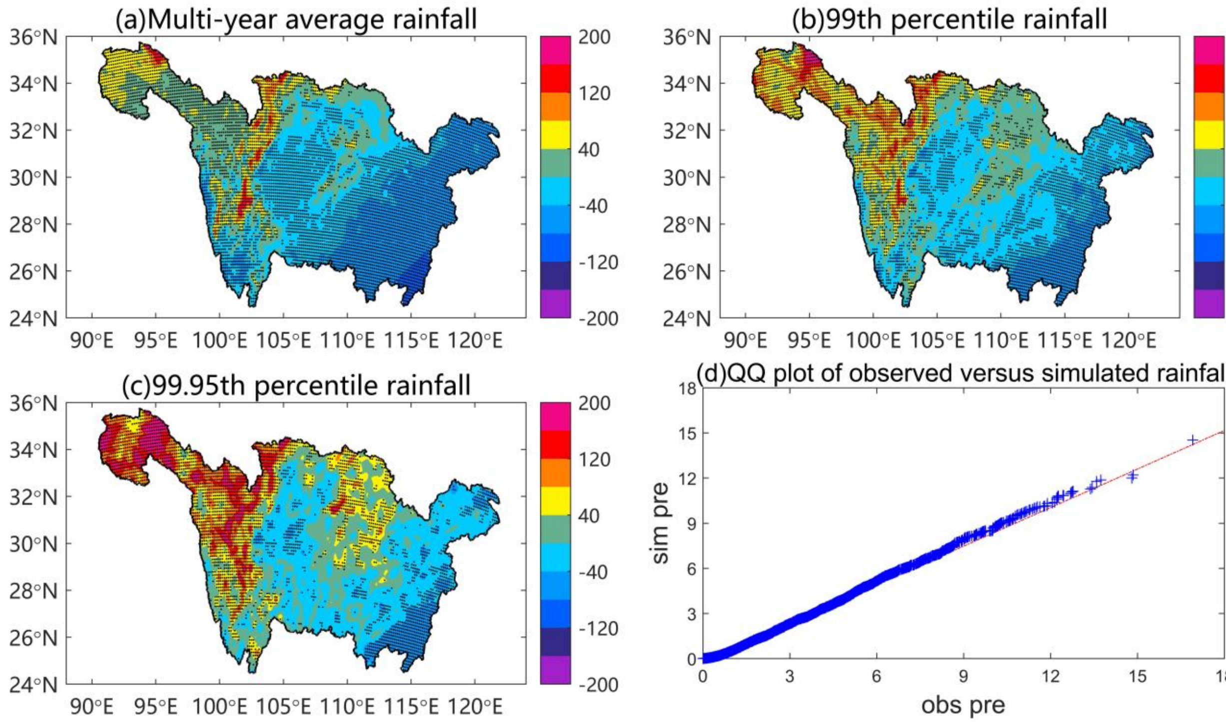

Figure A7. The bias of (a) multiyear average rainfall (%), (b) 99th percentile rainfall (%) and (c) 99.95th percentile rainfall (%) between the 2010 scenario and observed data, and (d) the qq-plot of observed rainfall versus simulated rainfall. The stippling regions show statistically significance of bias identified by t-test at a 5% significance level.

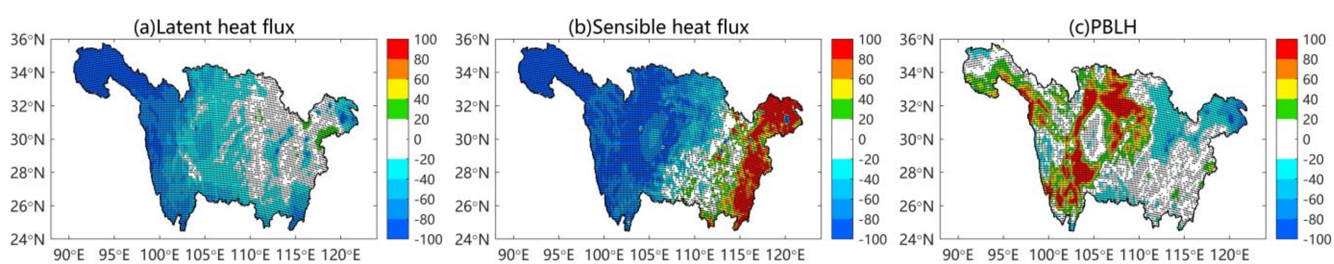

Figure A1. The biases of (a) latent heat flux (%), (b) sensible heat flux (%) and (c) PBL height (%) between the 2010 scenario and observed data. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

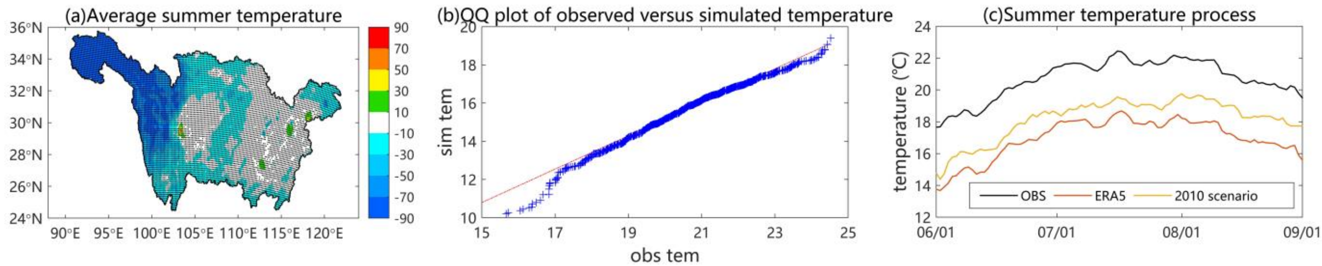

Figure 6. (a) The biases of average summer temperature (%) between the 2010 scenario and observed data, the stippling regions show statistically significance of bias identified by t-test at a 5% significance level.; (b) The qq-plot of observed temperature versus simulated temperature; (c) The basin-averaged summer temperature processes of observation, ERA5 and 2010 scenario.

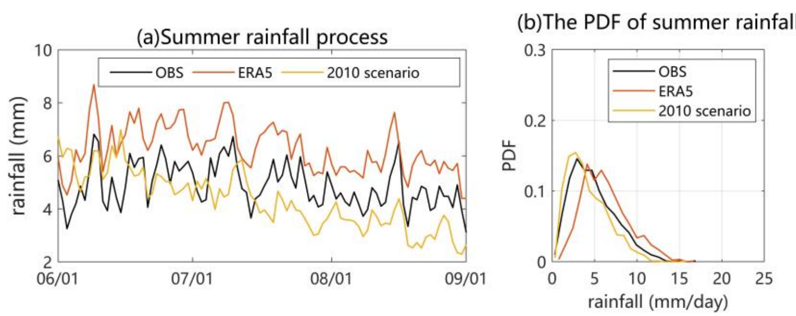

Figure 5. (a) The basin-averaged summer rainfall processes of observation, ERA5 and 2010 scenario; (b) The probability distribution functions of summer rainfall of observation, ERA5 and 2010 scenario.

4. The taylor diagrams are honestly pretty confusing, I would remove them and provide a table of biases instead. The correlation coefficients are rather low for temperature (the easiest for the model to accurately capture) and lower for rainfall when compared to observations. This leads me to believe that the model isn't configured properly for this region. If the above issues were tackled, then this opinion might change. One way to show that the model is well validated is to show that the temperature and rainfall falls within the spread of observations. Comparison to not only the station data but to an independent gridded dataset (such as ERA5, CRU, etc.) would strengthen this point.

Thanks for the comment. We have removed the taylor diagrams from the revised manuscript. The correlation coefficient was low for temperature might be because that the taylor diagrams were calculated at station basis by interpolating the gridded simulations to stations. When calculating the correlation coefficient of temperature at grid scale, the result was acceptable. We have added qq-plots of rainfall and temperature between simulated and observed data (Fig. 4 and Fig. 6), and find that the distribution of temperature and rainfall simulated by model are linear correlated to those of observation. We have also compared the simulated data with both station data and ERA5 dataset (Fig. 5 and Fig. 6). From the results, we find that the PDF of WRF-simulated rainfall is more similar to that of observation than that of ERA5, and the summer temperature simulated by WRF always falls within the spread between observation and ERA5 data. Please see the revised Fig. 4, Fig. 5 and Fig. 6 in our earlier reply to the General comment 3.

5. The percentiles of rainfall need to be defined better. What does 99th percentile mean in this case? Is it the 99th percentile of rainfall events over the 11 years? Without sub-daily rainfall, I'm not sure that this qualifies as extreme per se. A common extreme rainfall metric is the 99th percentile of daily maximum rainfall (requires sub daily rainfall to properly calculate). In my country, the storms that produce flash flooding often last only a few hours, vs a monsoon type rain that produces flooding from many, many hours of low intensity rainfall. More discussion of rainfall in this region would put this information into context. I would remove the figures that show changes to median rainfall and instead discuss some other metric of interest.

Thanks for the comment. The 99th percentile is the multiyear average value from the 99th percentile rainfall in each year. We do not use sub-daily rainfall because the flash flooding in Yangtze River basin is often caused by continuous rainfall lasts for a few days, as it is a big basin. A few hours of high intensity rainfall do not cause severe flooding due to the construction of cascade reservoirs along the river. Moreover, we have replaced the results of median rainfall with 99.95th rainfall in order to give a more comprehensive assess on the different levels of extreme rainfall. The analysis and relevant results have been added in the revised manuscript.

6. All the figures showing change between simulations need to have statistical testing. The figures all look very noisy and some of the changes to precipitation could be because the storms moved, not because more rain fell.

We agree with this comment. We have conducted statistical tests and modified all the figures showing change between simulations to present the results of statistical tests. We display the revised Fig. 9 as follows; results of other significance test have been incorporated into the revised manuscript.

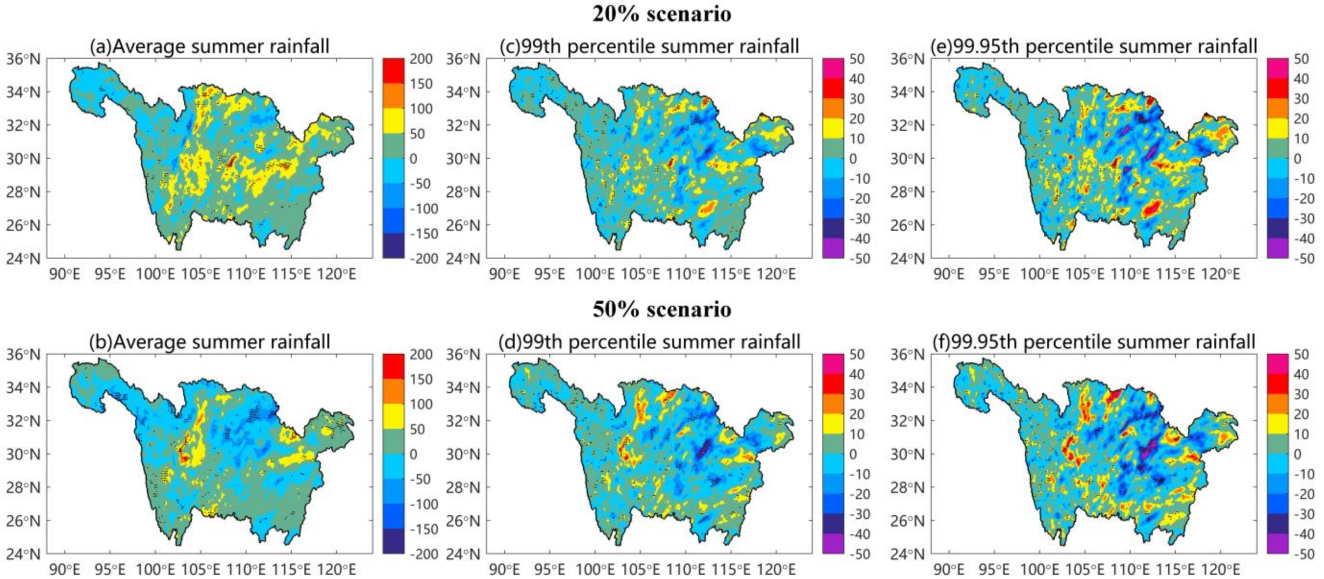

Figure 9. The changes in (a-b) average summer rainfall (mm), (c-d) 99th percentile summer rainfall (mm/day) and (e-f) 99.95th percentile summer rainfall (mm/day) between the 20% scenario and 2010 scenario, and between the 50% scenario and 2010 scenario. The stippling regions show statistically significance of changes identified by t-test at a 5% significance level.

7. Instead of bar graphs, boxplots or violin plots should be shown. This will capture the distribution of the change.

Thanks for the comment. We have changed Fig. 10 and Fig. 11 to boxplots to show the distribution of the changes in the revised manuscript.

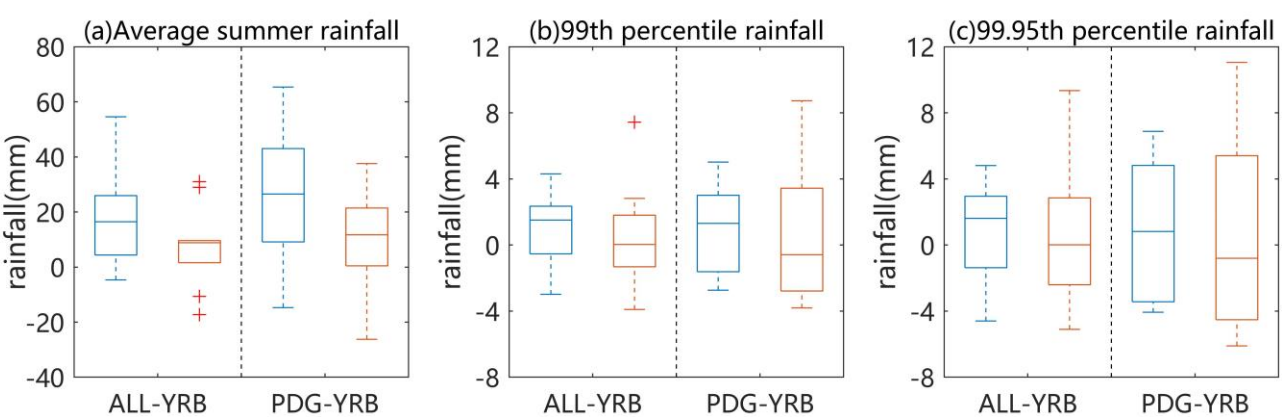

Figure 10. The changes in (a) average summer rainfall (mm), (b) 99th percentile summer rainfall (mm/day) and (c) 99.95th percentile summer rainfall (mm/day) between the two hypothesis scenarios (20% and 50% scenarios) and 2010 scenario in ALL-YRB and PDG-YRB area. The blue boxes represent the 20% scenario, while the red boxes represent the 50% scenario.

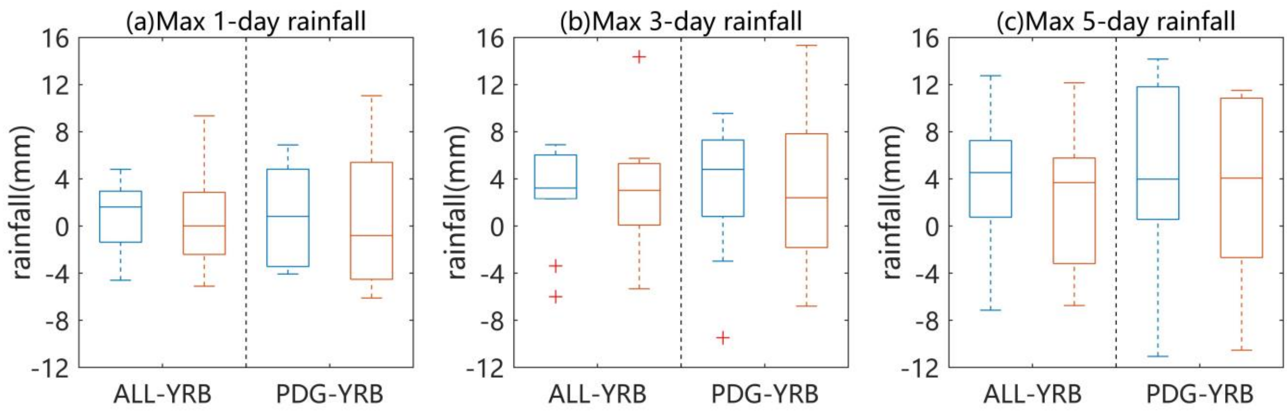

Figure 11. The changes in maximum 1-, 3-, 5-day rainfall between the two hypothesis scenarios (20% and 50% scenarios) and 2010 scenario in ALL-YRB and PDG-YRB area. The blue boxes represent the 20% scenario, while the red boxes represent the 50% scenario.

Minor specific comments:

The convention I have seen for abbreviating land use and land cover change is LULCC not LUCC.

Thanks for the comment. We have replaced all the “LUCC” with “LULCC” in the revised manuscript.

There are some English language errors in the text, but these don’t bother me that much and have been covered by other reviewers.

We have carefully checked the whole paper to improve the quality.